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

to trace back the evolution of modeling practices at play in the history of fluid mechanics, and especially when trying to relate observed turbulent phenomena with the Navier-Stokes equations, which was called the "turbulence problem."[132]

Manifestly, progress in the history of turbulence has been nonlinear. The methods that have been attempted in order to relate the observed phenomenon of turbulence with the Navier-Stokes equations have been so diverse that conceptual unity has been impossible to achieve. New approaches and new means of computation regularly opened new avenues of research. Methods for dealing with the turbulence problem involved the theories of partial differential and integral equations, functional analysis, qualitative dynamics, statistical theories, energy methods, ergodic theory, numerical computations, empirical identification of coherent structures, finite element analysis, etc. Regularly, these methods proved disappointing for those in search of general theories of turbulence.[133]

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written by a practicing scientist. However, see also Marcel Nordon, *Histoire de l'hydraulique. 2. L'eau démontrée* (Paris: Masson, 1992).

132 A noteworthy exception to the lack of attention historians have manifested towards fluid mechanics in the twentieth century is provided by G. Battimelli, "The Mathematician and the Engineer: Statistical Theories of Turbulence in the 20's," *Rivista di storia della scienza,* 1 (1984): 73-94.

133 Some of the most historically-minded articles used for this section are the following: P. Appel, et al., "Développements concernant l'hydrodynamique"; J. L. Synge, "Hydrodynamical Stability," *Semicentennial Addresses of the American Mathematical Society* (New York: AMS, 1938): 227-269; J. von Neumann, "Recent Theories of Turbulence," unpubl. report to the ONR (1949); repr. *Collected Works,* 6, ed. A. H. Taub (Oxford: Pergamon, 1963): 437-472; S. Goldstein, "Fluid Mechanics in the First Half of This Century," *Annual Review of Fluid Mechanics,* 1 (1969): 1-28. H. W. Liepmann, "The Rise and Fall of Ideas in Turbulence," *American Scientist,* 67 (1979): 221-228; and G. T. Chapman and M. Tobak, "Observations, Theoretical Ideas, and Modeling of Turbulent Flows—Past, Present, and Future," *Theoretical Approaches to Turbulence,* ed. D. L. Dwoyer, et al. (New York: Springer, 1985): 19-49.

In late nineteenth century hydrodynamics was perceived as being part of rational mechanics and closely linked with applied mathematics. It then opened to a wide array of concerns, attracting the attention of physicists and engineers especially. Practical problems raised by the development of air flight only furthered this trend. More and more, fluid mechanics became a subject studied by many communities with diverging goals. Unifying these different branches, embodying distinct modeling practices, posed a difficult conceptual problem which confronted several generations of students of turbulence.

In 1969, summing up a half-century of work on fluid mechanics, Sydney Goldstein, from Harvard University, could not help noticing the dispersion of his field:

> Fluid mechanics is a part of applied mathematics, of physics, of many branches of engineering, certainly civil, mechanical, and aeronautical engineering, and of naval architecture, and geophysics, with astrophysics and biological and physiological fluid dynamics to be added.[134]

Similarly, George K. Batchelor recalled that when founding the *Journal of Fluid Mechanics* in 1956, he felt that there was "a three-way split of literature on fluid mechanics" among theoretical and mathematical papers, experimental and observational papers, and those dealing with application. In 1981, he contended that this journal, meant to bridge the gap, had only been partly successful in doing so.[135]

Given the variegated nature of the field, what is presented here can only serve as a rough—and very partial—survey of the turbulence problem. My goal is to

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[134] S. Goldstein, "Fluid Mechanics," 4.
[135] G. K. Batchelor, "Preoccupation of a Journal Editor," *Journal of Fluid Mechanics,* 106 (1981): 1-25.

understand the conceptual setting in which the model suggested by Ruelle and Takens could insert itself and underscore the changes in modeling practices that it represented.

a) Fluids Are Described by the Navier-Stokes Equations

In order to evaluate the radical changes in modeling practices represented by the Ruelle-Takens model, with its skepticism towards the ontological status of the Navier-Stokes equations, how the turbulence problem constantly cast doubts on this fundamental law is briefly reviewed. The aim is twofold. First, in a somewhat cavalier fashion, history serves as an introduction of essential concepts of fluid mechanics, such as the Navier-Stokes equations and Reynolds numbers. Second, and most importantly, a sense of the always shifting nature of the modeling practices in hydrodynamics is briefly conveyed. The changing ontology of fluids will be seen to go hand in hand with the modeling practices available to the specialists.

(i) Euler's Equations

Around 1755, Leonhard Euler (1707-1783) contended:

> However sublime are the researches on fluids which we owe to the Messrs. Bernouilli, Clairaut, and d'Alembert, they flow so naturally from my two general formulae that one cannot sufficiently admire this accord of their profound meditations with the simplicity of the principles from which I have drawn my two equations, and to which I was led immediately by the axioms of mechanics.[136]

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[136] L. Euler, "Principes généraux de l'état d'équilibre des fluides"; "Principes généraux du mouvement des fluides"; and "Continuation des recherches sur la théorie des mouvements des fluides," *Opera Omnia,* 2nd ser., vol. XII (Lausanne: Orell Füssli Turici, 1954): 2-132; first published in 1757. The above quote was translated by A. P.

Euler is generally taken to have been the first to analyze the dynamics of fluids by applying Newton's second law to the study of flows. He wrote down a simple set of differential equations that admittedly governed the flow of a fluid in terms of its density and velocity field, an impressive reduction of prior disparate results to the laws of mechanics.

"By this discovery," Joseph Lagrange later wrote, "*all fluid mechanics was reduced to a single point of analysis,* and if the equations involved were integrable, one could determine completely, in all cases, the motion of a fluid moved by any force."[137] Following Lagrange, one might be inclined to believe that the problem was solved. Unfortunately, Euler's equations were nonlinear, and indeed have turned out to be quite difficult to solve analytically up to this very day. Lagrange was well aware of these difficulties as he immediately added: "unfortunately [Euler's equations] are so rebellious that up until now they have only been solved for very limited cases." Similarly, Euler had already been forced to admit: "If we cannot achieve complete knowledge of fluid motions, it is not to mechanics and to the insufficiency of the known principles that we should ascribe the cause, but analysis itself here fails us."[138]

In retrospect, this difficulty in using Euler's equations to account for observations stemmed from two sources: the intractability of the nonlinear equations

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Yonschkevitch, *s.v.* Euler, *Dictionary of Scientific Biography,* ed. C. C. Gillespie, 4 (New York: Charles Scribner's Sons, 1978), 481.

[137] J. Lagrange, *Mécanique analytique* (Paris, 1788; repr. Paris: Jacques Gabay, 1989), sec. X, 436. My translation and emphasis.

