CHAPTER VII: STRANGE ATTRACTORS

I am an old man now, and when I die and go to Heaven there are two matters on which I hope for enlightenment. One is quantum electrodynamics, and the other is the turbulent motion of fluids. And about the former I am really rather optimistic.
—Sir Horace Lamb.[1]

Si, jetant une pierre dans une mare, vous désirez savoir ce qui se passe, il vaut infiniment mieux faire l'expérience et la filmer, que d'essayer d'en faire la théorie; les meilleurs spécialistes de l'équation de Navier-Stokes seraient incapables de vous en dire plus.
—René Thom.[2]

We cannot hope that the old ape in us, clever as he may be, has direct comprehension of abstract physical or mathematical questions.
—David Ruelle.[3]

1. INTRODUCTION: A NEW ALTERNATIVE FOR THE MODELING PRACTICE OF PHYSICS

For centuries, physicists aimed at unveiling laws of nature. Marching into the steps of Sir Isaac Newton, they exploited the second law (*F=ma*) with great success. Just as

[1] At a meeting of the British Association for the Advancement of Science in London in 1932, as recalled by S. Goldstein, "Fluid Mechanics in the First Half of This Century," *Annual Review of Fluid Mechanics,* 1 (1969): 1-28, 23.

[2] "If you want to know what happens if you throw a stone into a pond, it is infinitely better to do the experiment and film it than to try to formulate a theory about it: the finest specialists in the Navier-Stokes equations would certainly be incapable of telling you more about it." R. Thom, "Une théorie dynamique de la morphogénèse," *Towards a Theoretical Biology, I: Prologomena,* ed. C. H. Waddington (Edinburgh: University of Edinburgh Press, 1968): 152-166, 154; repr. *MMM,* 13-38, 15.

Newton had uncovered the dynamical equations governing the motion of planets in heavens, physicists in the first half of the nineteenth century were able to derive from first principles mathematical relations for fluid flow. Although, except for a few simple (and simplistic) cases, it was rarely possible to exhibit exact solutions to the Navier-Stokes equations, as they came to be known, this derivation had become an inescapable part of the classical physics curriculum. The Navier-Stokes equations provided, it was believed, the foundation for any theoretical understanding of hydrodynamic phenomena. It was therefore a shock when, in 1971, two outsiders, a physicist specializing in statistical mechanics and a mathematician who studied dynamical systems, published a controversial article "On the Nature of Turbulence" claiming nothing less than a new "mechanism for the generation of turbulence," especially since the authors, as opposed to the current practice, never explicitly wrote down the Navier-Stokes equations.[4]

This chapter aims at providing an account of the changes in physical modeling which made it possible that a new model of the onset of turbulence could be proposed without its authors ever feeling the necessity of mentioning the law found a century and a half earlier by Claude Louis Navier and Sir George G. Stokes. Inspired by René Thom's ideas, conceived and written at the Institut des hautes études scientifiques in the spring of 1970 by the French physicist David Ruelle and the Dutch mathematician Floris Takens, this article is remarkable for several reasons reaching beyond its

[3] D. Ruelle, "The Obsession of Time," *Communications in Mathematical Physics,* 85 (1982): 3-5, 5.

introduction of the famous notion of *strange attractors,* which was to have a very bright future. Above all, Ruelle and Takens's article supplies both a *symptom* and a *direct cause* for crucial changes that have been widely affecting the modeling practice of theoretical physics ever since.

Based on first principles coming from either molecular hypotheses or continuum mechanics, the partial differential equations of physics acquired, in the course of the nineteenth century, an almost ontological status.[5] A telling and much studied instance of this process, which can be seen as originating in Fourier's analysis of heat flows, is provided by the rise of the notion of a field, which ultimately subsumed the ether under an abstract set of differential equations written down by Maxwell and his followers.[6] For Maxwell and Boussinesq, the complex diversity of behaviors exhibited by solutions to partial differential equations reinforced ontological commitments to them.[7]

---

[4] D. Ruelle and F. Takens, "On the Nature of Turbulence," *Communications in Mathematical Physics,* 20 (1971): 167-192; 23: 343-344; repr. *Chaos II,* 120-147; *TSAC,* 57-84. Quote from the abstract on p. 167.

[5] For an analysis of the dual basis for the derivation of mathematical laws based on two sets of hypotheses, molecular or continuous, see Amy Dahan Dalmedico, *Mathématisations. Augustin-Louis Cauchy et l'école française* (Paris: Albert Blanchard; Argenteuil: Éditions du Choix, 1992).

[6] See e.g. J. Z. Buchwald, *From Maxwell to Microphysics: Aspects of Ectromagnetic Theory in the Last Quarter of the Nineteenth Century* (Chicago: University of Chicago Press, 1985); B. J. Hunt, *The Maxwellians* (Ithaca: Cornell University Press, 1991). See also J. Fourier, *The Analytic Theory of Heat,* Transl. A. Freeman (Cambridge: Cambridge University Press, 1878).

[7] J. C. Maxwell, "Does the Progress of Physical Science Tend to Give any Advantage to the Opinion of Necessity (or Determinism) over that of the Contingency of Events and the Freedom of Will?" repr. *The Scientific Letters and Papers of James Clerk Maxwell,* 2, ed. P. M. Harman (Cambridge: Cambridge University Press, 1995): 814-823; V. J. Boussinesq, "Conciliation du véritable déterminisme mécanique avec l'existence de la vie et de la liberté morale" (1878); repr. *Cours de physique*

New modeling practices emerged in the late nineteenth-century with the development of statistical mechanics under Maxwell and Boltzmann's lead, which offered the hope of basing the understanding of fundamental laws of physics on the molecular hypothesis again. Quantum mechanics and the investigation of inter-molecular forces provided a further pull in this direction. Later, mainly after World War II, statistical approaches were further developed with much success. But the question of the relation between microscopic, molecular theories and macroscopic, continuous differential equations always spurred passionate debates. As far as macroscopic physics was concerned, the exploitation of fundamental laws, derived from general principles and expressed by differential equations, only partially justified by statistical and quantum mechanical considerations, remained the physicists' dominant foundation for their modeling practice.

In this context, the turbulence problem for fluid mechanics was a distressing one. If, in traditional histories of physics, the discovery of an equation has often been the culminating point, we might contend that the history of turbulence started with the equation. Indeed, only when this equation existed did turbulence become a theoretical problem. On the one hand, there was every reason to believe that the Navier-Stokes equations provided a faithful description of classical fluid flows. On the other hand, it was an experimental fact that extremely complex flows arose when the fluid was

mathématique de la Faculté des sciences, Compléments au Tome III. Paris: Gauthier-Villars, 1922; and H. Poincaré, *cience et méthode* (Paris: Flammarion, 1908). About this, see I. Hacking, "Nineteenth-Century Cracks in the Concept of Determinism," *Journal of the History of Ideas,* 44 (1983): 455-475; and M. A. B. Deakin, "Nineteenth-Century Anticipations of the Modern Theory of Dynamical Systems," *Archive for History of Exact Science,* 39 (1988): 183-194.

submitted to intense external stress; this complexity was called turbulence. The turbulence problem lay in the relation between fundamental equations and their solutions. In his own flowery way, French hydrodynamicist Joseph Kampé de Fériet used the common metaphor of an inaccessible mountain peak to express the problem:

> On one side, on a peak covered with perpetual snow, there waves in the wind, in loneliness and silence, the flag of the Navier[-Stokes] equations; an unfathomable abyss divides this icy summit from the ground on which is pouring the incessant rain of experimental results. It is on this ground, sometimes slightly boggy because of the abundance of rain, that mathematical models are tentatively elaborated; . . . but is it not premature to brand these models as theories of turbulence?[8]

To bridge the chasm dividing the Navier-Stokes equations from feasible experiments or known solutions, was the "turbulence problem."[9] The above quote also exhibits an interesting distinction between theories and models of turbulence, a distinction that the study of Ruelle and Takens's article and its reception among fluid dynamicists will underscore.

In their heart, physicists had always recognized that fundamental laws were but the beginning of a theoretical solution to any problem. But for a long time, when unable to solve the equations explicitly, physicists had few mathematical tools which still could have enabled them to account for natural phenomena in a satisfactory

---

[8] J. Kampé de Fériet, in *Mécanique de la turbulence. Colloque international du CNRS de Marseilles, 28 August - 2 September, 1961,* ed. A. Favre (Paris: Éditions du CNRS, 1962). See also Philippe Delache's 1977 cartoon reproduced in U. Frisch, *Turbulence: The Legacy of A. N. Kolmogorov* (Cambridge: Cambridge University Press, 1995), 253.

[9] F. Noether, "Das Turbulenzproblem," *Zeitschrift für andgewandte Mathematik und Mechanik,* 1 (1921): 125-138.

manner.[10] Historically, Ruelle and Takens's article signaled the reencounter of physics with qualitative mathematics. It would help to initiate a powerful alternative to the endless quest for the final law of nature. Instead, more and more physicists started to look anew into mundane phenomena, without relying too heavily on fundamental laws. These laws, they began to think, might be unreachable with certainty, but they hoped nonetheless to provide deep theoretical explanations for experimental data.

This chapter examines the conditions that enabled Takens and especially Ruelle to attack the turbulence problem with some success and to come up with new modeling practices. Their argument is summarized and contrasted with other alternative pictures for the onset of turbulence. Their special situations at the IHÉS shaped the way they saw the possibility of adapting mathematical techniques of dynamical systems theory to the study of turbulence. The earlier career of Ruelle in statistical physics is then seen as providing the ground on which this new modeling practice could grow.

In a second part, this chapter tries to place the Ruelle-Takens model within a long-term survey of the history of turbulence. This part is meant to underscore the changes in modeling practice that this model afforded. The history of the relationship between the fundamental equations of fluid mechanics and turbulence is briefly reviewed. It is then explained how a certain subdiscipline of fluid mechanics, called

[10] A important research path that, unfortunately, I am *not* pursuing here could be called the *big science* of fluid flows. With large computers and wind tunnels, the modeling of fluid flows in concrete situation was very different in practice than anything before.

hydrodynamic stability theory, was best suited to accommodate Ruelle and Takens's approach.

Briefly, Ruelle's new alternative for physicists' modeling practice displaced the emphasis often put on specific models or fundamental laws of nature, in order directly to tackle classes of models. "Contemporary rational thinking goes through successive ontologies in name of the epistemic reality," Simon Diner once wrote.[11] Without resolving the conundrum of the nature of the relationship existing between fundamental laws and observation, this new practice made models cheap and dispensable, and rather focused on some essential topological features of observed behaviors which were assimilated to the structural, yet dynamical, characteristics of classes of models. In short, some physicists *stopped looking at specific representations of nature in order to study the consequences of the mode of representation itself.*

2. THE NATURE OF TURBULENCE: THREE ALTERNATIVES

The article published in 1971 by David Ruelle and Floris Takens "investigate[d] the *nature* of the solutions of [the Navier-Stokes equations], making only assumptions of a very general nature on [the equations]."[12] It provides an excellent probe for examining this significant shift in the modeling practice of some physicists. They introduced a method characteristic of an attitude, inspired by René Thom in particular, that would become widespread. For Ruelle and Takens, it was not so much the

[11] S. Diner, "A Renewal of Mechanism: Toward an Instrumental Realism," *Dynamical Systems: A Renewal of Mechanism,* ed. S. Diner et al. (Singapore: World Scientific, 1986): 273-284, 281.

detailed structure of the Navier-Stokes equation that mattered, but the very fact that fluids could be described, with an amazing degree of precision, by dissipative differential equations. From this fairly general starting point, and several other technical assumptions which they did not even care to derive from the fundamental equation, Ruelle and Takens were able to redefine the nature of turbulence and "give some insight into its meaning, *without knowing [the Navier-Stokes equations] in detail.*"[13] Quite decisively, they also made *qualitative* predictions that could be tested *in vitro* or *in silico,* that is, by numerical simulations of fluid flows.

Originally rejected by a referee of the *Archive for Rational Mechanics and Analysis,* Ruelle and Takens's article had to be published in a journal of which Ruelle himself was an editor.[14] At first, according to Ruelle, the response from the physics community to these "controversial ideas on turbulence" was slow to come and cold.[15] This is not difficult to understand considering the extreme technicality of the mathematics involved in the article from the point of view of physicists, and the heresy against Landau's widely accepted model. Besides, for those who penetrated the mathematical technicalities, the crucial reliance of their argument on the concept of *generic solutions* was rather hand-wavy. As described in Chapter V, this matter was

---

[12] D. Ruelle and F. Takens, "On the Nature," 168. My emphasis.
[13] D. Ruelle, "Méthodes d'analyse globale en hydrodynamique," *TSAC:* 1-56, 7.
[14] Ruelle, *Chance and chaos,* 56, 63. Interview of David Ruelle by the author (7 February 1997).
[15] Only 7 citations in the *Science Citation Index,* other than from the authors themselves, for the years 1971-1974. See Graph 8 and D. Ruelle, "Turbulent Dynamical Systems," *Proceedings of the International Congress of Mathematicians* (Warsaw, August 1983), 275; *Chance and chaos,* 66.

far from being settled in 1970. The following shows that Ruelle and Takens's use of this concept very much relied on the practices they observed around René Thom.

a) The Argument of Ruelle and Takens's Paper

In 1971, David Ruelle and Floris Takens suggested, but did not show rigorously, that when a fluid was subjected to increasing external stress, it went through a succession of bifurcations, where different modes of vibration—i.e. different frequencies—appeared.[16] So far, this merely was a rephrasing of the model proposed by Lev Landau in 1944 and, independently, by Eberhard Hopf in 1942-1948. But Ruelle and Takens went on to suggest, albeit once again without providing a rigorous demonstration, that this bifurcation sequence had to stop after the manifestation of three different modes, because a "strange attractor" appeared in a "generic" manner, and the fluid motion ceased to be quasiperiodic.[17] Strictly *aperiodic motion* was the new definition they proposed for turbulence.