[138] Quoted in P. Costabel, "La mécanique des milieux continus," *Histoire générale des sciences,* ed. R. Taton, tome III, vol. I, 2nd ed. (Paris: PUF, 1981), 99.

and the neglect of an important characteristics of real fluids. As G. A. Tokaty, one of the rare historians of fluid mechanics, remarked in series of mixed metaphors:

> Leonhard Euler was not a contributor to, but the founder of, Fluidmechanics [*sic*], its mathematical architect, its great river. . . . But the beautiful trousers he tailored had no buttons, they failed to include viscosity. The buttons were provided by Claude Navier.[139]

(ii) Navier and the Molecular Hypothesis

Indeed, Euler's assumptions, basically equivalent to neglecting friction inside the fluid, eventually turned out to be too crude to provide a realistic description of most liquids. In 1822, Claude Louis Navier (1785-1836) modified Euler's equations by introducing a dissipative effect, later called *viscosity* (denoted ν below).[140]

> The considerable or total differences that, in some cases, the natural effects present with respect to the results of known theories [Euler's], show the necessity . . . of taking into account certain molecular actions that principally manifest themselves in phenomena of motion.[141]

Navier thus hypothesized that a force existed between fluid molecules that was proportional to their velocity relative to one another. "In a fluid in motion, two molecules approaching one another repel one another more strongly, and . . . two molecules that get further apart repel one another less strongly than they would if their actual distance remained constant."[142]

From this basis, Navier derived the laws of motion for the particles of an incompressible fluid (i.e. with constant density), which he expressed as a set of three

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[139] G. A. Tokaty, *A History of Fluidmechanics,* 73 and 88.
[140] Claude Louis Navier, "Mémoire sur les lois du mouvement des fluides," *Mémoires de l'Académie des sciences,* 6 (1823): 389-440.
[141] C. Navier, "Mémoire sur les lois du mouvement," 389.
[142] C. Navier, "Mémoire sur les lois du mouvement," 391.

differential equations. In a modern vectorial form, these can be reduced, for incompressible fluids, to the following single expression:

$$\frac{\partial \mathbf{v}}{\partial t} + (\mathbf{v} \cdot \nabla)\mathbf{v} = -\frac{1}{\rho}\operatorname{grad} p + \nu\,\Delta\mathbf{v};$$

where $\mathbf{v}=(v_x,v_y,v_z)$ represents the three spatial directions of the velocity of fluid particles at each point; $p(x,y,z)$ is pressure, and ρ is density, assumed constant. Together with the continuity equation ($\operatorname{div} \mathbf{v} = 0$, in the case of an incompressible fluid), found by Euler and unmodified by Navier, the description would become widely known as the Navier-Stokes equations. Since then, they have been the basis of every theoretical description of fluids.

A significant difference existed between the method Navier used to derive this equation and Euler's analysis. In conformity with the principles of the Laplacian school of physics, Navier considered the forces acting on a single molecule in the fluid and derived the equations of motion for these molecules. On the other hand, Euler made no hypothesis as to the composition of the fluid, and based his consideration solely on the average speed in small elements.[143] The certainty of Navier's equations therefore hinged on the acceptance a particular hypothesis concerning the nature of intermolecular forces. In the late 1820s, a fierce debate, concerning the then closely related topic of elasticity in solids, rooted Navier against Siméon Denis Poisson (1781-1840), a close follower of Laplace, who had also derived

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[143] R. Fox, "The Rise and Fall of Laplacian Physics," *Historical Studies of Physical Sciences,* 4 (1974): 89-136. See also C. C. Gillespie, R. Fox, and I. Grattan-Guiness, *s.v.* Laplace, *Dictionary of Scientific Biography,* 15, Suppl. 1 (New York: Charles Scribner's Sons, 1978): 273-403; and A. Dahan Dalmedico, *Mathématisations.*

a similar set of equations starting from different hypotheses.[144] Navier's proposal was indeed somewhat heterodox for Poisson for two reasons. What mattered for Navier was not the exact form of the forces between molecules, but rather those arising when equilibrium was disturbed, assuming that they canceled each other at rest. Furthermore, in Navier's scheme, the force between molecules depended on their relative velocity, rather than their position.

This debate highlights the fact that the assumptions made by Navier involved a certain degree of arbitrariness. "Poisson never seemed content with purely mathematical models as description of the underlying physics. He wanted to provide explanations, not descriptions."[145] For Navier, molecules hardly represented more than material points—centers of attractive and repulsive forces suitable for calculations—which allowed him to bypass some of the problems plaguing the Laplacian school. Indeed, by insisting on always having attractive actions between molecules, the progress of this school's program was hindered by infinite densities. Therefore, Navier's assumptions were already one step away from a specific reliance

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[144] C. Navier, "Note relative à l'article intitulé: 'Mémoire sur l'équilibre et le mouvement des corps élastique', page 337 du tome précédent," *Annales de chimie et de physique,* 2e sér., 38 (1828): 304-314; S.-D. Poisson, "Réponse à une note de M. Navier insérée dans le dernier cahier de ce journal," *Ibid.,* 435-440; C. Navier, "Remarques sur l'article de M. Poisson, insérée dans le cahier d'août, page 435," *Ibid.,* 39 (1828): 145-151; S.-D. Poisson, "Lettre à M. Arago," *Ibid.,* 204-211; and C. Navier, "Lettre à M. Arago,"*Ibid.,* 40 (1829): 99-107. C. Navier, "Postface au débat avec Poisson," *Bulletin des sciences mathématiques de Férussac,* 11 (1829): 243-253. On the Navier-Poisson debate, see A. Dahan Dalmedico, *Mathématisations,* 266-273.
[145] D. H. Arnold, "Poisson and mechanics," in *Siméon-Denis Poisson et la science de son temps,* ed. M. Métivier, P. Costabel, and P. Dugac (Palaiseau: École polytechnique, 1981): 23-37, 35.

on the nature of such forces, since only variations away from equilibrium mattered, and not the equilibrium itself.

(iii) Stokes: The Robustness of Partial Differential Equations

In fact, English physicist George Gabriel Stokes eventually published in 1845 an article showing that molecular assumptions were unnecessary in order to derive Navier's equations.[146] Already on November 27, 1843, Adhémar Barré de Saint-Venant (1797-1886) had also read a note at the Académie des sciences in which he derived Navier's equations "without making suppositions about the magnitude of attractions and repulsions between molecules as a function of either their distances or their relative speed."[147] With this step, the Navier-Stokes equations became more reliable than any specific assumption concerning molecular forces. Indeed the very molecular hypothesis could be done away with.[148] In a manner that recalls Fourier's treatment of heat, the fundamental tool for the description of fluids became the differential equation, rather than specific suppositions about ultimate constituents of fluids and interactions between them.

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[146] G. G. Stokes, "On the Theory of the Internal Friction of Fluids in Motion, and of the Equilibrium and Motion of Elastic Solids," *Transactions of the Cambridge Philosophical Society,* 8 (1845): 245; repr. *Mathematical and Physical Papers,* 1 (Cambridge, 1880; New York: Johnson Reprints, 1966): 75-129. See A. Dahan Dalmedico, *Mathématisations,* 291-294, 429-430; and C. Smith and M. N. Wise, *Energy and Empire,* chap. 4.

[147] "sans faire de suppositions sur la grandeur des attractions et répulsions des molécules en fonction, soit de leurs distances, soit de leurs vitesses relatives (1240)." A. B. de Saint Venant, "Note à joindre au Mémoire sur la dynamique des fluides, présenté le 14 avril 1834," *CRAS,* 17 (1843): 1240-1243.

[148] It was G. G. Stokes who noted that Saint-Venant's "method does not require the consideration of ultimate molecules at all." G. G. Stokes, "Report on Recent

However, neither was Stokes's treatment free from assumptions. Stokes's was a geometrical derivation that assumed that Navier's equations needed, in the words of Osborne Reynolds, "to involve no other assumption than that the stresses, other than that of pressure uniform in all directions, are linear functions of the rates of distortion."[149] In other words, Stokes made an hypothesis of a "daring simplicity" to the effect that internal pressures were directly proportional to the velocities, which could only be valid for small velocities. "Hence although [the Navier Stokes equations] may apply with great accuracy to cases of slow motion, we have no assurance of their validity in other cases."[150] Therefore, the possibility existed that his derivation of the Navier-Stokes equations might not be valid for 'turbulent' motions, which admittedly involved large velocities for the fluid.

The name of the game then became solving the Navier-Stokes equations, together with the continuity equation (div v = 0) and boundary and initial conditions—solutions which would provide the exact time evolution of fluid flows. However, the Navier-Stokes equations maintained the nonlinear character of Euler's

Researches in Hydrodynamics," *Reports for the British Association for the Advancement of Science* (1846), Part I; repr. *Papers,* 1: 157-187, 184.

[149] O. Reynolds, "On the Dynamical Theory of Incompressible Viscous Fluids and the Determination of the Criterion," *Philosophical Transactions of the Royal Society,* A186 (1894): 123-164; repr. *Papers on Mechanical and Physical Subjects,* 2 (Cambridge: Cambridge University Press, 1901): 535-577. See G. G. Stokes, "On the Theories," 88ff.

[150] H. Lamb, *A Treatise on the Mathematical Theory of the Motion of Fluids* (Cambridge: Cambridge University Press, 1879), 221. On later grounds to believe this hypothesis, see H. Lamb, *Hydrodynamics,* 575. J. L. Synge emphasized the "daring simplicity" of Stokes's assumption in "Hydrodynamical Stability," 231.

equations, and even more than the latter, they turned out to be extremely unmanageable mathematically, except for a few simple cases.[151]

b) The Turbulence Problem: From Hydraulics to Physics

Turbulence was *not* simple, not from the standpoint of the Navier-Stokes equations. To show their distress in face of this formidable problem, fluid dynamicists liked to cite Horace Lamb's comparison, the epigram of this chapter, between quantum electrodynamics and the turbulent motion of fluids. "About the former I am rather optimistic," Lamb remarked.[152] And he was right. While QED was solved by Feynman, Schwinger, Dyson and Tomonaga in the 1950s, while particle physics and quantum field theory witnessed impressive advances in the following decades, barely a dent was made in the problem of turbulence, despite considerable efforts. "Hydrodynamics," Ruelle contended in 1981, has "remained somewhat in the backwaters of the scientific storm of this century."[153]

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[151] It was Arnold Sommerfeld who, at the Rome International Congress of Mathematicians in 1908, insisted on the nonlinarity of the Navier-Stokes equation as being the source of the difficulty in interpreting theoretically turbulent phenomena: "Ein Betrag zur hydrodynamischen Erklärung der turbulenten Flüssigkeitsbewegungen," *Atti del quarto congresso internazionale dei matematici in Roma 1908,* ed. G. Castelnuovo, 3 (Rome, Accademia dei lincei, 1909): 116-124, 118.
[152] Quoted by S. Goldstein, "Fluid Mechanics," 23. Also in Paul C. Martin, "The Onset of Turbulence: A Review of Recent Developments in Theory and Experiment," *Statistical Physics: Proceedings of the International Conference [Budapest, August 1975],* ed. L. Pál and P. Szépfalusy (Amsterdam: North-Holland, 1976): 69-96.
[153] D. Ruelle, "Differentiable Dynamical Systems and the Problem of Turbulence," *Bulletin of the American Mathematical Society,* 5 (1981): 29-42, 30; repr. *Proceedings of Symposia in Pure Mathematics,* 39(2) (1983): 141-154; *TSAC,* 233-246, 234.

(i) Early Studies of Turbulence: Poiseuille, Darcy, Boussinesq, etc.

The first sign that the fundamental equations written down by Navier, Saint-Venant, and Stokes did not resolve the problem came from practical hydraulic works and experiments. George Stokes had derived his equations in order to deal with the paradigmatic case of the resistance that a solid body opposed to the flow of liquids. There also difficulties linked with 'turbulence' arose, but careful experimental studies of problems of this kind were harder to perform.[154] As for pipes, since the work of Navier in 1838, the accepted law stated that the square of the resistance R of water flowing in pipes was proportional to the mean velocity U ($R^2 \propto U$).[155]

An ex-Polytechnician and a medical doctor teaching physics at the Faculté de Médecine of the Sorbonne, Jean-Louis Poiseuille (1799-1869) undertook, in the 1840s, to test experimentally this law of Navier's for reasons that had to do with the study of blood flows in capillary veins. He had several very narrow glass tubes built (of diameters from .013 mm to .65 mm) and studied the resistance they opposed to water flows. These experiments appeared to challenge Navier's law, since they showed that resistance was directly proportional to velocity ($R \propto U$).[156] Poiseuille thought that his experiment cast doubts on Navier's whole approach.

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[154] Experiments dealing with the resistance opposed to ship motions always were plentiful. In the interwar period, many likewise investigated this problem in relation with wings of airplanes.
[155] C. Navier, *Leçons à l'École des ponts et chaussées* (Paris, 1838), no. 108.
[156] J.-L. Poiseuille, "Recherches expérimentales sur les mouvements des liquides dans les tubes de très petits diamètres," *CRAS,* 11 (1840): 961-967; 1041-1048; 12 (1841): 112-115; ""Recherches expérimentales sur le mouvement des liquides dans les tubes de très-petits diamètres," *Mémoires des savants étrangers,* 9 (1846); V. Regnault, et

To meet this challenge, several propositions were introduced to modify the Navier-Stokes equations in order to find the correct fundamental law.[157] But by the 1860s, Émile Mathieu and Joseph Boussinesq proved that Navier's resistance law was "*de facto* independent from all parts of his memoir relative to internal friction," i.e. from the Navier-Stokes equations.[158] For Saint-Venant, Poiseuille's experiments became a severe restriction imposed on the equations of hydrodynamics: there was "no need to adopt formulae of so strange a complication."[159] In fact, Poiseuille's experiments were turned into a crucial confirmation of Stokes linear hypothesis and the Navier-Stokes equations for a wide range of cases and were used to determine the viscosity of water as a function of temperature.[160] Indeed, the Navier-Stokes equations had successfully stood up to the challenge.