One striking feature of Ruelle and Takens's article was that they did not feel the need to write down the Navier-Stokes equations explicitly. As we have seen previously, as early as the fall of 1968 Ruelle had hoped that his approach could bear on any problem concerning dissipative systems, and not only fluid motions. Their

---

[16] Many notions, including the Navier-Stokes equations (see p. 509), from fluid mechanics and dynamical systems theory that are used here but may not be already clear to the reader are later introduced in the course of this chapter. They are however not needed at present.

[17] In 1978, the Ruelle-Takens scenario was deemed to arise after the appearance of only two modes; S. Newhouse, D. Ruelle, and F. Takens, "Occurrence of Strange Axiom *A* Attractors Near Quasi-Periodic Flows on T^m, $m \geq 3$," *Communications in Mathematical Physics,* 64 (1978): 35-40; repr. *TSAC,* 85-90.

omission of the Navier-Stokes equations probably was intended to underscore the fact that their argument remained independent of its precise form.

> The time evolution of a velocity field [for a fluid] is given by the Navier-Stokes equations
> $$\frac{dv}{dt} = X_{\mu}(v)$$
> where X_{μ} is a vector field over H [the space of velocity fields $v(x,t)$]. For our present purposes it is *not* necessary to specify further H or X_{μ}.[18]

The parameter μ represented the external stress applied on the fluid. In the case of motion through a pipe, or past an object, a unique parameter depending on the physical characteristics of the flow—the so-called Reynolds number *Re*—was the only one determining the global behavior of the motion, i.e. whether or not it was turbulent. For other situations, like convection when a fluid is heated from below, the Raleigh number *Ra* played a similar role. As is explained below, the determination of critical values of these parameters, at which the properties of the motion changed, had been, for almost a century, the subject of numerous theoretical and empirical studies.

But Ruelle and Takens were not interested in particular critical values of the parameter μ, only the general features of motion as this parameter increased. When $\mu=0$, there was no external stress; but because of friction, the fluid would always come to rest as time went to infinity. Transitory motion was not their concern. The parameter μ represented external forces, so that when it was nonzero, the fluid could, despite friction, be maintain in a state of perpetual motion. For small μ, the motion

[18] D. Ruelle and F. Takens, "On the Nature," 168. My emphasis. Compare with the Navier-Stokes equations on p. 509.

tended towards a *stationary motion,* that is, the velocity field remained constant for all time: $v(t;\mu) \equiv v_0(\mu)$.

At a certain critical value μ_1, the flow went though a bifurcation, which Ruelle and Takens identified as a the *Hopf bifurcation.* This meant that the velocity field ceased to be independent of time, but started to oscillate at a given frequency ω_1. The flow was periodic. In phase space, this was expressed by saying that, while for stationary flows a fixed point existed which was an *attractor* for the system, when the oscillatory mode appeared, this point 'exploded' into a closed curve.

At a further critical value of the parameter μ, a second bifurcation occurred which gave rise to a second frequency ω_2 of oscillation, and so on. This quasiperiodic behavior was the picture conjectured by Landau and Hopf. When the value of the parameter μ increased sufficiently, a situation arose in which "the fluid motion becomes very complicated, irregular and chaotic, we have turbulence."[19] But how could one describe this "chaotic" flow?

As the title of Ruelle and Takens's paper indicated, what was at stake was the very nature of turbulence. For Hopf, and especially Landau, the quasiperiodic flow that resulted from the appearance of several oscillatory modes was turbulent. Ruelle and Takens claimed that the quasiperiodic case was not *generic* for general dissipative dynamical systems. From this, they concluded that it had no chance of being observed and that one had to look elsewhere for a "mathematical explanation of turbulence."[20]

[19] D. Ruelle and F. Takens, "On the Nature," 168.
[20] D. Ruelle, "Strange Attractors as a Mathematical Explanation of Turbulence," *Statistical Models and Turbulence: Proceedings of the Symposium at the University of*

Ruelle and Takens's model for the onset of turbulence was dependent on their exploitation of three main sources. First, their reliance on concepts stemming from the qualitative study of dynamical systems made plain how much their explanation of turbulence was dependent on the work of Thom, Smale, and his students. Second, the emphasis with which they studied the Hopf bifurcation, and the techniques they used to do so, were novel. As will become apparent, Hopf's pair of articles had by and large fallen into oblivion. Ruelle and Takens's article was crucial in drawing attention back to them and linking them with Landau's scheme for turbulence. The third important strain lay in Ruelle's earlier work. In the following, these three strains will be examined. Like for most of Chapter V above, there will be no pretense of presenting an original history. Rather, this will convey, with an infusion of relevant contextual elements, a re-reading of the sources that were important for Ruelle and Takens when in 1970 they wrote their famous article.

b) The Quasiperiodic Model for the Onset of Turbulence

(i) Physics à la Landau

As was said in Chapter VI, David Ruelle's interest in fluid dynamics surely went as far back as 1968. And his inroad into the field was provided by the famous textbook *Fluid Mechanics* by Landau and Lifshitz. A Russian specialist in fluid mechanics who wrote one of its only existing modern histories, G. Tokaty expressed his strong admiration for this book as such:

California, La Jolla, 1971, ed. M. Rosenblatt and C. van Atta, Lecture Notes in Physics, 12 (Berlin: Springer, 1972): 292-299.

> in my personal opinion and experience, among the more recent contributions to fluidmechanics [*sic*], . . . by far the most outstanding in essence and beautiful in mathematical form was the great book *Mekhanika Sploshnykh Sred* [The Mechanics of Continuous Media] by Landau and Lifshitz, Moscow, 1954; it is difficult to imagine a professional pleasure superior to that experienced while reading this book.[21]

Ruelle did not seem to have enjoyed reading Landau and Lifshitz's book as much as Tokaty, but he did find a gem in it. "I worked my way slowly through the complicated calculations that these authors seem to relish," he recalled, "and suddenly fell on something interesting: a section on the onset of turbulence, without complicated calculations."[22] In retrospect, it seems easy to notice that Landau's suddenly verbose prose was indicative of a conjecture. But he had nevertheless deemed this conjecture important enough to publish it in a separate note, included in the 1944 volume of the Proceedings of the Soviet Academy of Sciences.[23]

Little is known about the conditions in which Lev Landau came up with his model for the onset of turbulence. In most biographical writing about him, the World War II years do not seem to be well documented. It is only mentioned that his Moscow Institute was evacuated to Kazan where he worked on defense-related problems, and in particular the detonation of explosives.[24] It would be interesting to

---

[21] G. A. Tokaty, *A History and Philosophy of Fluidmechanics* (Henley on Thames: G. T. Foulis, 1971), 223.
[22] D. Ruelle, *Chance and Chaos,* 53.
[23] L. D. Landau, "On the Problem of Turbulence," *Doklady Akademi nauk SSSR (C. R. de l'Académie des sciences de l'URSS),* 44 (1944): 311-314; repr. *Chaos II,* 115-119; and L. D. Landau and Evgueni Lifshitz, *Fluid Mechanics,* chap. 3, (Oxford: Pergamond, 1959); *Mécanique des fluides* (Moscow: Mir, 1971); first Russian ed. 1954.
[24] See, e.g., A. Livanova, *Lev Landau,* transl. I. Sokolov (Moscow: Mir [1978], 1981), 39; A. I. Akhiezer, "Recollections of Lev Davidovich Landau," Physics Today, 47 (June 1994): 35-42; and G. Gorelik, "Lev Landau, Prosocialist Prisoner of the Soviet

know whether his theory of turbulence stemmed from consideration of this kind of applications. And although he seems to have expressed contempt for mathematical physics—he called it "mathematical lyricism"[25]—it would also be important to know the extent to which he was in contact with the Soviet mathematicians already discussed, namely Andronov, Kolmogorov and their followers.

A most important feature to notice about Landau's article was his reliance on arguments of a very general nature, which already made them potentially applicable to situations much different from the turbulence problem. No doubt Lev Davidovich Landau (1908-1968) impressed his mark on many, if not all, portions of theoretical physics in the twentieth century. Landau's contribution to the turbulence problem may not have been as spectacular as in other fields, but it provided a concise, intuitive picture of the mathematical mechanism responsible for turbulence. In addition to his theoretical work, Landau built a successful school of theoretical physics and trained a whole generation of students, for whom he wrote, with Evgueni Lifshitz, the famous *Course in Theoretical Physics,* of which the book on fluid mechanics was one volume. This course remains one of the most comprehensive syntheses of this field in the twentieth century. "I am the last universal physicist," Landau was quoted as saying after Enrico Fermi's death in 1954, and many indeed agreed with him.[26]

The paper published in 1944, can therefore be seen as a manifestation of Landau's universality. Indeed, as opposed to most previous works on the onset of

---

State," Physics Today, 48 (May 1995): 11-15; Karl Hall, "Moral Economy of Soviet Physics Circa 1937," HSS Meeting in Atlanta (November 11, 1996).

[25] A. Livanova, *Lev Landau,* 69.

[26] A. Livanova, *Lev Landau,* 11-12.

turbulence in fluids, Landau's explanation of this phenomenon relied on general arguments and applied to every situation where turbulence arose in fluids. It was an actualization of a practice Ruelle called "Physics à la Landau."[27] It consisted in modifying the linearized theory by including the first nonlinear terms in the perturbation expansion, and sometimes, as in the case of turbulence, exhibiting new and interesting qualitative features.

In his 1944 paper, which was included almost untouched in his book with Lifshitz, Landau investigated the behavior of the solutions of the Navier-Stokes equations for Reynolds numbers slightly above critical values. Using old arguments about the stability of solutions, he contended that an oscillatory perturbation to the stationary solution was bound to arise, past a critical value of *Re*. In fact, he had to acknowledge that there was no theoretical foundation for this phenomenon. Only experimental data indicated that it was so.[28] In any case, Landau's exploitation of perturbation techniques allowed him to give an intuitive, but mathematically-informed justification for the fact that the motion ceased to be stationary for Reynolds numbers larger than a critical value. Moreover, this justification was easily transposable to any other case where such an oscillatory behavior could be obtained from a stationary solution.

Boldly, Landau guessed, with no computation to back him up, that this process would repeat *ad infinitum,* with new modes constantly appearing as the Reynolds number went up.

---

[27] D. Ruelle, "Idéalisation en physique," in *Logos et théorie des catastrophes,* ed. J. Petitot (Geneva: Patiño, 1988): 99-104, 103.

> In the course of the further increase of the Reynolds number, new and new periods appear in succession, and the motion assumes an involved character typical of developed turbulence. For every value of *Re* the motion has a definite number of degrees of freedom; in the limit, as *Re* tends to infinity, the number of degrees of freedom becomes likewise infinitely large.[29]

In the 1954 book version of the argument, Landau again avoided deducing the appearance of the second frequency, much less the following ones, from the equations of motion. He wrote that this study should be attempted along the same line as for the first oscillatory mode. In a revealing footnote, he added: "But [it] has not been [done] in any case, due to exceptional mathematical difficulties."[30] As the process was repeated over and over again, new frequencies were supposed to appear in such a way that physical parameters of the fluid flow were given by an expression of the form:

$$x(t)= f(\omega_1 t,\ldots,\omega_k t)$$

where the frequencies $\omega_1,\ldots,\omega_k$ were not rationally related. Then Landau defined turbulence as being this complicated quasiperiodic behavior. "In this way, for $Re>Re_{\text{crit}}$ the motion quickly acquires a complex, inextricable character. Such a motion is said [to be] *turbulent.*"[31]

During World War II, as is well known, another Soviet scientist made a fundamental contribution to the study of turbulence which shaped much of the work of the succeeding decades. In 1941, Andrei Kolmogorov published his famous article on the statistical theory of developed turbulence. As it was concerned with the global features of fluid flows at very high Reynolds numbers (so-called *developed*

[28] L. D. Landau and E. Lifshitz, *Mécanique des fluides*, 127.
[29] L. D. Landau, "On the Problem of Turbulence," 314.
[30] L. D. Landau and E. Lifshitz, *Mécanique des fluides*, 131n. My translation.