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al., "Rapport sur un Mémoire de M. le docteur Poiseuille, etc.," *CRAS,* 15 (1842): 1167-1186.

157 See A. B. de Saint-Venant, "Sur l'hydrodynamique des cours d'eau," *CRAS,* 74 (1872): 570-577; 649-657; 693-701; and 770-774, esp. pp. 655-657. Reports of the Academy of Sciences were published in Combes, et al., "Rapport sur un Mémoire de M. Maurice Lévy, relatif à l'hydrodynamique des liquides homogènes, particulièrement à leur écoulement rectiligne et permanent," *CRAS,* 68 (1869): 582-588; and A. B. Saint-Venant, Delaunay, and J. Bertrand, "Rapport sur un Mémoire de M. Kleitz, intitulé Études sur les forces moléculaires dans les liquides en mouvement, et applications à l'hydrodynamique," *CRAS,* 74 (1872): 426-438, 430. Kleitz's memoir was not recommend for publication.

158 A. B. de Saint-Venant, "Sur l'hydrodynamique des cours d'eau," 577. É. Mathieu, "Sur le mouvement des liquides dans les tubes de très-petits diamètres," *CRAS,* 57 (1863): 320-324; J. Boussinesq, "Mémoire sur l'influence des frottements dans les mouvements réguliers des fluides," *Journal de mathématiques pures et appliquées,* 13 (1868): 377-424; and Serret, O. Bonnet, and A. B. de Saint-Venant, "Rapport sur un Mémoire de M. Boussinesq, présenté le 27 juillet 1868 et relatif à l'influence des frottements dans les mouvements des fluides," *CRAS,* 67 (1868): 287-289.

159 A. B. de Saint-Venant, "Sur l'hydrodynamique des cours d'eau," 697.

160 "On voit donc que les expériences de M. Poiseuille démontre l'exactitude des formules de Navier." J. Boussinesq, "Théorie des phénomènes constatés par les expériences de M. Poiseuille," *CRAS,* 65 (1867): 46-48, 48.

Of course, practical incentive were never lacking for a careful study of water flows in pipes. "There are few branches of physico-mathematical sciences [which are] more important as far as constant application to practic[al matters] than that dealing with the motion of water in pipes."[161] In 1858, a long article was published by Henri Darcy, an *inspecteur général des Ponts et chaussées,* in which he studied the influence of the diameter of water pipes extensively.[162] Darcy also questioned the validity of the assumption that fluid flows remained parallel to the pipes, yet an exact solution of the Navier-Stokes. "*Ruptures,* eddies [*tourbillonnements*] and other complicated or oblique motions, which must greatly influence the intensity of friction, arise and develop more in large sections."[163] Darcy fell back on Poiseuille's results only when he supposed that the diameter of his pipes was small. For the first time, a clear distinction between two types of fluid motion was emphasized.[164]

In 1867-1877, Valentin Joseph Boussinesq (1842-1929) tackled the difficult problem of the flow of water in large pipes. A specialist of this "*intimate* mechanics . . . which is that of actual things of the earthly world, and whose beautiful and noble study is, by and large, more arduous than that of the planetary world," Boussinesq went further than anyone before him in the "beautiful and difficult science" of

[161] D'Aubuisson to F. Arago (1 October 1829); quoted in H. Darcy, "Recherches expérimentales relatives au mouvement de l'eau dans les tuyaux," *Mémoires présentés par divers savants à l'Académie des sciences de l'Institut impérial de France,* (2) 15 (1858): 141-403, 144-145.
[162] H. Darcy, "Recherches expérimentales."
[163] H. Darcy, "Recherches expérimentales," 322.
[164] H. Darcy, "Recherches expérimentales," 215 and 354. "L'opinion que le 'tourbillon' est la base du changement dans la loi de la résistance a déjà été formulée par [Saint-Venant in 1851]; elle a été admise par J. Boussinesq et G. G. Stokes. H. Darcy signala

hydrodynamics.[165] In particular, he carefully studied the experimental results of Poiseuille and Darcy, and tried in 1868 to derive the distinction between the two types of motion in a theoretical fashion, starting from the Navier-Stokes equations. The first sentence of the long memoir he devoted to this topic in 1877 therefore stated as a matter of fact that:

> Fluids move in two different ways, according to whether they flow in very narrow pipes or in spaces with sections comparable to that of large pipes or uncovered canals.[166]

In his memoir on running waters, Boussineq made the radical suggestion, following Saint-Venant, that the viscosity coefficient that entered the Navier-Stokes equations might vary in space and time and with the geometry and size of the sections of the pipe. This suggestion notwithstanding, the flow of water remained, and for long, "a distressing enigma [*énigme désespérante*] against which distinguished spirit stumbled in vain."[167] But one notes that by then, the validity of the Navier-Stokes equations as the adequate basis for the description of turbulent flows had ceased to be questioned.

également ce passage du régime régulier au régime irrégulier." P. Appel et al., "Développements," 199.

[165] Manucript of Saint-Venant's proposition of Boussinesq for a chair at the Academy of Sciences (4 January 1886). Also, J. Boussinesq, *Notice sur les travaux scientifiques de M. J. Boussinesq* (Lille: L. Danel, 1880 and 1883). Arch. AdS.

[166] J. Boussinesq, "Essai sur la théorie des eaux courantes," *Mémoires des savants étrangers,* 23(1) (1877): 1-680; preceded by O. Bonnet, Phillips and A. B. de Saint-Venant, "Rapport sur un Mémoire de M. Boussinesq présenté le 28 octobre 1872 et intitulé 'Essai sur la théorie des eaux courantes'," [publ. in *CRAS,* 76 (1873): 924-943]. See also J. Boussinesq, "Essai théorique sur les lois trouvées expérimentalement par MM. Darcy et Bazin, pour l'écoulement uniforme de l'eau dans les conduites," *CRAS,* 71 (1870): 389-393.