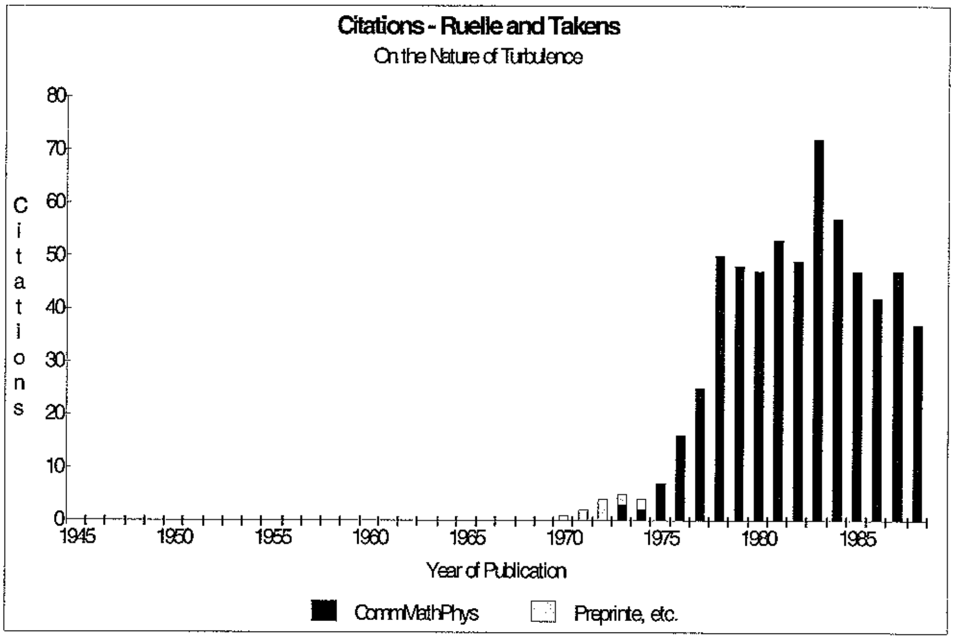
turbulence), and not at all with the mechanism responsible for the transition to turbulence, it will not be dealt with it in any detail.[32] But, since the roots of Ruelle and Takens's theory lay in a confrontation between fluid mechanics and dynamical systems theory, one may wonder what a mathematician of Kolmogorov's stature, who had deep knowledge in both hydrodynamics and qualitative dynamics, and who directed a seminar on just these two topics together in the late 1950s at the Moscow State University, might have thought of Landau's scheme. According to his student Vladimir Arnol'd, Kolmogorov did not believe in Landau's theory and as early as the late 1950s made fun of Landau's torus: "Apparently, he [Landau] did not know of any other dynamical systems."[33]

(ii) The Hopf Bifurcation

One of the most important effects of Ruelle and Takens's article, as shown by the citation analysis summarized in Graph 8, was to draw the attention of a wide circle of scientists back to a pair of forgotten papers written in the 1940s. As two specialists of the stability theory approach of turbulence, Daniel Joseph and David Sattinger, wrote at about the same time as Ruelle and Takens: "Hopf's paper appeared in the *Sach.*

[31] L. D. Landau and E. Lifshitz, *Mécanique des fluides*, 131. My translation, and my emphasis.

[32] On Kolmogorov's contribution to the theory of developed turbulence, which lies outside of the scope of the present study, I refer to a careful introduction to the subject: U. Frisch, *Turbulence.* A quick historical treatment is to be found in M. Farge, "Évolutions des théories sur la turbulence développée," *Chaos et déterminisme,* ed. A. Dahan Dalmedico, et al.: 212-245. See also his original paper: A. N. Kolmogorov, "Local Structure of Turbulence in an Incompressible Fluid at Very High Reynolds Numbers," *Doklady Akademi Nauk SSSR,* 30 (1941): 299-303.



Graph 8: Citations to David Ruelle and Floris Takens, "On the Nature of Turbulence," *Communications in Mathematical Physics,* 20 (1971), according to the *Science Citation Index,* 1949-1988.

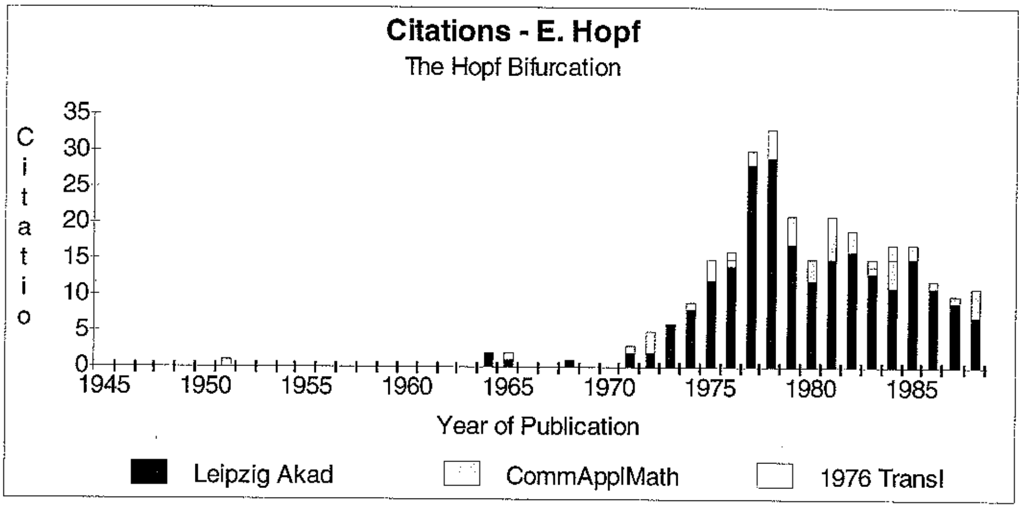
Akad. der Wiss. of 1942 and, unfortunately, is not generally available. . . . His results and techniques do not appear to be widely known."[34]

The citation analysis of David Ruelle and Floris Taken's 1971 famous article allow us to assess Ruelle's own perception that it was slow to be appreciated. Graph 8 shows the evolution in the number of citations to this article. Knowing that the paper was rejected by the referee of the first journal it was sent to, we might suspect an underground career. In order to best account for initial reactions to the preprint, I have

---

[33] V. I. Arnol'd, "On A. N. Kolmogorov," *Golden Years of Moscow Mathematics,* ed. S. Zdravkovskan and P. L. Duren (Providence: American Mathematical Society, 1993): 129-153, 130.

[34] D. D. Joseph and D. H. Sattinger, "Bifurcating Time Periodic Solutions and their Stability," *Archive for Rational Mechanics and Analysis,* 45 (1972): 79-109, 81.



Graph 9: Citations to Eberhard Hopf, "Abzweigung einer periodischen Lösung," and "A Mathematical Example," and also the translation: "Bifurcation of a Periodic Solution," according to the *Science Citation Index,* 1945-1988.

included citations to Ruelle's work labeled as "to be published", "unpublished", "personal communication", and "preprint," during the period 1970-1974, with no insurance, however, that they specifically referred to Ruelle and Takens's work.

Even by taking into account its potential underground trajectory, the career of the Ruelle-Takens article was quite slow to take off, indicating that its reception was not a trivial affair. The period that saw the biggest surge in interest was 1975-1978, thus mirroring almost perfectly the reception of Lorenz's paper, with a shorter latency period (Fig. 11).

Citation analysis also establishes that Ruelle and Takens's paper was crucial for having brought attention on Hopf's work on bifurcation theory. In graph 9, I plotted the evolution of the number of citations that both of his articles dealing with the subject received in the period from 1945 to 1989. It is remarkable that these papers were hardly ever cited before 1971. Hopf's 1948 paper was mentioned only once, in 1951 by fluid mechanicist G. Batchelor, while his 1942 Leipzig paper was cited, for the first time, 22 years after its publication, by Courant Institute mathematician Jürgen Möser. This was two years before Thom referred to the Hopf bifurcation in *Structural Stability and morphogenesis.* It becomes quite obvious that it was the success of the Ruelle-Takens hypothesis that made Hopf's.

In his two articles, Eberhard Hopf (1902-1983) introduced and studied a *bifurcation* [*Abzweigung*], now known as the Hopf bifurcation (Fig. 13).[35] Modestly, he pointed out that he scarcely thought there was anything new in his result, emphasizing that the methods had "been developed by Poincaré perhaps 50 years ago."[36] Indeed, Henri Poincaré tackled a very similar problem in *Les Méthodes nouvelles de la mécanique céleste* published in 1892. He studied the following equation, reminiscent of Ruelle and Takens's version of the Navier-Stokes equation:

$$\frac{dx_i}{dt} = X_i(x_1,\ldots,x_n); \quad \frac{\partial X_i}{\partial t} = 0.$$

Poincaré then posed his problem as such: "Suppose that, in the [above] equation, the functions X_i depend on a certain parameter μ; suppose that in the case μ=0, we were

[35] E. Hopf, "Abzweigung einer periodischen Lösung von eine stationären Lösung eines Differentialsystems," *Berichten der Mathematisch-Physischen Klasse des Sächcischen Akademie der Wissenschaften zu Leipzig,* 94 (1942): 1-22; "Bifurcation of a Periodic Solution from a Stationary Solution of a System of Differential Equations," transl. L. N. Howard and N. Kopell, in *The Hopf Bifurcation and Its Applications,* ed. J. E. Marsden and M. McCracken (New York: Springer, 1976): 163-193; and E. Hopf, "A Mathematical Example Displaying Features of Turbulence," *Communications on Applied Mathematics,* 1 (1948): 303-322.

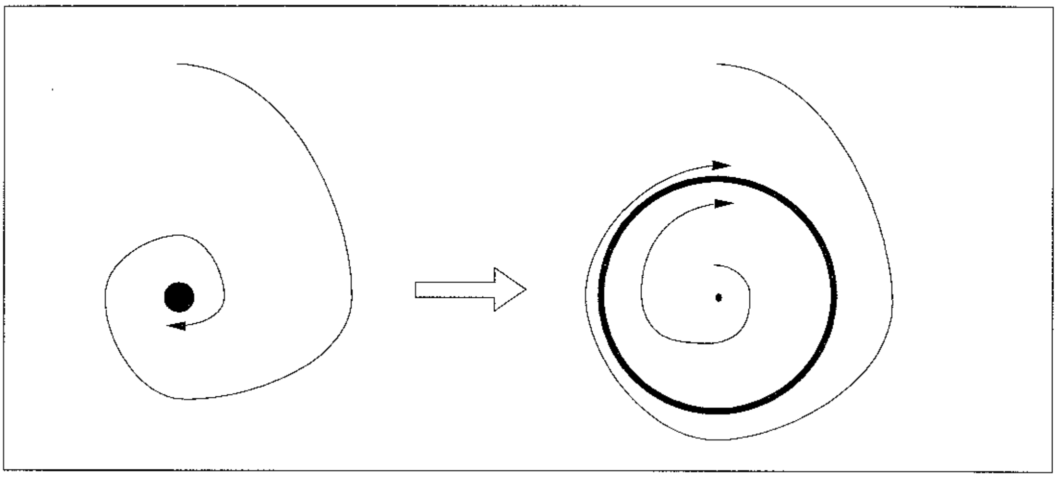


Figure 13: The Hopf Bifurcation of a Point Attractor into a Close Trajectory. Redrawn from R. Thom, *SSM,* 97.

able to integrate the equations, and that we thus noticed the existence of a certain number of periodic solutions. In which conditions will we have the right to conclude that the equations still exhibit periodic solutions for small values of μ?"[37] As opposed to Hopf however, Poincaré had not used the word bifurcation to describe this situation.[38]

Still, Hopf believed his results to be "not without value" because they bore on non-conservative systems, which Poincaré did not consider. It must also be noticed that this bifurcation was extensively discussed by Aleksandr Andronov already in the

[36] E. Hopf, "Bifurcation," 168.
[37] Henri Poincaré, *Les Méthodes nouvelles de la mécanique céleste,* 1 (Paris: Gauthier-Villars, 1892), 81.
[38] He reserved the word bifurcation for another case: H. Poincaré, "Sur l'équilibre d'une masse fluide animée d'un mouvement de rotation," *Acta Mathematica,* 7 (1885): 159-380; repr. *Œuvres,* 7: 40-141. See O. Gurel, "Poincaré's Bifurcation Analysis," *Bifurcation Theory and Applications in Scientific Disciplines,* ed. O. Gurel and O. E. Rössler (New York: New York Academy of Sciences, 1979): 5-26.

early 1930s.[39] Vladimir Arnol'd later wrote that the neglect of Andronov's contribution in the 1970s may have been partly his fault since he failed to emphasize it when he started talking—to Thom especially—about the Hopf bifurcation in the mid-1960s.[40] Nevertheless, for the mathematicians and physicists who took up Hopf's work where he had left it in 1948, "Hopf's crucial contribution was the extension from two dimensions to higher dimensions," apparently not considered by Andronov, who mainly worked in two dimensions.[41]

Born in 1902, Eberhard Hopf received his Ph.D. from Berlin University in 1925 for a dissertation in real analysis.[42] From 1926 to 1930 he then worked at the *Astronomische Rechenzentrum* at Berlin University. Like Poincaré and Birkhoff, he approached the study of differential equations starting from astronomical concerns. Hopf then spent the next six years in the United States at the Harvard Astronomical Observatory and then at the Massachusetts Institute of Technology (MIT), Cambridge, Mass., where he might have been in contact with George D. Birkhoff.

---

[39] V. I. Arnol'd, "Catastrophe Theory," 229-230, in which are cited: A. A. Andronov, "Mathematical Problems of the Theory of Self-Oscillations," *The First All-Union Conference on Auto-Oscillations, November 1931* (Moscow: GTTI, 1933): 32-71; and A. A. Andronov and E. A. Leontovich, "Some Cases of Dependence of Limit Cycles on Parameters," *Uschen. Zap. Gor'k. Univ.*, no. 6 (1939): 3-24. See also S. Diner, "Les voies du chaos déterministe dans l'école russe," *Chaos et déterminisme,* ed. A. Dahan Dalmedico et al., 342.
[40] V. I. Arnol'd, *Catastrophe Theory,* 35.
[41] J. E. Marsden and M. McCracken, "Preface," *The Hopf Bifurcation and Its Applications,* ed. J. E. Marsden and M. McCracken (New York: Springer, 1976), ix.
[42] Information for the following paragraphs was gathered from: M. Denker, "Eberhard Hopf: 04-17-1902 to 07-24-1983," *Jahresbericht der deutschen Mathematiker-Vereinigung,* 92 (1990): 47-57; and A. Icha, "Eberhard Hopf (1902-1983)," *Nieuw Archief voor Wiskunde,* 4th ser., 12 (1994): 67-84. Significantly, earlier tributes to Hopf did not emphasize his work on the Hopf bifurcation; see, e.g., P. M. Anselone,

In 1936, he was appointed at Leipzig University to fill the chair of the late Leon Lichtenstein, the founding editor of the *Mathematische Zeitschrift.* Going back to Germany when it was ruled by the Nazis, Hopf's move was resented by some Cambridge mathematicians. Throughout those years, Hopf worked on fields of mathematics that were closely related to physical concerns, more precisely the theory of partial differential equations and ergodic theory. In this later field, one most note that using the work of Birkhoff and von Neumann, he proved the ergodicity of surfaces of negative curvature.[43] This was the work that René Thom studied for his doctorate, and presented at the Bourbaki Seminar in 1951.

In 1942, Eberhard Hopf was drafted in the German war effort and called to serve at the *Luftfahrtforschungsinstitut* in Einrig near Munich. Was it in contact with applied fluid dynamics that he became interested in bifurcation theory? One thing is certain: already in 1942, Hopf's paper briefly pointed out the possible application to fluid dynamical cases, a field that he had started considering in the early 1940s.[44]

Appointed to Carathéodory's chair at Munich in 1944, he left in 1947 for the Institute for Mathematics and Mechanics at NYU, directed by Richard Courant (later to become the Courant Institut), "imported . . . as a paper clip scientist for the U.S.

"In honor of Professor Eberhard Hopf on the occasion of his seventieth birthday," *Applied Analysis,* 3 (1973): 1-5.

[43] See especially, E. Hopf, *Ergodentheorie*, Ergebnisse der Mathematik und ihrer Grenzgebiete, 5(2) (Berlin: Springer, 1937), where a unified treatment of this young field of analysis was introduced. For a history of this theory, see A. Lo Bello, "On the Origin and History of Ergodic Theory," *Bollettino di storia delle scienze matematiche,* 3 (1983): 37-75.

[44] E. Hopf, "Ein allgemeiner Endlichkeitssatz der Hydrodynamik," *Mathematische Annalen,* 117 (1941): 764-775.

Army." He stayed abroad because, he said, "in the USA, there's more time for research."[45]

It was while at Courant's Institute that he wrote his second paper dealing with the Hopf bifurcation, this time strongly emphasizing the turbulence problem. It was published in the Institute's own *Communications in Applied Mathematics,* in an issue where "all the papers . . . represent[ed] results which were obtained at the Institute . . . under contract with the Office of Naval Research of the US Navy."[46]

In 1948, Hopf took up a teaching assignment with the U.S. Navy and then joined the Institute of Mathematics at Indiana University (in order to work with Clifford Truesdell), where he stayed until his retirement in 1972. He spent the last decades of his mathematical career working on hydrodynamic problems. But rather than exploiting the Hopf bifurcation, he chose to develop further Jean Leray's ideas, also cited by Ruelle and Takens, and introduced below. Says his biographer, Hopf's "interest in hydrodynamics and turbulence . . . [was] based on his deep understanding of differential equations."[47]

(iii) What Hopf Did and What He Did Not Do, Compared with Ruelle and Takens

There are many similarities between Hopf's original pair of papers and Ruelle and Takens's. From today's point of view, Hopf's work can be seen as already drawing the connection between turbulence and bifurcation theory, as exhibiting notions similar to

[45] M. Denker, "Eberhard Hopf," 48. About the Courant Institute, see A. Dahan Dalmedico, "L'Institut Courant: le bastion des maths appliquées," *La Recherche,* 300 (1997): 106-111.
[46] *Communications on Applied Mathematics,* 1 (1948).

that of attractors and genericity, and, like Landau, as explaining turbulence in terms of an accumulation of frequencies. There also were obvious differences between Hopf's work and Ruelle and Takens's. To confront them is a worthwhile exercise since it underscores where the originality lay in Ruelle and Takens's treatment of turbulence.

To start with, the appearance of a periodic solution from a stationary one in a system of the form $dx/dt=F(x,\mu)$ forms the basic result of Hopf's 1942 paper. The method Ruelle and Takens later used to prove Hopf's theorem hardly departed from Hopf's. Moreover, already in 1942, examples taken from hydrodynamic situations were on Hopf's mind. In fact, he used classic results about the stability of solutions of hydrodynamic equations as a way to orient his investigation of the bifurcation:

> Since in nature only stable solutions can be observed for a sufficiently long time of observation, the bifurcation of a periodic solution from a stationary solution is observable only through the latter becoming unstable. Such observations are well known in hydrodynamics. For example, in the flow around a solid body; the motion is stationary if the velocity of the oncoming stream is low enough; yet if the latter is sufficiently large it can become periodic.[48]

In 1942, Hopf also mentioned the case of Taylor-Couette flows, an example also briefly raised by Ruelle and Takens.[49] In 1948, Hopf's second paper was exclusively concerned with hydrodynamics and turbulence. Of course, the first equations to be found in this article dealing with hydrodynamics were the Navier-Stokes equations. But, like Ruelle, Hopf saw that his approach could be applied to problems beyond turbulence. "There is no doubt, however, that many characteristic

---

[47] M. Denker, "Eberhard Hopf," 48.
[48] E. Hopf, "Bifurcation," 167.
[49] E. Hopf, "Bifurcation," 169n.

features of the hydrodynamic phase flow occur in a much larger class of similar problems governed by non-linear space-time systems."[50]

Like Ruelle and Takens, Hopf too parametrized the Navier-Stokes equation in an abstract way by using the letter μ, but it was proportional to the inverse of Ruelle and Takens's own μ. So, for μ large, he wrote, "the only flow observed in the long run is a stationary one (laminar flow). This flow is stable against arbitrary initial disturbances." He clearly expressed that this flow represented "a single point in Ω [i.e. the phase space]."[51]

For smaller μ, the common understanding then was, Hopf contended, that "the turbulent flow observed instead displays a complicated pattern of apparently irregularly moving 'eddies' of various sizes." While Leray had suggested in the 1930s that turbulence was due to a loss of regularity of the solutions (see below p. 472), Hopf argued that there existed a smallest size of eddies, so that, on a small scale, the flow seemed laminar and "the regularity of the flow would never be doubted."

Instead "the qualitative mathematical picture which the author conjecture[d]" was the following. In the long run, the solution corresponded to a manifold $M(\mu)$ in phase space which was invariant under phase flow. This manifold had a finite number of dimensions $N(\mu) = \dim M(\mu)$. For μ large, M was a point and N=0. After the first bifurcation at μ_1, M became a one-dimensional Poincaré limit cycle. Then for μ_2, μ_3, etc. smaller and smaller, $N(\mu)$ increased at each bifurcation. Unaware of Landau's work on turbulence, or not seeing the connection, Hopf believed his model for

---

[50] E. Hopf, "A Mathematical Example," 304.

turbulence "to be the first of its kind." [52] He emphasized that it was in *qualitative* accord with turbulence, but not in a *quantitative* one. From today's perspective, it is easy to see that this scheme overlapped with Landau's, but this was expressed in the totally different language of qualitative dynamics. It is therefore not surprising that David Ruelle, aware of both Landau's model and dynamical systems theory as it was being developed at the IHÉS in the late 1960s, would see this similarity.

From a retrospective standpoint, many concepts used by Ruelle and Takens, with which they came in contact only through the latest developments in qualitative dynamics, are also to be found in Hopf's articles. He worked in phase space as a convenient way to visualize the solutions of the Navier-Stokes equations. Like Ruelle and Takens, he wondered: "What is the asymptotic future behavior of the solutions, how does the phase flow behave for $t \rightarrow \infty$?"[53] This proto-notion of attractors, which however lacked certain of its important characteristics, can easily be recognized as Birkhoff's ω-limit sets.

Moreover, Hopf used a proto-notion of genericity already present in the work of Poincaré and Birkhoff on ergodic theory, with which he was well acquainted. "Stability here means that the 'majority' of phase motions tends for $t \rightarrow \infty$ toward $M(\mu)$. We must expect that there is a 'minority' of exceptional motions that do not converge toward M."[54] But, as opposed to his predecessors and successors, he made no attempt

[51] E. Hopf, "A Mathematical Example," 304-305.
[52] All quotes above are from E. Hopf, "A Mathematical Example," 304. Note that Landau was not cited by Hopf.
[53] E. Hopf, "A Mathematical Example," 304.
[54] E. Hopf, "A Mathematical Example," 305.

at giving a mathematical definition of what was meant by "majority" and "minority" when dealing with phase motions.

What therefore could Ruelle and Takens do that Hopf had not? Well, the second Hopf bifurcation, in which the limit cycle is 'inflated' into a torus, had been conjectured by Hopf, but not rigorously derived. Using Poincaré section techniques, by then a classic tool of qualitative dynamics, Ruelle and Takens showed that the second Hopf bifurcation could indeed occur. Generally, the outlook of Ruelle and Takens's article was also quite different from Hopf's. While Hopf's papers elegantly suggested his results, after thirty years of Bourbakism, Ruelle and Takens's looked much more rigorous, too much so for physicists, but as we shall see, not enough for mathematicians.

Of course, Ruelle and Takens's most original suggestion was that the picture conjectured by Landau and Hopf was not generic, and that an open set of strange attractors, a notion they then introduced, had to exist in the vicinity of any quasiperiodic flow involving at least four oscillatory modes. Ruelle and Takens's exploitation of recent results in dynamical systems theory, and more importantly, their adaptation of what I have called the modeling practices of applied topologists, will be the topic of section 3 below. Another possible explanation of turbulence that Ruelle considered was Jean Leray's. This case highlights a wholly different mathematical approach of the turbulence problem, much more informed by analysis.

c) Leray: Turbulence as Irregularity

On October 22, 1969, mathematician Jean Leray of the Collège de France spoke at the Institut des hautes études scientifiques. The title of his talk was: "Turbulent Solutions to the Equations of Fluid Mechanics."[55] This had been the topic of his doctoral thesis in 1933, supervised by French fluid mechanist Henri Villat.[56] Since the mid-1930s, however, Leray had spent little time working specifically on this problem. He rather chose to devote much of his time to the study of partial differential equations and topology in more abstract ways. As Ruelle and Takens only referred to Leray's 1934 article in *Acta mathematica,* we may therefore suppose that he essentially presented the same views when he addressed the physicists of the IHÉS, 35 years later.

(i) Turbulent Solutions

In his doctoral thesis, Leray conceived of turbulence as the breaking down of the Navier-Stokes equations at a certain point. Faced with the problem of establishing the existence of solutions to the Navier-Stokes equations, Leray considered an integral equation, due to the Upsala physicist C. W. Oseen, whose solutions did not need to be

---

[55] *Rapport scientifique, Année 1969 - Séminaires et conférences* (2/6/70), 6. Arch. IHÉS. At the time Ruelle first started to become interested in fluid mechanicshe met with Leray, on 16-18 May 1968, at the *Sixième rencontre entre physiciens et mathématiciens de Strasbourg,* where the later spoke of Feynman's integrals. I however ignore whether they discussed turbulence on this occasion.

[56] J. Leray, "Études de diverses équations intégrales non linéaires et de quelques problèmes que pose l'Hydrodynamique," *Journal de mathématiques pures et appliquées,* 12 (1933): 1-82; "Essais sur les mouvements plans d'un liquide visqueux que limitent des parois," *Journal de mathématiques pures et appliquées,* 13 (1934): 331-418; "Sur le mouvement d'un liquide emplissant l'espace," *Acta Mathematica,* 63 (1934): 193-248.

differentiable.[57] In doing so, Leray succeeded in using the most recent techniques of topology and the theory of functional sets in order to solve a physically-motivated problem, but at a cost:

> We tried to establish the existence of a solution to the Navier-Stokes equations corresponding to a given initial state: we succeeded only by renouncing regularity of the solution at some conveniently chosen instants.[58]

Leray thus defined "turbulent solutions to the Navier-Stokes equations" as irregular solutions to Oseen's integral equation. The relation of "turbulent solutions" to what physicists called turbulence, however, was not obvious.[59] This work on the theory of equations and fluid mechanics was highly praised by Villat and Lebesgue, and in 1934, Leray received the well-endowed Henri de Parville Prize from the Academy of Sciences.[60]

Leray's suggestion was totally different from what Landau and Hopf would propose ten to fifteen years later. Both had assumed that the Navier-Stokes equations would apply at any Reynolds number whatsoever. "There must exist, in principle, for all problems," Landau and Lifshitz postulated, "an exact stationary solution of

---

[57] C. W. Oseen, "Sur les formules de Green généralisées qui se présentent dans l'hydrodynamique et sur quelques unes de leurs applications," *Acta mathematica,* 34 (1911): 205-284; and several articles published in the *Arkiv för matematik, astronomi och fysik,* from 1906 to 1919. Oseen already suggested that irregular solutions represented turbulence, see P. Appel, H. Begin, and H. Villat, "Développements concernant l'hydrodynamique," d'après l'article allemand de A. E. Hove, *Encyclopédie des sciences mathématiques pures et appliquées,* French ed. J. Molk and P. Appel, original German ed. F. Klein and C. H. Müller, tome IV, vol. 5 (Paris: Gauthier-Villars, 1914; repr. Jacques Gabay, 1993), 181.