Once again in this chapter, we encounter a fascinating case of interaction between men coming from a variety of backgrounds. Engineers, medical doctors, physicists, and mathematicians, all exchanged theories, experimental results and practices at a node represented by the Academy of Sciences of Paris. Limited by the topic of this dissertation, one can only hope that more historical study will be undertaken on this topic. For the time being, the conclusion which should be drawn from this cursory account of the scientific investigation of water flows in pipes is the following: after half a century of research, the "distressing enigma" was still there, but the conviction that the Navier-Stokes equations represented the basis of any theoretical explanation of the phenomena was considerably reinforced.[168]

(ii) Osborne Reynolds's Experimental Discovery of Turbulence

Systematic study of turbulent flows, however, only began with the British physicist Osborne Reynolds (1842-1912) around 1876.[169] By importing to England the work done on the Continent, by adopting a physical, empirical attitude and merging hydraulic and mathematical studies, Reynolds greatly changed the outlook of the subject. "The English teach mechanics as an experimental science," Poincaré once

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[167] A. B. de Saint-Venant, "Sur l'hydrodynamique," 774; quoted by J. Boussinesq, "Essai sur la théorie des eaux courantes," 6. About Prandtl's use of Boussinesq's suggestion, see G. Battimelli, "The Mathematician and the Engineer," 84-86.
[168] One should note however that boundary conditions (the so-called no-slip boundary condition which was later adopted) were very much still a matter of intense debate. See P. Duhem, *Recherches sur l'hydrodynamique,* 2 (Paris, 1904): 79-95, for a history of boundary conditions for viscous fluid.
[169] Experiments on pipes were also performed by H. Hagen in 18??, but his theoretical interpretation was not convincing and his experiments did not receive much notice at the time. G. H. L. Hagen, *Abhandlungen der Akademie in Berlin* 1854, éd. 1855,

wrote; "on the Continent, it is still exposed more or less as a deductive, *a priori* science. The English are right, this goes without saying."[170]

To start with, Reynolds established on the basis of extensive experimentation that "the internal motion of water assumed one or the other of two broadly distinguishable forms."[171] Furthermore, he identified a single parameter that controlled the behavior of the fluid. His experiments showed that this parameter, later named the *Reynolds number,* possessed a critical value at which the motion changed its form.

Although a most cursory observation of fluid flows could reveal that turbulent motions were, on the face of it, very different from smooth ones, it was Reynolds's achievement to distinguish them unambiguously. "A clear surface of moving water has two appearances," he wrote, "the one like that of a plate of glass, in which objects are reflected without distortion, the other like that of sheet glass, in which the reflected objects appear crumpled up and grimacing."[172] Reynolds assumed that these two characteristics corresponded to two distinct types of motion, which later came to be labeled as *laminar* and *turbulent,* although Reynolds preferred the terms "direct" for the former and "sinuous" for the latter. The first occurrence of this "very

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mathematische Abhhandlung, 17. On this, see M. Brillouin, *Leçons sur la viscosité des liquides et des gaz* (Paris: Gauthier-Villars, 1907), 196ff.

[170] H. Poincaré, *La Science et l'hypothèse* (Paris: Flammarion, 1918), 110. My translation. On laboratory culture and theoretical practices in late nineteenth-century England, see A. Warwick, "Le laboratoire Cavendish: A Cambridge, deux mondes s'opposent," *La Recherche,* 300 (1997): 70-75.

[171] O. Reynolds, "An Experimental Investigation of the Circumstances which Determine whether the Motion of Water shall be Direct or Sinuous, and of the Law of Resistance in Parallel Channels." *Philosophical Transactions of the Royal Society,* 174 (1883): 935; repr. *Papers,* 2: 51-77, 52; for date, see p. 58.

descriptive term"—i.e. turbulence—was attributed to William Thompson, later Lord Kelvin, by Horace Lamb.[173]

This distinction between "direct" and "sinuous" flows was provided by experiments, *not* theory. From March to April 1880, Osborne Reynolds experimented with glass tubes which would lead him to this conclusion. He was assisted by Mr. Forster, of Owens College, who built several glass tubes of varied diameters fitted with trumpet mouthpieces, so that the water from a tank above might enter without disturbance. As water was made to flow through the tube, a colored streak was added to clear water. What Reynolds and Foster observed was the subsequent appearance of the streak. "The general results [of their observations] was as follows:–

> When the velocities were sufficiently low, the streak of colour extended in a beautiful straight line through the tube. . . .
>
> As the velocity was increased by small stages, at some point in the tube, always at a considerable distance from the trumpet or intake, the colour band would at once mix up with the surrounding water, and fill the rest of the tube with a mass of coloured water. . . .
>
> On viewing the tube by the light of an electric spark, the mass of colour resolved into a mass of more or less distinct curls, showing eddies.[174]

The central question for Reynolds was whether there was a clear way to distinguish between these two kinds of motion. "Did steady motion hold up to a

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[172] O. Reynolds, "An Experimental Investigation," 52.
[173] H. Lamb, *Hydrodynamics*, 6th ed. (Cambridge: Cambridge University Press, 1932; repr. 1993), 664. See W. Thomson, "On the Propagation of Laminar Motion Through a Turbulently Moving Inviscid Fluid," *B. A. Report* (1887): 386-495; *Philosophical Magazine,* 24 (1887): 342-353; repr. in Kelvin, *Mathematical and Physical Papers,* 4, ed. J. Lamor (Cambridge: Cambridge University Press, 1910): 308-320. On p. 311, Thomson defined "the average velocity of the turbulent motion," but not turbulence itself.
University of Cambridge for 1888 (p. 321)."

critical value and then eddies come in?" he asked. "Did the eddies first make their appearance as small and then increase gradually with the velocity, or did they come suddenly?"[175] Reynold's experiment seemed to settle these questions in favor of a sudden appearance of turbulence. There was a critical speed below which motion was steady, and above which it became "sinuous." The critical speed depended on the experimental setting; it varied with the radius of the pipe and with the water temperature, which had an effect on its density and viscosity.

With his experiments Reynolds found that the appearance of "sinuous" motions, or eddies in the colored streak, occurred in all cases at a critical value of a single dimensionless parameter. Considering U as the mean speed along the tube of a fluid with viscosity μ and density ρ, and a single parameter D characterizing linear dimensions of the tube, say its diameter, Reynolds defined the parameter as follow:

$$Re = \frac{\rho D U}{\mu}.$$

Depending on how he defined "criticality," that is, on which observation Reynolds used in order to locate the change of character of fluid motion, experiments showed the value of the critical Reynolds number for the appearance of "sinuous" motion lying between 2000 and 12,000.[176] The consistency of this value in several

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[174] O. Reynolds, "An Experimental Investigation," 59-60.
[175] Questions 4 and 6, in O. Reynolds, "An Experimental Investigation," 57-58.
[176] O. Reynolds, "On the Dynamical Theory," 536. In his 1883 paper, Reynolds expressed his results using Poiseuille's law: $P = (1+\alpha T+\beta T^2)^{-1} \propto \mu/\rho$, α=0.0336 and β=0.00221, T being the temperature of the water. In this case, he found P/Uc=43.79, c being the diameter of the pipe, corresponding to Re~12,000, as the value at which steady motion broke down. The law of resistance, however, also changed, at a critical speed, from being proportional to mean water velocity. In this case, the critical point

experimental situations proved, Reynolds thought, "not only the existence of a critical velocity at which eddies come in, but that it is proportional to the viscosity and inversely proportional to the diameter of the tube."[177]