[58] J. Leray, "Sur le mouvement," 245.

[59] J. Leray, "Sur certaines classes d'équations intégrales non linéaires," *CRAS,* 194 (1932): 1627-1629, 1629. See J. Leray, "Problèmes non-linéaires," *L'Enseignement mathématique,* 35 (1936): 139-151, 149.

hydrodynamical equations. . . . These solutions formally exist for any Reynolds number."[61] Neither Landau nor Hopf made any remark that might indicate that he had considered Leray's explanation of turbulence at the time. It was not that Leray's proposition was disproved by any later findings, but rather that no evidence was offered that his hypothesis—a radical one since it implied that the fundamental equation of fluid mechanics was no longer true after some critical point—was needed in order to account for turbulence.[62] But it is worth noticing here that, in the 1950s, Eberhard Hopf would choose to pursue Leray's approach instead of the bifurcation explanation he suggested in 1948.[63]

Leray's hypothesis ran against most of the history of turbulence in that it supposed that the Navier-Stokes equations ceased to describe fluid flows faithfully after a certain critical value, and had to be replaced by an integral equation.[64] This

---

60 See *CRAS,* 199 (1934): 1479; and Lebesgue and Villat's praises in Paul Fallot's report, Assemblée des professeurs (24/11/46). Arch. CdF. G-iv-l 27Bb.
61 L. D. Landau and E. Lifshitz, *Mécanique des fluides,* 126. My translation from the French.
62 As late as 1991, Marie Farge would contend that Leray's "hypothesis has been up until now neither confirmed nor refuted." M. Farge, "Évolutions des théories," 223.
63 Among many papers, see E. Hopf, "Remarks on the Functional-Analytic Approach to Turbulence," *Hydrodynamic Instability,* ed. G. Birkhoff, et al. (Providence: AMS, 1962): 157-164.
64 There is much that could be said about Leray's work on turbulence and how it fits with long-term trends in the history of fluid mechanics, and especially with the interplay of mathematics, physics and engineering in the French context of the interwar period. This effort was done around Villat and sponsored in large part by the Ministry of Air: "one of the first examples [in France] of a scientific and technological policy." See P. Mounier-Kuhn, "L'enseignement supérieur, la recherche mathématique et la construction de calculateurs en France (1920-1970)," *Colloque Enseignement supérieur et formations technico-scientifiques supérieures en Lorraine - XIXe-XXe siècles, Metz, décembre 1995,* 8. See P. Mounier-Kuhn, "Un programme technologique national: la Mécanique des fluides," *Programmes Villes et institutions scientifiques, Rapport final,* ed. A. Grelot and M. Grossetti (CNRS PIR Villes, 1996).

suggestion could be seen as stemming from the work of Yves Rocard, whom we have already encountered studying self-oscillations (Chapter V). In 1929, the young Rocard gave a series of ten conferences at the Institut de mécanique des fluides at the Sorbonne, in which he proposed to examine the relationship of hydrodynamics with the kinetic theory of gases.[65] The molecular hypothesis, together with the statistical methods introduced by Maxwell and Boltzmann, allowed him to recover the Euler and Navier-Stokes equations, which were thereby confirmed as being consistent with contemporary beliefs about the molecular structure of matter. Boundary conditions could however have the effect of changing the dynamical equations of fluids. He concluded that "the molecular hypothesis . . . imposed by reality, already leaves the classical frame of hydrodynamics." This conclusion, he added, "takes nothing away from the mathematical interest of the problems of viscous fluid hydrodynamics, but *one should not expect ever to see a definitive concordance of [hydrodynamics] with data.*" The turbulence problems—imposed by experiments, Rocard noted, and not theory—might necessitate going beyond the classical theory of Navier and Stokes.

> That viscous fluid hydrodynamics itself possessed the power to solve, to treat such problems, this had hitherto not appeared to involve the shadow of a doubt, so great was the confidence in the value of this discipline to adapt to facts. Now *there is ground for us to be more worried, more skeptical* with respect to these possibilities and . . . it seems clear that it is on the contrary by

---

I thank Pierre Mounier-Kuhn for having provided me these two texts. On the Pérès-Malavard computing machine for hydrodynamics, see J. Pérès, with L. Malavard, *Cours de mécanique des fluides (fluides parfaits, aile portante, résistance)* (Paris: Gauthier-Villars, 1936); and L. Malavard, *Applications des analogies électriques à la solution de quelques problèmes de l'hydrodynamique* (Paris: Blondel de la Rougery; Gauthier-Villars, 1934).

[65] Y. Rocard, *L'Hydrodynamique et la théorie cinétique des gaz* (Paris: Gauthier-Villars, 1932).

> leaving the framework of classical hydrodynamics that difficulties of this kind could be tackled.[66]

By undermining the validity of the Navier-Stokes equations, Rocard's speculations might have provided the ground on which Leray's model could be built.

Even while unhappy with the scheme proposed by Landau and Hopf, David Ruelle quite bluntly dismissed Leray's ideas, too. "While such a breakdown [of the validity of the Navier-Stokes equations] may happen, we think that it does not necessarily accompany turbulence."[67] He just did not want to deal with the complicated issue of the existence and uniqueness of the solutions to the Navier-Stokes equations: "Turbulence has probably nothing to do with these difficulties."[68] This later field had been, and would be, the object of numerous studies; it was the concern of many applied mathematicians, without much to show as an end result of decades of research as far as physical problems were concerned. Ruelle just deemed it irrelevant, since he could propose an explanation which only relied on the hypothesis that turbulent flows were the solution of a dissipative nonlinear equation, and not necessarily of precisely the Navier-Stokes equations.

(ii) What Use for the Theory of Equations? Existence and Uniqueness Theorems

David Ruelle therefore deemed a whole thriving branch of (applied) mathematics, namely the search for existence and uniqueness theorems in the theory of partial

---

[66] Y. Rocard, *L'Hydrodynamique,* 149-150. My emphasis.
[67] D. Ruelle and F. Takens, "On the Nature," 57. In his Lausanne lecture notes, Ruelle only cited the conclusion of Leray's paper, which I quoted above. D. Ruelle, "Méthodes d'analyse globale," 5-6.
[68] D. Ruelle, "Strange Attractors as a Mathematical Explanation," 295.

differential equations, as of little importance for the study of turbulence. This attitude was in a striking opposition to the views expressed by Szolem Mandelbrojt when, on February 16, 1947, he recommended Jean Leray's candidacy for the chair on Theory of Differential and Functional Equations at the Collège de France.[69] In his defense, Mandelbrojt claimed that formal integration of differential equations had lost the importance it once had.

> Even supposing that a [formal integral] could be obtained, one would have, in order to grasp the properties of the solution thus computed, much more difficulty than by simply starting from the fact that it satisfies the given equation. In other word, progress is rarely achieved by adopting this formal point of view, as it is now called with some contempt, which for that matter is rather justified.[70]

Mandelbrojt then claimed that the first property that one needed to establish was that the solution to an equation with given boundary conditions, *existed.* The second was that it was *unique*. Only after "these two problems having been solved, do we need study the general propoerties of the solution."[71] Strikingly, physicist Ruelle totally reversed mathematician Mandelbrojt's order of priority.

The approach to the study of differential equations which focused on existence and uniqueness theorems had a long history in mathematical physics. This history regularly exhibited misunderstanding between mathematicians and physicists. Its classic expression was that "in general, existence theorems have little interest for the

---

[69] A founding member of Bourbaki whom he had since left, Mandelbrojt was Benoît Mandelbrot's uncle. For biographical information about Mandelbrojt, and in particular his involvement with Bourbaki, see L. Beaulieu, *Bourbaki,* 384 and passim.
[70] Exposé de S. Mandelbrojt, Assemblée des professeurs (16/2/47). Arch. CdF. G-iv-l 28f.
[71] Exposé de S. Mandelbrojt (16/2/47). Arch. CdF. G-iv-l 28f.

physicist."[72] And Rayleigh confirmed this in 1916, when, reviewing the fourth edition of Horace Lamb's *Hydrodynamics,* he wrote:

> 'existence theorems,' . . . though demanded by the upholders of mathematical rigour, tell us only what we knew before, as Kelvin used to say. . . . What is strange is that there should be so wide a gap between [the physicist's] intuition and the lines of argument necessary to satisfy the pure mathematician.[73]

Leray's work in hydrodynamics could be taken as a counterexample for this widely shared view.[74] Here existence theorems seemed to imply that turbulence was due to the breaking-down of the classic Navier-Stokes equations. On the contrary, Ruelle and Takens's hypothesis was simply a reaffirmation that existence theorems were secondary to the physicist's concern with the behavior of solutions.

In view of Bourbaki's dominance of postwar French mathematics, it is somewhat surprising that Leray was chosen over another candidate who was none other than André Weil. Not only one of the most prominent Bourbakis, Weil was, by

---

[72] V. Volterra, "Drei Vorlesungen über neuere Fortschrifte der mathematischen Physik," *Arkiv der Mathematik und Physik,* 22 (1914): 97-181; repr. *Opere matematiche,* 3 (Rome: Accademia nazionale dei lincei, 1957): 389-470, 441. Quoted by J. Gray, "Mathematics and natural Science in the 19th Century: The extraordinary Success of the Classical Approaches. Poincaré, Volterra, Levi-Civita, Hadamard," *Colloque International d'histoire des mathématiques, Luminy Marseilles, September 1997.* See also G. Israel, "Vito Volterra: un fisico matematico di fronte ai problemi della fisica del Novecento," *Rivista di storia della scienza,* 1 (1984): 39-72.
[73] Lord Rayleigh, Review of H. Lamb, *Hydrodynamics,* 4th ed., *Nature,* 97 (1916): 318; repr. *Scientific Papers,* 6 (Cambridge: Cambridge University Press, 1899-1920): 400-401. Quoted by S. Goldstein, "Fluid Mechanics," 3.
[74] "You people spend much time and much wit to show the existence of solutions whose existence is often evident to us for obvious physical reason," Theodore von Kármán imagined an engineer saying to a mathematician. "Tooling up Mathematics for Engineering," *Quarterly of Applied Mathematics,* 1 (1943): 1-6, 4. On the other hand, Leray's methods for proving existence and uniqueness theorems were those taken up by Eberhard Hopf, and further developed by, among others, the founder of an important school of applied mathematics in postwar France, J.-L. Lions, "Les équations de Navier-Stokes," *Séminaire Bourbaki,* 11(3) (May 1959): exposé #184.

then, already recognized worldwide as a first class mathematician.[75] About Weil, Mandelbrojt simply remarked in 1947 that he was "a very great mathematician, but [that] there [was] no point in talking here of his work, since there [was] no link between Weil's work and the title of the chair created."

This shows that the relation between pure and applied, Bourbakism and anti-Bourbakism is no simple matter when dealing with immediate postwar France. Many factors informed the preference expressed by the Assembly of Professors at the Collège de France. We must note that Leray had not been Mandelbrojt's first choice. Indeed, the procedure for hiring new professors at the Collège de France was a two-step process. When funds for a chair became available, the initial decision that professors had to make was how to rename it, and only at a further meeting did they suggest people to fill it. In practice, however, the naming of the chair was the moment when decisions were really taken, as it often was the case that professors presented candidates at the same time as they argued for their chair. On November 26, 1946, Mandelbrojt expressed his preference that the chair replacing Paul Langevin's, after his retirement, be named "General Analysis and Calculus of Probability" and filled by Maurice Fréchet. In his proposal, Mandelbrojt argued for the daring abstraction of Fréchet's method, which, recalling Bourbaki's, focused on the study of elements of as general a nature as possible.[76]

---

[75] In June 1940, the Rockefeller Foundation asked G. D. Birkhoff and S. Lefschetz to designate the French mathematicians to be rescued from the debacle. Out of 9 names, only Weil (who was Jewish) and Henri Cartan belonged to the younger generation. L. Beaulieu, *Bourbaki,* 387.

[76] Proposition de la création d'une chaire d'Analyse générale et calcul des probabilités, par S. Mandelbrojt, Assemblée des professeurs (24/11/46). Arc. CdF. G-iv-l 27Cc.

A biologist, Paul Fallot dared to oppose Mandelbrojt, who was the only mathematician present at this meeting. He presented a masterful defense for a chair devoted to the theory of equations and to be filled by Jean Leray, whom he knew from having been his colleague at Nancy before the war.[77] Arguing that this topic of mathematics was hardly represented in Paris, Fallot underscored its fundamental importance as a "mathematical tool" which, for lack of necessary improvement, often blocked the progress of mathematical physics. Having shown the importance of the theory of equations for mathematics and other sciences, Fallot listed Leray's accomplishments, where his work on hydrodynamics figured prominently. Leray's work was remarkable for the new methods he used for the solution of differential equations encountered in nature. Moreover in the difficult circumstances of the war, Leray had also exhibited a gift for abstract topology. For Fallot, Leray's main strength lay in his ability to marry abstraction to practical concerns.

No doubt Jean Leray's war experience also played a role in his election at the Collège de France in 1946-1947. In Fallot's words, this was "a topic that one is almost ashamed of having to tackle, since it concerns not only the domain of Science." From 1940 to 1945, Jean Leray had spent five years in a German camp, where he put together a captive university. He refused to buy his freedom by becoming professor at

One should not go too far in identifying Fréchet with Bourbakism. I leave this responsibility to Mandelbrojt. See his debate with Daniel Lacombe in Synthèse, *Notion de structure et structure de la connaissance* (Paris: Albin Michel, 1956): 97-135. In addition, far from being an unconditional promoter of abstraction, Fréchet worked on building a calculating machine at the Institut Henri-Poincaré in 1939-1940. On this, see P. Mounier-Kuhn, "L'enseignement supérieur."

[77] Rapport de M. Paul Fallot sur la chaire de "Théorie des équations différentielles et fonctionnelles," Assemblée des professeurs (24/11/46). Arch. CdF. G-iv-l 27Bb.

the University of Berlin. This experience seems to have pushed him further to the side of pure mathematics. During his captivity, "Jean Leray pursued his research, but took care to leave aside questions of mechanics, which could have interested the enemy."[78]

"The history of Science shows," Fallot contended, "that great advances in Mathematics always, or almost always, had as their starting point the necessity of finding new methods of computation in order to account for phenomena that old methods were powerless to analyze." Significantly, the professors of the Collège de France, by 26 cast ballots against 8, preferred the methods explored by Leray to Fréchet's abstraction.[79] In effect, they voted at the same time against Fréchet's earlier attempts at numerical computations, which in the postwar era would provide new methods *par excellence* for the integration of equations.

3. DYNAMICAL SYSTEMS IN THE RUELLE-TAKENS MODEL

As early as the 1920s, George D. Birkhoff wrote that "topology deserves to obtain a more prominent position in physical theories than it has yet obtain."[80] It took a while for this to happen. But we may contend that if Ruelle and Takens were able to make a dent in such a formidable problem as turbulence, it was mainly because they were in a good position to capitalize on recent developments in topology, and especially in the

[78] F. Lot, "Jean Leray, aventurier de l'abstrait," *Figaro littéraire* (6-12 mars 1965), 11. However, Leray taught a course on compressible fluid in 1946 where he dealt at length with wings of airplanes: *Mécanique des fluides compressibles.* Cours du Centre d'études supérieures de la mécanique, section des fluides compressibles (1946). Jussieu Lib. Note the similarity of Leray's experience as a war captive with Fernand Braudel's, see Pierre Daix, *Braudel,* (Paris: Flammarion, 1995).
[79] Procès-verbal, Assemblée des professeurs (24/11/46). Arch. CdF. G-iv-1 27V. Rapport de M. Paul Fallot. G-iv-1 27Bb.

theory of dynamical systems brought about by Thom and Smale. The physicist's eye of David Ruelle, after having been in contact with these developments, could perceive the problem with Landau's scheme. It was unrealistic because *non-generic.* Infinite odds were against it. Before this could be shown rigorously, however, there was a lot of work to be done.

a) Thom, Smale, and the Concept of Attractors

(i) Acknowledgments

"The authors take pleasure in thanking R. Thom for valuable discussion, in particular introducing one of us (F.T.) to the Hopf bifurcation. Some inspiration for the present paper was derived from Thom's forthcoming book [*Stabilité structurelle et morphogénèse*]." Thus read the acknowledgments in Ruelle and Takens's article.

When the paper was written, in the spring of 1970, the IHÉS was indeed being visited by many mathematicians important for the development of what in Chapter VI I have called the modeling practice of applied topologists. Ruelle later remembered :

> The reason I did not like Landau's description of turbulence in terms of modes is that I had heard seminars by René Thom and studied a fundamental paper by Steve Smale called "Differentiable Dynamical Systems." . . . The former is my colleague at the Institut des Hautes Etudes Scientifiques near Paris, and the latter makes frequent visits there. From them I had learned the modern developments of Poincaré's ideas on dynamical systems, and from these developments, it was clear that the applicability of the mode paradigm is far from universal.[81]

---

[80] G. D. Birkhoff, "The Mathematical Nature of Physical Theories," *American Scientist,* 31 (1943): 281-310, 310; repr. *Papers,* 2: 890-919, 919.
[81] D. Ruelle, *Chance and Chaos,* 55. Note that his use of the word paradigm is here a clear reference to T. S. Kuhn's ideas: "I am not an uncritical believer in Kuhn's ideas; in particular, they appear to me of little relevance to pure mathematics. The physical

Ruelle and Takens's most original innovation was the concept of a *strange attractor*, for which they obviously relied on the concept of *attractor* as it was being defined in dynamical systems theory. While having a long prehistory that could go as far back as Poincaré and Birkhoff, the attractor concept was very recent.

(ii) Attractors

Arguments involving attractors were pervasive in Thom's work on catastrophe theory. While Ruelle and Takens made it famous, it was Thom who actually already drew attention to the Hopf bifurcation in *Structural Stability and Morphogenesis.*[82] Because their structural stability was immediate, Thom rarely considered attractors that were more complicated than points. But the limit cycle appearing after a Hopf bifurcation provided him with an example of what he called a "generalized catastrophe."[83] Confidently, Thom also raised the possibility of a second Hopf bifurcation from cycle to torus. In the first edition of his book, it was when dealing with the Hopf bifurcation that Thom revealed his ignorance of Poincaré, attributing the source of the word 'bifurcation' to Hopf.[84] Noticing, clearly as a consequence of Ruelle's work, its possible relevance for turbulence, Thom moreover used the Hopf bifurcation to model the phenomenology of mitosis.

---

concepts of *modes* and *chaos* seem, however, to fit rather well Kuhn's description of *paradigms.*" *Ibid.*, 177n7. His emphasis.

[82] R. Thom, *SSM,* 97-100, 108, 263-264, and 283.

[83] As Thom never was very clear about what he meant by a generalized catastrophe in mathematical terms, whether he actually considered the Hopf bifurcation as one of these may therefore be open to debate. Nevertheless, it is clear that he considered it to be a catastrophe, which was not elementary since not stemming from gradient dynamics. R. Thom, *SSM,* 97, 103.

[84] R. Thom, *SSM,* 1972 ed., 105. Corrected in later eds.

Nevertheless, the use of the Hopf bifurcation in itself which was little noticed in Thom's work. His greatest innovation lay in the description of this process in terms of *attractors.* It is not clear whether Smale or Thom first introduced this important concept in the theory of dynamical systems. "Each says the other invented it."[85] Actually, the first appearance in print of the word 'attractor' in this context is due to neither Thom nor Smale. In September 1959, Pinchas Mendelson, from the Polytechnic Institute of Brooklyn, gave a talk "On Unstable Attractors" at the Symposium on Ordinary Differential Equations and their Applications, in Mexico City, a meeting attended by both Smale and Thom.[86] No definition of attractor was then given and it was used as a synonym for a critical point which was the only minimal set of the ω-limit of the dynamics. This was very close to the definition later coined by Thom and Smale, and taken up by Ruelle and Takens: "A closed subset A of the non-wandering set Ω is an attractor if it has a neighborhood U such that [the ω-limit set of U is] A."[87]

Indeed, Thom's manuscript for *Stabilité structurelle et morphogénèse,* which I have seen at Princeton University and which clearly was revised between 1966 and

---

[85] Bob Williams's comment in *From Topology to Computation,* ed. M. Hirsch et al., 183. See their first definitions in R. Thom, *SSM,* 39; and S. Smale, "Differentiable Dynamical Systems," 786. At least once, however, Thom personally claimed responsibility for the term. R. Thom, "Problèmes rencontrés dans mon parcours mathématique: un bilan," *Publications mathématiques de l'IHÉS,* 70 (1989): 199-214, 203.

[86] P. Mendelson, "On Unstable Attractors," *Boletín de la Sociedad matemática méxicana,* 5 (1960): 270-276.

[87] D. Ruelle and F. Takens, "On the Nature," 170. For more on the early use of attractors, see R. F. Williams, "One-dimensional Non-Wandering Sets," *Topology,* 6 (1967): 473-487.

1969, included many instances of the use of the word 'attractor' as well as derived notions, such as the basin of an attractor. But it did *not* include the definition finally provided in 1972.[88] In a few places, the term "minimal sets" first used by Thom, was crossed out and replaced by "attractors."[89] The attractor concept thus came to carry a basic significance in Thom's modeling practice:

> Every object, or physical form, can be represented as an *attractor C* of a dynamical system on a space *M* of *internal variables.*[90]

Similarly, the term attractor was also defined in Smale's famous review paper "Differentiable Dynamical Systems." But it then appeared only on the 40th page of the paper, and was used only briefly, in particular to introduce the attractor that Ruelle and Takens would take up as an example of a "strange attractor." Shown to Smale by Jürgen Möser, this set was only brought up as an example of a nontrivial attractor, locally the product of a Cantor set and a manifold.[91] In his review paper, Smale's goal remained the classification of flows, and not that of their attractors. After the publication of Ruelle and Takens's work, the latter would become the goal of many 'chaologists' in the next generation.

As often happens in articles containing a concept appealing to the imagination, Ruelle and Takens did not formally define strange attractors in 1970.[92] They

---

[88] R. Thom, *SSM,* 38-40; *Stabilité structurelle,* 1972 ed., 56-57.
[89] See the discussion of the Hopf bifurcation, R. Thom, *SSM,* 98.
[90] R. Thom, *SSM,* 320; quoted by M. W. Hirsch, "The Dynamical Systems Approach," 29.
[91] S. Smale, "Differentiable Dynamical Systems," 786-788.
[92] D. Ruelle and F. Takens, "On the Nature," 170; "Méthodes d'analyse globale," 13, 23. They also used terms like "bizarre attractor" and "vague attractor." Only the latter, introduced by Thom, was defined in too technical a fashion to comment on this here. R. Thom, *SSM,* 27 and 39. Already used in the 1966 version of the manuscript. See

illustrated what they meant by strange attractors by using Smale's example. In practice, they included all attractors that were neither fixed points, nor limit cycles, nor quasiperiodic sets.

In studying dynamic processes in terms of attractors, the most important concept introduced by Thom actually was not the attractor itself, but rather the *basin of an attractor,* which Smale did not use in 1967. Indeed, the definition of an attractor provided by Thom and Smale could be seen as merely an insightful combination of Birkhoff's classic definitions of nonwandering, ω-limit, and minimal, sets, all well known to most people working on dynamical systems. But by defining the basin of an attractor A as the set of all points whose ω-limit set is A, i.e. which are attracted to A (this basin being the neighborhood U appearing in Ruelle and Takens's definition above), Thom allowed the cutting-up of the base space into several parts that could be studied separately.

As was shown in Chapter III, the proper context in which to understand this notion of attractor is in relation to Waddington's epigenetic landscapes. In this case, the concept of basin is immediate, as indicated by Thom's metaphor:

> on a contour map the basins attached to different rivers are separated by watersheds, which are pieces of crest lines, and these separating lines descend

simple introductions in D. Ruelle, "Strange Attractors," *Mathematical Intelligencer,* 2 (1980): 126-127; *TSAC,* 195-206; "Les attracteurs étranges," *La Recherche,* 11, (1980): 133-144; and C. Grebogi, E. Ott, and J. A. Yorke, "Chaos, Strange Attractors, and Fractal Basin Boundaries in Nonlinear Dynamics," *Science,* 238 (1987): 632-638. A good popular exposition of strange attractors can also be found in Gleick, *Chaos* and Kellert, *In the Wake of Chaos.*

> to saddle points, where they meet like ordinary points, but rise to summits where they may have flat cusp points.[93]

Focusing on the dual concepts of attractors and their basins allowed a program to be imagined. This program could be described as the classification of attractors of dynamical systems and the way they interacted with one another. It was a direct analogue of Thom's catastrophe theoretic program. No doubt it presented a formidable task, but the task could be contemplated.

b) Modeling Practices at the Institut des Hautes Études Scientifiques

Clearly, Ruelle's association with "applied topologists" was for him a major source of inspiration. But many historical treatments of chaos theory, except for some written by mathematicians, have tended to obscure this fact and especially the "inspiration" Ruelle acknowledged taking away from catastrophe theory. As Thom's works remained quite controversial for the following decades, this neglect might be interpreted as an instance of careful purification, conscious or not, of controversial sources by working scientists.[94]

But what I want to claim more. That Ruelle and Takens's work relied on *concepts* introduced by Smale and Thom in dynamical systems theory is clear simply from looking at their joint paper. I contend, however, that Ruelle's *modeling practice* was crucially shaped by close contact with the activities that went on around Thom at

---

[93] R. Thom, *SSM,* 39-40. See illustration 25 of the plaster models built by Marcel Froissart for Thom, in *SSM,* illus-x.