Reynolds considered that this observation of "sinuous" motion in water and the relation between critical velocity, diameter, and viscosity stood "prominently forth, as to invite or defy theoretical treatment." Indeed, although apparently founded by Stokes on sound first principles and successfully tested for "direct" flow, theory was conspicuously unable to account for unsteady motion in water. "The theory of hydrodynamics has so far failed with the slightest hint why it should explain these phenomena [of sinuous flow] encountered by large bodies moving at sensibly high velocities through water, or that of water in sensibly large pipes." Did responsibility lie with "some fundamental principles of fluid motion of which due account has not been taken in the theory"?[178] Were the Navier-Stokes equations to be modified in order to account for "sinuous" motion?

Reynolds thought unlikely the prospect of finding anything faulty in the Navier-Stokes equations. Still, he believed that "they might contain evidences which had been overlooked, of the dependence of motion on a relation between the dimensional properties and the external circumstances of motion (55)." Indeed Reynolds noticed that the dependence of tube resistance on the velocity of the flow

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was also only a function of P/Uc which was then equal to 278. This later value was consistent with earlier experimental results by Darcy (1857) and Poiseuille (1845), and is also equivalent with $Re \sim 2000$. See Reynolds, "An Experimental Investigation," 60 and 74.

[177] O. Reynolds, "An Experimental Investigation," 75.

[178] O. Reynolds, "An Experimental Investigation," 52-53.

was distressing in the sense that it seems to imply a dependence on absolute dimensions. In an early example of dimensional analysis, he noted that the ratio μ/ρ of the viscosity over the density was a "quantity of the nature of the product of a distance and a velocity (54)."[179] The Navier-Stokes equations also confirmed that the ratio of the nonlinear term $(\mathbf{v}\cdot\nabla)\mathbf{v}$ to the viscosity terms $\nu\Delta\mathbf{v}$ was proportional to the Reynolds number *Re*.

> Of course without integration the equations only gave the relation without showing at all in what way the motion might depend upon it. It seemed, however, to be certain, if the eddies were due to one particular cause, that integration would show the birth of eddies to depend on some definite value of [*Re*].[180]

For Reynolds, the next theoretical step therefore should have been to deduce the critical value of the Reynolds number starting from the Navier-Stokes equations. At first, however, Reynolds did not present such an investigation, which he noted would have "involved the integration of the equations for unsteady motion in a way that has

179 O. Reynolds, "An Experimental Investigation," 54-55. Reynolds's own reconstitution of the path that led him to this simple idea might be of interest for the history of dimensional analysis: "It is always difficult to trace the dependence of one idea on another. But it may be noticed that no idea of dimensional properties . . . occurred to me until after the completion of my investigation on the transpiration of gases, in which was established the dependence of the law of transpiration on the relation between the size of the channel and the mean range of the gaseous molecules (54)." Historical references for dimensional analysis are the following: Rayleigh, "Presidential Address," *British Association Reports* (Montréal, 1884): 1-23; *Papers,* 2: 333-354, 344; "The Principle of Similitude." *Nature,* 95 (1915): 66-68; repr. *Papers,* 6: 300-305; A. Vaschy, "Sur les considérations d'homogénéité en Physique," *CRAS,* 114 (1892): 1416-1419; and E. Buckingham, "On Physically Similar Systems: Illustrations of the Use of Dimensional Equations," *Physical Review,* 4 (1914): 345-376. See also P. W. Bridgman, *Dimensional Analysis* (New Haven: Yale University Press, 1963).
180 O. Reynolds, "An Experimental Investigation," 55.

not been accomplished, and which, considering the general intractability of the equations, was not promising."[181]

When Reynolds came back to this problem in 1894-1895, he tackled the theoretical problem of the determination of critical Reynolds numbers, and introduced statistical methods.[182] While both of these approaches would be widely followed later, it is only necessary for my purpose to discuss theoretical attempts at evaluating critical Reynolds numbers. Apparently, this line of research was triggered by the fact that "the stability or instability of the steady motion of a viscous fluid" had been proposed by William Strutt, Lord Rayleigh, as the subject for the Adams Prize of the University of Cambridge for 1888.[183] Well undertaken by the work of British physicists of Thomson's and Rayleigh's stature among others, this subject of stability theory would evolve into a thriving subdiscipline of fluid mechanics which attracted the attention of the likes of Lorentz,[184] Sommerfeld, and Heisenberg. Following Reynolds and his contemporaries, the evaluation of critical Reynolds numbers was accomplished for a few simple cases, known as the Poiseuille, Couette, and Bénard flows, which became paradigmatic of stability theory.

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[181] O. Reynolds, "An Experimental Investigation," 57.
[182] O. Reynolds, "On the Dynamical Theory."
[183] Rayleigh, "Further Remarks on the Stability of Viscous Fluid Motion," *Philosophical Magazine,* 38 (1914): 609-619; repr. *Papers,* 6: 266-275, 267. William Thomson [Lord Kelvin], "Broad River Flowing Down an Inclined Plane Bed," *Philosophical Magazine,* 24 (1887): 188-196, and 272-278; repr. *Papers,* 4, 321-330 and 330-337, 321. About the problem of the stability of fluid motion for Thomson, see C. Smith and M. N. Wise, *Energy and Empire,* chap. 12.
[184] See H. A. Lorentz, "Ein allgemeiner Satz, die Bewegung einer reibenden Flüssigkeit betreffend, nebst einigen Andwendungen desselben," *Abhandlungen über theoretischen Physik,* 1 (1907): 43-71.

c) Stability Theory: The Conceptual Unit Challenged by the Ruelle-Takens Model

In 1953, as a graduate student, Russel J. Donnelly asked Lars Onsager at Yale University what stability theory was: "He informed me that hydrodynamic stability was a small field of physics carried on by 'a small crew'. The crew members were identified as Chia-Chiao Lin (at MIT), Subrahmanayan Chandrasekhar (at the University of Chicago) and Geoffrey Ingram Taylor (at the University of Cambridge)."[185] The history of stability theory went as far back as Georges Stokes in the first half of the nineteenth century and it had been the subject of much controversy up until Lin's impressive synthesis in 1945-1955.[186]

At the basis of stability theory lay the assumption that the Navier-Stokes equations provided the correct description of turbulent, as well as laminar flows. Since it was an experimental fact that the laminar solution ceased to be observed when the Reynolds number went though a certain critical value between 1,000 and 100,000 depending on the geometry of the arrangement, there had to exist other solutions which were realized only at large Reynolds numbers.