[94] A telling example of this attitude is provided by a recent article of Ivar Ekeland's in which while comparing catastrophe theory with chaos, he totally obscured the fact that they depended on one another for their historical genesis: "La théorie des catastrophes. Relu 20 ans après par son auteur." *La Recherche,* 301 (1997): 89.

the IHÉS. In this way, his situation at Bures-sur-Yvette, not only provided him an opportunity to learn about new developments in a mathematical theory that were only starting to become known outside a small circle, but showed him ways in which these new mathematical tools could be used in the concrete practice of building models for natural phenomena. Briefly put, the modeling practice Ruelle and Takens adapted to the problem of turbulence could be described as follows: (1) *isolate topological features* in some process, in this case the onset of turbulence; (2) explore the way these features can, or cannot, *bifurcate* under various circumstances, relying on the postulate of genericity; and (3) provide *explanations* based on mathematical results.

In addition, Ruelle's position at the IHÉS was directly responsible for his meeting with Floris Takens. Born in 1940, Takens was a mathematician of the University of Groningen in the Netherlands. Just a few years earlier, he had received his Ph.D. from the Mathematisch Instituut of the University of Amsterdam, working with Nicolaas Kuiper, who we may recall was interested in topics close to Smale's, Thom's and Zeeman's, and would become the second director of the IHÉS in 1971. Working on the singularities of differentiable mappings and vector fields, Takens and Kuiper had been in contact with the IHÉS and had invited John Mather in 1969.[95] Following a suggestion of Kuiper's, Thom invited Takens to spend the 1969-1970 academic year at Bures-sur-Yvette.[96] His resulting paper with Ruelle was the only

[95] F. Takens and J. Mather files. Arch. IHÉS.
[96] Lettres de Nicolaas Kuiper à René Thom (16/4/69); de Floris Takens à René Thom (27/4/69). Arch. IHÉS.

incursion Takens, who remained a specialist in the theory of dynamical systems, ever made into physics.[97]

As Takens acknowledged in his joint paper with Ruelle, even his knowledge of the Hopf bifurcation was due to Thom. In fact, Takens's contribution, like that of the IHÉS in general, again confirms that more than mere abstract concepts, it was practices that were in the process transferred from mathematics to physics.

In order to make this claim clearer, let me compare the modeling practice exhibited by Ruelle and Takens with those examined when looking at the almost contemporary 1971 Bahia Symposium. Recall that at this occasion Smale, Thom, and Zeeman had presented exemplary exposés of their modeling practices with respect to the exploitation of what they called global analysis or catastrophe theory. In Chapter VI, the fact that they already differed significantly in the ways they chose to model natural phenomena has been made explicit. Their practices of mathematization disagreed on the manner in which one should identify physical parameters with topological features. In brief, while Smale relied on a discipline that was already highly mathematized, Thom and Zeeman were quite cavalier in this respect. The mathematical techniques they used were very close to one another, but clearly distinct: Zeeman using the formal theory of elementary catastrophes, Thom a vaguer theory of generalized catastrophes, and Smale general topological methods. When trying to interpret their mathematical results, their attitude again showed divergences. Zeeman looked for differential equations that could be experimentally tested, Thom

[97] See e.g. F. Takens, "Singularities of Vector Fields," *Publications mathématiques de l'IHÉS,* 43 (1974): 47-100.

made great claims but fell back on vague philosophical arguments to back them up, and Smale more prudently refrained from drawing important conclusions from the "theorems" he had partly proved.

From their article, we may infer that, of the above three, Ruelle and Takens's modeling practice was closest to Smale's. The substratum they had to deal with was rather uncontroversial. It was given by the velocity field of fluid flows governed by the Navier-Stokes equations. But just as for Zeeman's case, the identification could not be immediate: in particular, the phase space of this velocity field was infinite-dimensional, which posed a problem as far as the techniques of Smale's dynamical systems theory were concerned. They proved the legitimacy of the reduction from an infinite number of dimensions to two dimensions, but not the more complex cases of three and four dimensions.[98] Briefly, while the substratum was straightforwardly identified, the pertinence of reducing it to a low-dimensional manifold was not obvious in mathematical terms. Here they relied on common assumptions about the onset of turbulence—Landau's degrees of freedom—without attempting to derive them from the Navier-Stokes equations.

Generally speaking, it might be worth emphasizing a truism, namely that a mathematization of natural phenomena can only succeed in the direction afforded by available mathematical tools. Leray's irregularity hypothesis for turbulence stemmed from his ability to prove, or not, existence theorems for the Navier-Stokes equations; Ruelle and Takens bold assumption was required by the modeling practices they wanted to exploit, which forced them to work in low-dimensional spaces, because only there were the tools available. Neither Leray's nor Ruelle and Takens's model for turbulence was never rigorously proved or disproved. Changes in ontological beliefs were thus crucially informed by limitations on available mathematical techniques, and to be fair, in Ruelle and Takens's cases, by experiments (Chapter VIII).

c) Strange Attractors and Genericity

Ruelle and Takens made one crucial conceptual innovation when they introduced the notion of a strange attractor, which however always remained hazy for mathematicians.[99] Due to the presence in the neighborhood of the multidimensional manifold suggested by Hopf of an open set of "strange attractors," they claimed, the quasiperiodic scenario had no chance of being observed. But again, this innovation in dynamical systems theory was mediated though the practice of using such concepts as attractors and genericity.

The core of most of Smale's, Thom's and Zeeman's practices, both in mathematics and in their modeling activity, was the identification of generic properties. Even while picking up the term from Italian algebraic geometry, Thom was aware that it was a slippery concept. "The adjective 'generic' is used in mathematics in so many senses that to restrict its usage within the framework of formal theory is probably unreasonable." Between 1969 and 1972, he acknowledged that Smale had made a welcome clarification when he restricted the use of the adjective to properties of the topological space, rather than to the points of this

[98] F. Takens and D. Ruelle, "On the Nature," section 5, 176-178.

space.[100] As Thom then emphasized, the use of genericity was an art that was difficult to make rigorous. This very point would become one of the weaker, and much criticized, parts of Zeeman's whole approach in using catastrophe theory for modeling natural phenomena.

Ruelle and Takens's reliance on genericity therefore was another place where they had to adapt not only a *concept* from dynamical systems theory to the turbulence problem, but also a *practice,* which was mediated through their close interaction with Thom's school at the IHÉS. First, they noticed that Smale's example was stable under small pertubations. From which, they concluded that "the existence of such a 'strange' attractor therefore is not a non-generic pathology."[101]

By showing that in the neighborhood of quasiperiodic motions in more than 3 dimensions a generic set of such strange attractors existed, Ruelle and Takens felt entitled to pronounce that complicated quasiperiodic motions, i.e. with more than three frequencies, could not physically occur. They redefined turbulence as aperiodic fluid motion. But one should note the tentativeness of their language when they made such a proposition.

> For $\mu>0$ we know <u>very little</u> about he vector field X_μ. Therefore it is <u>reasonable</u> to study *generic* deformations from the situation at $\mu=0$. In other words we shall ignore possibilities of deformations which are <u>in some sense</u>

---

[99] See, e.g., M. W. Hirsch, "The Dynamical Systems Approach to Differential Equations," *Bulletin of the American Mathematical Society,* n.s., 11 (1984): 1-64, 30.
[100] R. Thom, *SSM,* 35n.1. The middle sentence referring to Smale's suggestion was absent from the manuscript. See S. Smale, "Differentiable Dynamical Systems," 748.
[101] D. Ruelle, "Méthodes d'analyse globale," 13; D. Ruelle and F. Takens, "On the Nature of Turbulence," 171. In 1971, Ruelle acknowledged that "the notion of genericity . . . is not very satisfactory when physical applications are considered." See "Strange Attractors as a Mathematical Explanation," 293n.

> exceptional. This point of view could lead to serious error if, by some law of nature which we have overlooked, X_μ happens to be in a special class with exceptional properties. It appears however that a three-dimensional viscous fluid conforms to the pattern of generic behavior which we discuss.[102]

As stated before, Ruelle and Takens's approach made no use of the particular form of the Navier-Stokes equations. "Of course something is known of this structure, and also of the experimental conditions under which turbulence develops, and a theory should be obtained in which these things are taken into account."[103] But they would not attempt it. Commenting of Ruelle and Takens's work, a hydrodynamicist, Manuel Velarde, argued that this was

> a point of *philosophy*: . . . without arguing about their relevance [*i.e.* of Navier-Stokes equations] to physics and more specifically to the study of turbulence, I ought to confess we can forget about them here.[104]

This attitude had the advantage of being potentially applicable to other cases of dissipative systems, in particular to some oscillating chemical systems (the so-called Belousov-Zhabotinski reaction), which Ruelle explored in a following article.[105] Characteristic of the modeling practices of applied topologists, it would

---

[102] D. Ruelle and F. Takens, "On the Nature," 168. Italics are original; underlined words are phrases are emphasized by me. In 1971, Ruelle wrote: "The possible generic types of asymptotic behavior of . . . vector fields have not been completely classified. It seems however that, apart from attracting critical points and attracting closed orbits, the behavior described [as follows] is typical: complicated and apparently erratic with sensitive dependence on initial conditions." See "Strange Attractors as a Mathematical Explanation," 294.
[103] D. Ruelle and F. Takens, "On the Nature," 176.
[104] M. G. Velarde, "Steady-States, Limit Cycles and the Onset of Turbulence. A Few Model Calculations and Exercises," *Nonlinear Phenomena,* ed. T. Riste: 205-247, 210. My emphasis.
[105] D. Ruelle and F. Takens, "On the Nature," 176; see D. Ruelle, "Some Comments on Chemical Oscillations," *Transactions of the New York Academy of Sciences,* series II, 35 (1973): 66-71; repr. *TSAC,* 109-115. On these chemical systems, see A. T. Winfree, "Rotating Chemical Reactions," *Scientific American* (June 1974): 82-95;

exert an important attraction for the new alternative in the modeling practice of physicists as promoted by Ruelle during the following decade.

As the bottom line, Ruelle and Takens proposed a new definition of turbulence. This was how they exploited the mathematical results derived from their topologization of the problem of turbulence. The mathematical theory grounding it remained shaky. As late as 1981, an early supporter of their model had to acknowledge that:

> in spite of its mathematical character, the Ruelle-Takens approach is still mathematically speculative in the sense that it is based on some concrete conjectures about the Navier-Stokes equations, conjectures which are so far supported only by indirect evidence, not by any solid and precise analysis of the equations themselves.[106]

Indeed remaining very mathematical in its outlook, Ruelle and Takens's paper ended up relying on numerical and experimental results before it became acceptable to a majority of physicists.

In conclusion, one should note that Ruelle and Takens's model exhibited features which, given the right conditions, might be observable. They proposed nothing less than a redefinition of turbulence, based on a rigorous mathematical property of the solution: *aperiodicity*. This property would be a bit tricky to detect in a noisy experimental situation. Nonetheless, in his subsequent paper dealing with chemical oscillations, Ruelle offered a precise direction for experimental research on the onset of turbulence. In this picture, only a limited number of frequencies should

---

Blair Johnson, *Nonlinearity, Irreducibility, and Emergent Properties: A Short History,* Senior Thesis (Princeton University, 1994).

appear before the power spectrum became continuous as a consequence of aperiodicity. Experimenters only had to search for this kind of continuous spectra. Finally, Ruelle and Takens's model, if correct, would exhibit an extreme "sensitiveness to initial conditions," which that should be detectable experimentally, as well as analytically from the Navier-Stokes equations.[107] This property was not shared by Landau's theory of turbulence.

> Landau's turbulence, . . . people say, is inadequate to account for experimental data. Thus the usual dogma in the physicists' community that chaos or turbulence arises either from interaction of an infinite number of degrees of freedom or from an external noise . . . is just over![108]

But this had to await confirmations based on numerical calculations and experiments in the lab (Chapter VIII).

4. DAVID RUELLE, THE 'MONSTER': THE CAREER OF A MATHEMATICAL PHYSICIST

Albeit a permanent professor at the Institut des hautes études scientifiques, David Ruelle could well have not attended Thom and Smale's seminars. He might not have perceived their interest as far as theoretical physics was concerned. As explained above, three main strains of research lay behind Ruelle and Takens's paper: the quasiperiodic picture of turbulence (Hopf and Landau), dynamical systems theory (Smale and Thom), and mathematical physics. This section addresses the third of these strains by focusing on Ruelle's earlier career. A closer look at his mathematical

---

[106] O. E. Lanford, "Strange Attractors and Turbulence" in *Hydrodynamic Instabilities and the Transition to Turbulence,* ed. H. L. Swinney and J. P. Gollub (Berlin: Springer, 1981): 7-24, 7.
[107] This phrase is first used by D. Ruelle in "Some Comments," 70; *TSAC,* 114.
[108] M. G. Velarde, "Steady States," 208.

physics, and especially the circumstances which led to his hiring at the IHÉS in 1963, clarify why he was in a position both to see the relevance of Thom's and Smale's ideas and to adapt them for the specific concerns of theoretical physicists.

a) Still Another Mathematical Physicist?

It has been said that mathematical physics, after a glorious time in early twentieth-century, notably with Poincaré, had disappeared, leaving theoretical physics in its place. This is incorrect. Mathematical physics enjoyed rather happy years in the 1950s and 1960s. In particular, this period saw, under the principal impulse of Arthur S. Wightman, a considerable development of constructive or axiomatic quantum field theory, using Laurent Schwartz's distributions. This highly mathematical branch of theoretical physics was also importantly developed in Europe, especially around Res Jost, professor at the *Eidgenössische Technische Hochschule* (ETH) in Zürich.[109]

It was from this domain that David Ruelle came. Born in 1935, he received a doctorate from the Université libre of Brussels, but mainly working under the direction of Jost. Ruelle then spent two years at Zürich, before leaving for the Institute for Advanced Study (IAS) at Princeton for another two years (1962-64). During his stay in the US, he started working on statistical mechanics, trying to take advantage of

[109] In 1982, Ruelle expressed his admiration for his mentors as such: "The relation between physics—real physics—and mathematics—real mathematics—has not been as easy one in the last thirty years. It took vision to see that this relation is possible and fruitful now. . . . Res Jost in Zürich, Freeman Dyson and Arthur Wightman in Princeton had that vision, and made many other share it." D. Ruelle, "Large Volume Limit of the Distribution of Characteristic Exponents in Turbulence," *Communications in Mathematical Physics,* 87 (1982): 287-302, 287; *TSAC,* 295-310, 295.

the sophisticated mathematical techniques used in axiomatic quantum field theory in order to establish rigorously some general results.

But Jost and Wightman were among the physicists who visited the IHÉS soon after its foundation, in 1959. In October 1961, a handwritten note, added to an invitation list discussed during a Scientific Committee of the IHÉS (which included Montel, Oppenheimer, Motchane, and the future permanent professor of theoretical physics Louis Michel), bore Ruelle's name. Following a suggestion of Jost's, it was proposed that he should be invited.[110] But Ruelle did not come then, no doubt preferring to go to Princeton.

On his more or less yearly pilgrimage to the United States, during which Léon Motchane never failed to go to Princeton to visit the director of the IAS Robert Oppenheimer, who was among the founding members of the IHÉS. During his trip in 1962, Motchane discussed with Ruelle the possibility of attracting him to Bures-sur-Yvette.[111]

His colleagues' opinion concerning Ruelle were laudatory: Wightman, C. N. Yang and Jost (who wanted to keep him at Zürich), all strongly recommended him. Motchane thus envisioned the creation of a permanent professorship for him at the IHÉS. He was young and dynamic; he knew well American and European researchers in his field; he might be just what was needed for developing the IHÉS. The only shadow, Motchane explained, was that he was a bit too mathematical in his approach. "Obviously, he is a theoretician of a mathematical type, but very recently, he did some

---

[110] *Comité scientifique* (17/10/61). Arch. IHÉS.
[111] Lettre de Léon Motchane à David Ruelle, à Princeton (2/5/63). Arch. IHÉS.

work on statistical mechanics, which indicates a great variety of interests."[112] To Oppenheimer, Motchane wrote:"[Ruelle's] somewhat formal, mathematical orientation obviously fits with the atmosphere reigning here, but one could wonder whether this specialization of our Physics Section in a single direction is a good thing."[113]

Two points are important: Ruelle had just started to work in a field that was new to him, and he did mathematical physics. We shall see below that these two points probably are not without relation to one another. At this time, foreign physicists who belonged to the Scientific Committee of the IHÉS distrusted the increasing specialization of the Institute solely in mathematical physics. When in 1965 the opportunity presented itself to hire Vladimir Glaser as a permanent professor, Oppenheimer voiced his concerns: "the faculty of your Institute should have at least one, and preferably more than one, physicist concerned with the actualities of present experimental exploration of fundamental physical problems." Motchane took notice that according to Oppenheimer, Peierls and Weisskopf, "in theoretical physics, the physics should be forgotten."[114]

Moreover, to hire Ruelle at that time—he was 28 years old—was a risk. Jost, his ex-mentor testified to this ten years later: "David Ruelle was very young, and working in a new field, and above everything with tools of such penetrating rigour that he was once likened by a famous physicist not to a fellow theoretician but to a

[112] Lettre de Léon Motchane à Victor Weisskopf (24/3/63). Arch. IHÉS.
[113] Lettre de Léon Motchane à Robert Oppenheimer (27/7/63). Arch. IHÉS.

monster [*sic*]."[115] English theoretical physicist Rudolph Peierls, just coopted in 1963 into the Scientific Committee of the IHÉS, and who hardly knew mathematical physics, stated:

> I am certainly familiar with the reputation of Dr. Ruelle, though I have not met him personally and have not had an occasion to study his paper in any details. . . . [H]is papers are not very easy to read. . . . Ruelle is concerned with the rather abstract and formal side of [theoretical physics] and this applies also to some extent to Lehmann and to Michel [the other two physics permanent professors].[116]

But Ruelle strongly impressed those among his elders who fathomed his work. Weisskopf "very strongly" commended him to Motchane. "To my mind, Ruelle is just the right man for you and you should go all out to get him."[117] Res Jost pressed Motchane to do all in his powers to bring Ruelle back to Europe, the more so since Ruelle had caught Princeton's interest.[118] When Motchane asked for the advice of the director of the IAS, Oppenheimer's secretary replied that he and his colleagues thought "highly enough of Ruelle to be considering him for a professorship here."[119] This could not fail to alarm Motchane: "I want at all price to avoid all that could be taken for competition between our Institutes."[120]

Finally, although Oppenheimer and Ruelle had inded discussed the possibility for the latter to remain at Princeton, Ruelle finally decided to accept Motchane's offer

---

114 Lettres de Léon Motchane à Robert Oppenheimer (6/5/65); de Robert Oppenheimer à Léon Motchane (20/5/1965); de Léon Motchane à Robert Oppenheimer (15/3/65). Arch. IHÉS.
115 Lettre de Res Jost à Nicolaas Kuiper (18/1/74). Arch. IHÉS.
116 Lettre de Rudolph Peierls à Léon Motchane (6/7/63); de Léon Motchane à Rudolph Peierls (26/6/63). Arch. IHÉS.
117 Lettre de Victor Weisskopf à Léon Motchane (27/6/63). Arch. IHÉS.
118 Lettre de Res Jost à Léon Motchane (20/6/63). Arch. IHÉS.
119 Lettre de Verna Hobson à Léon Motchane (9/7/63). Arch. IHÉS.

in September 1963. He joined the IHÉS in October 1964. In accepting his offer, Ruelle warned Motchane that although he was working in a new field, statistical mechanics, "I maintain a great interest in field theory to which I intend to come back." [121] But he never did.

b) Ruelle, Statistical Physics, and the Military

As soon as he arrived at the IHÉS, Ruelle wrote a research proposal for the *Direction des Recherches et Moyens d'Études* (DRME), the Gaullian organization which sponsored most defense-related research in France. Following the defection of several important subscribers, the IHÉS faced major financial hardships which jeopardized its very survival.[122] The Ruelle contract would have furnished a non-negligible amount of 350,000 F for two years, or about 10% of the budget of the Institut, allowing Motchane to breath a bit easier.[123] The proposal was titled "Convergence Theorems and the Existence of Phase Transitions in Statistical Mechanics." We thus see that Ruelle was well engaged in statistical mechanics. But his approach was special. Indeed, as Motchane wrote:

> by modern analytic techniques (functional spaces, Banach spaces), he establishes convergence theorems for different thermodynamic functions. . . .

---

[120] Lettre de Léon Motchane à Robert Oppenheimer (27/7/63).
[121] Lettres de Léon Motchane à David Ruelle (2/5/63); de David Ruelle à Léon Motchane (15/5/63); de Léon Motchane à David Ruelle (10/9/63), de David Ruelle à Léon Motchane (16/9/63). Arch. IHÉS.
[122] Notes de séances manuscrites de l'Assemblée générale (23/9/64); projet de lettre (non-envoyée) de Léon Motchane à Pierre Chatenet (18/12/64); lettre de Léon Motchane à Frank Bowles (5/2/65). Arch. IHÉS. See Chapter IV above.
[123] Rapport du Conseil d'administration à l'Assemblée générale (23/9/64); lettre de Léon Motchane à Lucien Malavard (17/11/64); de Léon Motchane à Pierre Aigrain (8/12/64). Arch. IHÉS.

> An original aspect of RUELLE's approach on these problems of statistical mechanics was to show the analogy of his formalism with the one of axiomatic quantum field theory.[124]

Ultimately, albeit initially accepted by Pierre Aigrain who then was scientific advisor for the DRME, Ruelle's research proposal was rejected. It was rejected because it did not fit the normal work sponsored by the DRME ["*la ligne de travail normale de la DRME*"].[125] The IHÉS intended to do "rigorously pure research sponsored in a perfectly disinterested spirit [*esprit de mécénat purement désintéressé*]." Ruelle and Michel were ready to provide arguments to justify military interest in these researches, but, ultimately—especially since discussions with the Prime Minister's Cabinet about a direct sponsorship from the Government were turning out favorably—the directorate of the IHÉS decided to "show extreme intransigence about the principle."[126]

c) The Structure of Physical Theories: The Bourbakization of Physics?

Ruelle's work in statistical mechanics would nevertheless prove truly exceptional. According to Jost, "David Ruelle for the first time—one hundred years after Ludwig Boltzmann and about 70 years after Willard Gibbs—finally laid down the mathematical foundations of statistical mechanics."[127] In 1968, Ruelle would collect his results in a book called *Statistical Physics: Rigorous Results.* In the introduction,

---

[124] Lettre de Léon Motchane à Lucien Malavard (17/11/64). Arch. IHÉS.
[125] Lettre de Annie Rolland à Léon Motchane (19/1/65). Cf. également Note manuscrite de Annie Rolland suite à un coup de téléphone de André Grandpierre (12/1/65), et lettre du Général Lavaud au Général René Cogny (15/5/62): La DRME "s'interdit toute action qui pourrait l'assimiler à un mécénat." Arch. IHÉS.
[126] Note manuscrite de Annie Rolland suite à un coup de téléphone de André Grandpierre (12/1/65). Arch. IHÉS.

Ruelle clearly expressed his deep belief in what a rigorous mathematical approach brought to the theoretical study of physics. It was a "rewarding experience," he wrote. "[M]athematical analysis gives to the physical world a new structure and meaning. The knowledge of this structure and meaning constitutes an understanding of the 'nature of things' as deep as we can hope to be." Several domains of physics were then (1968) very interesting and open to "insightful" mathematical treatment, Ruelle contended, even while repudiating his origins: "An exception to this statement may be relativistic quantum mechanics, largely because of 'overgrazing,' but there are also vast areas of *terra incognita.*" He expressed his admiration for the Bourbakist work.

> The progress of mathematical physics could be significantly promoted, in the author's opinion, by the availability of results of important mathematical theories in concise form and without proofs, in the spirit of Bourbaki's 'Fascicules de Résultats'.[128]

These reflections offer us a means to grasp some of the reasons why Ruelle, other than because of his great personal talent, could successfully jump laterally from one field to another. "I like to change rather often my centers of interest. But one can find in all my works a constant feature: the striving for mathematical rigor in the exposition of physical theories."[129]

In the late 1960s rigor often served as a synonym for a Bourbakist attitude. In other words, Ruelle's emphasis on rigor should be interpreted as an indication that a structural approach informed his modeling practice on physics. We may well here

---

127 Lettre de Res Jost à Nicolaas Kuiper (18/1/74). Arch. IHÉS.
128 David Ruelle, *Statistical Physics: Rigorous Results* (New York: W. A. Benjamin, 1969), vii-viii.
129 "La nature de la turbulence. Une interview de David Ruelle," *CNRS-Info,* special issue "L'École françaises du chaos," n.d [1989], 12-13.

recall Motchane's contemporary pronouncements (Chapter VI), when he contended that the kinship of structures in extremely diverse domains allowed the mathematician, without becoming an expert, to grasp the essential features of the scientific domains he invested.[130] As mentioned in Chapter IV, this structuralist attitude could be seen as a fundamental character of the ideology pushed forward by the Institut des hautes études scientifiques, both in its structure and function.

The Bourbakist reordering of mathematics, and above all the emphasis put on the concept of a mathematical structure, much more than the actual concept as such, was supposed to allow the mathematician or the mathematical physicist, without becoming an expert in a foreign field, to grasp some of its deep structures, its essence. In the best cases, this should set the ground for a fruitful dialogue with the experts. In Ruelle's case, as opposed to most of the applications of catastrophe theory suggested by Thom and Zeeman, this dialogue took place. Even for physicists, Bourbaki's structures were thus an important cultural connector. I will return to this issue below

5. A LONG-TERM DISCIPLINARY SURVEY OF THE TURBULENCE PROBLEM

From the point of view of the historian of science, turbulence represents a challenge, and this might be the reason why secondary literature has remained extremely scarce on this topic.[131] Because of this scarcity of secondary sources, we need very cursorily

---

[130] Léon Motchane, "Éléments de Rapports scientifique [1967] à l'Assemblée [générale du 8/5/68]," 4. Arch. IHÉS.

[131] I have only been able to find one somewhat recent monograph devoted to the history fluid mechanics, traditionally included in histories of rational mechanics: G. A Tokaty, *History and Philosophy of Fluidmechanics*. Significantly, this book was