> It is only reasonable to infer from this that the laminar flow, while still a solution, ceases to be a stable one, or at least the most stable. It is plausible to conclude that the turbulent flow represents one or more solutions of a higher stability, and that these come into existence or at least acquire their higher stability, only for high values of Reynolds' number.[187]

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[185] R. J. Donnelly, Review of *The Life and Legacy of G. I. Taylor* by George Batchelor, in *Physics Today,* 50(6) (1997): 82.
[186] Some historical remarks and references are to be found in P. G. Drazin and W. H. Reid, *Hydrodynamic Stability* (Cambridge: Cambridge University Press, 1981).
[187] J. von Neumann, "Recent Theories," 439.

In 1945, Chia-Chiao Lin, a student of Theodore von Kármán at Caltech, summarized the "final aims" of most of the work in stability theory as follows:

> 1) The first aim of stability theory is to determine whether a given flow (or a given class of flows) is ultimately unstable for sufficiently large Reynolds numbers.
>
> 2) The second purpose is to determine the minimum critical Reynolds number at which instability begins. . . .
>
> 3) Finally, we want to understand the physical mechanism underlying the phenomena by giving theoretical interpretations and experimental confirmations of the results obtained from mathematical analysis.[188]

With the goals of stability theory stated as such, one sees that the model suggested by Ruelle and Takens only addressed the first of Lin's purposes. In addition, the modeling practice they introduced was at odds with many of those used by stability theorists.

In the following, going back to the beginning of the century will show the successes and controversies that surrounded stability theory. Again, we will pay attention mainly to the modeling practices involved, to the confidence put on the Navier-Stokes equations, and not so much the specific context for each contribution. Lin's synthesis will show that his approach, while resolving long-standing controversies, became irrelevant for the turbulence problem and was almost immediately superseded by new nonlinear methods. These nonlinear methods were those directly challenged by the picture suggested by Ruelle and Takens.

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[188] C.-C. Lin, "On the Stability of Two-Dimensional Parallel Flows," Quar*terly of Applied Mathematics,* 3 (1945): 117-142; 218-234; and 277-301, 1; repr. *Selected Papers,* 117.

(i) Ancestors and Controversy

Already in 1843 George G. Stokes conjectured that among the causes for the "discrepancy between theory and observation," besides internal friction, a possibility existed that flows might be "unstable." In certain circumstances, he suggested, it might happen that a flow "though dynamically possible, nay the *only* dynamically possible when the conditions which we have supposed are accurately satisfied, is unstable, so that the slightest cause produces a disturbance in the fluid, which accumulates . . . till the motion is quite perturbed."[189] Although close in its expression to sensitive dependence on initial conditions, this statement should not be confused with an anticipation of chaos. Rather Stokes insisted on the well known fact that some solutions of differential equations may be unstable, as for a ball rolling on an edge which might fall on either side.

Introduced by Rayleigh in 1880, a general method, called the method of small oscillations, was favored for the investigation of the stability of small perturbations of a stationary flow.[190] A solution $(\mathbf{V}, p)$ was said to be stationary if it satisfied the time-independent Navier-Stokes equations:

$$(\mathbf{V}\cdot\nabla)\mathbf{V} = -\frac{1}{\rho}\operatorname{grad} p + \nu\Delta\mathbf{V}.$$

[189] G. G. Stokes, "On Some Cases of Fluid Motions," *Transactions of the Cambridge Philosophical Society,* 8 (1845), 105; repr. *Papers,* 1: 17-68, 53-54. Noticed by O. Reynolds, "An Experimental Investigation," 55.

[190] See, e.g., the following review articles: F. Noether, "Das Turbulenzproblem," 128-131; J. L. Synge, "Hydrodynamical Stability," 235ff. Rayleigh, "On the Stability, or Instability, of Certain Fluid Motions," *Proceedings of the London Mathematical Society,* 11 (1880): 57-70; repr. *Papers,* 1: 474-487.

To study small disturbances of the stationary motion, solutions of the following form were studied:

$$\mathbf{v}(x,t) = \mathbf{V}(x) + \varepsilon\mathbf{v}'(x,t) + \text{ higher order terms in } \varepsilon;$$

where ε was small for small Reynolds numbers. Then the equations statisfied by $\mathbf{v}'$ could be derived by subsituting the above *Ansatz* in the Navier-Stokes equations and neglecting terms of higher order in ε, which amounted to a linearization of the equation. The method of small oscillations consisted in supposing that $\mathbf{v}'$ had the following form:

$$\mathbf{v}'(x,t) = e^{\sigma t}\mathbf{F}(x).$$

The question of the stability or instability of the perturbation was then reduced to the question of computing whether the real part of σ, in general a complex number, was or was not positive. In the positive case the magnitude of the disturbance would grow exponentially. An infinitesimal variation from the stationary solution would then become, after a certain time, large enough to be observed. In general, a critical Reynolds number Re_{crit} could be computed, below which the motion was stable with respect to small oscillatory disturbances, and above which it became unstable. What then would be the observed solution of the Navier-Stokes equations, however, received no answer.

This approach to stability was very difficult to put in practice and had obvious limitations. First, it required that an exact solution to the Navier-Stokes equations be computed to start with. This theory "can only be attempted in cases when one possesses a special solution, however we know how rare are the exact solutions of motions in viscous fluids."[191] There were more or less three classes of flows where the techniques of stability theory had been applied: the Poiseuille flow, characteristically involving flows in pipes or canals of different sections driven by a pressure gradient; the Couette flow, involving flows between surfaces, typically coaxial cylinders or parallel planes, moving at a constant speed relative to one another; and the Bénard convective flow, in which a layer of fluid was heated from below. Each of these different types of flows could also be studied in two dimensions, which led to significant simplification, but left the results open to criticism.

Second, a number of different assumptions, all of which could be questioned, entered into the above procedure and led to contradictory results for the plane Couette flow. In particular, in the 1910s, following Arnold Sommerfeld's lead, Ludwig Hopf and R. von Mises concluded that the flow remained stable for all Reynolds numbers.[192] This result was difficult to test exprimentally, however, and seemed a "surprising result from a physical point of view."[193] In other words, between two infinite plates moving at a constant relative velocity, turbulence should not develop! In 1923, in his doctoral thesis, Werner Heisenberg used different approximation

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[191] H. Villat, *Leçons sur les fluides visqueux,* recueillies et rédigées par Julien Kravtchenko (Paris: Gauthier-Villars, 1943), 423. See also Villat's early work, "Sur quelques progrès récents des théories hydrodynamiques," *Bulletin des sciences mathématiques,* 42 (1918): 43-60; 72-92; and his thesis: "Sur la résistance des fluides," *Annales scientifiques de l'École normale supérieure,* 28 (1911): 203-311.
[192] A. Sommerfeld, "Ein Betrag zur hydrodynamischen Erklärung;" R. von Mises, "Kleine Schwingungen und Turbulenz," *Jahresbericht der Deutschen Mathematiker-Vereinigung,* 21 (1912): 2416248; "Beitrag zum Ozscilationsproblem," *Festschrift Heinrich Weber* (Leipzig and Berlin, 1912): 252-282; L. Hopf, "Der Verlauf kleiner Schwingungen auf einer Strömung reibender Flüssigkeit," *Annalen der Physik,* 44 (1914): 1-60.
[193] J. L. Synge, "Hydrodynamical Stability," 261.

methods (similar to the Wentzel-Kramers-Brillouin method later used in quantum theory) to derive a critical Reynolds number for the same case. His method was harshly criticized by Noether and spurred a long-standing controversy that was not resolved until C.-C. Lin's work in the late 1940s.[194] This controversy had two important effects. First, it once again cast doubt on the validity of the Navier-Stokes equations; and second, it made people wonder about the pertinence of the scheme of small oscillations for the determination of critical values. It seemed that one was entitled to concur with William Orr's gloomy diagnosis: "It would seem improbable that any sharp criterion for stability of fluid motion will ever be arrived at mathematically."[195] Other methods were thus developed, most notably energy methods and Ludwig Prandtl's boundary layer theory.[196]

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[194] W. Heisenberg, "Über Stabilität und Turbulenz von Flüssigkeitsströmen," *Annalen der Physik,* 74 (1924): 577-627 [his doctoral thesis (Munich, July 1923)]; repr. *Gesammelte Werke/Collected Works,* ser. A, 1, group 1, introductory essay by S. Chandrasekhar and H. Rechenberg. About Heisenberg's thesis and the following controversy, see J. Mehra and H. Rechenberg, *The Historical Development of Quantum Theory,* 2 (New York: Springer, 1982), Section I.7: 49-63. For his critique, see F. Noether, "Zür asymptotischen Behandlung der stätionären Lösungen im Turbulenzproblem," *Zeitschrift für angewandte Mathematik und Mechanik,* 6 (1926): 232-243, 242. It was Heisenberg himself who later noted the similarity of his method with the WKB approximation: "On the Stability of Laminar Flow," *Proceedings of the International Congress of Mathematicians, Cambridge, Mass., 1950,* 2 (Providence: AMS, 1952): 292-296.

[195] W. McF. Orr, "The Stability or Instability of the Steady Motion of a Fluid. Part II: A Viscous Liquid," *Proceedings of the Royal Irish Academy (Dublin),* A27 (1907): 69-138. Similarly, Rayleigh declared in 1916: "One can hardly deny that [theoretical hydrodynamics] is out of touch with reality." Cf. his review of Lamb's *Hydrodynamics, Papers,* 6: 401.

[196] For energy methods, see F. Noether, "Das Turbulenzproblem," 131-133; J. L. Synge, "Hydrodynamical Stability," 263-266. About boundary layer theory, see L. Prandtl, "Über Flüssigkeitsbewegung bei sehr kleiner Reibung," *Proceedings of the Third International Congress for Mathematics* (1904): 484-491; *Führer durch die*

(ii) Success with Taylor-Couette Flow: Sequence of Instabilities

In 1923, stability theory witnessed a "conspicuous triumph in the work of G. I. Taylor [1886-1975]."[197] This work has been emphatically praised by many a fluid dynamicist. "One of the most influential investigations of 20th-century phyics," according to Donnelly, Taylor's paper was seen as a definite proof for the correctness of the Navier-Stokes equations and a definitive settlement in favor of the no-slip boundary conditions.[198] "It was a tour de force which, more than any other single paper, established hydrodynamic stability as a distinct field."[199] Contrary to Ruelle and Takens's later views, Taylor contended:

> It seems doubtful whether we can expect to understand fully the instability of fluid flow without obtaining a mathematical representation of the motion of a fluid in some particular case in which instability can actually be observed, so that a detailed comparision can be made between the results of analysis and those of experiments.[200]

The particular case he selected was that of a liquid contained between two coaxial cylinders, rotating with different frequencies. As opposed to the infinite planes considered before him, such motion would be readily observable in a carefully designed experiment. In addition, Lord Rayleigh had worked out a criterion for

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Stromungslehre, 3rd ed. (Braunschweig: Fr. Vieweg und Sohn, 1942); *Guide à travers la mécanique des fluides,* transl. A. Monod (Paris: Dunod, 1952).

[197] J. L. Synge, "Hydrodynamic Stability," 228. See G. I. Taylor, "Stability of a Viscous Liquid contained between Two Rotating Cylingers," *Philosophical Transactions of the Royal Society (London),* A223 (1923): 289-343.

[198] About Taylor's work, see R. J. Donnelly, "Taylor-Couette Flow: The Early Days." *Physics Today,* 44(11) (1991): 32-39; where the above quote is to be found.

[199] G. K. Batchelor, *The Life and Legacy of G. I. Taylor* (Cambridge: Cambridge University Press, 1996), 88.

[200] G. I. Taylor, "Stability of a Viscous Liquid," 290.

inviscid fluid.[201] In the 1880s, this type of experiment had been performed by H. R. A. Mallock before the Royal Society, and Maurice Couette for his doctoral thesis.[202] Interestingly, Couette undertook his experiments not in order to show the stability of the motion, but rather to answer the following "fundamental question": "Is the interior friction coefficient [i.e. viscosity] a well-defined physical quantity?"[203] This question naturally came up as a consequence of Boussinesq's suggestion of considering viscosity as a function of space and geometry, as well as of the physical characteristics of the fluid. Couette's experiments confirmed that the motion assumed one of two regimes: "the first exactly conforms to the simplest integral of the Navier[-Stokes] equations; the second does not conform to these integrals."

Not content with providing a complete theoretical study of the stability of Couette flows, notably using Bessel functions, Taylor also performed careful experiments which agreed with his theory and provided further directions for research. Like Couette before him, he indeed noticed that the first instability was not the only one. Between steady motion and fully developed turbulence, many different instabilities arose one after the other. With his glass cylinder, Taylor observed that the first instability involved the formation of rolls perpendicular to the axes of the cylinders. He provided a complete theoretical derivation of these solutions. He moreover noticed that at higher Reynolds numbers spirals and other types of vertical

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201 Lord Rayleigh, "On the Dynamics of Revolving Fluids," *Royal Society Proceedings,* A (1916): 148-154; repr. *Papers,* 6: 447-453.
202 M. Couette, "Sur un nouvel appareil pour l'étude du frottement des fluides," *CRAS,* 107 (1888): 388-390; "Études sur le frottement des liquides," *Annales de chimie et de physique,* 6th ser., 21 (1890): 433-510. [His thesis.]
203 M. Couette, "Études sur le frottement," 433.

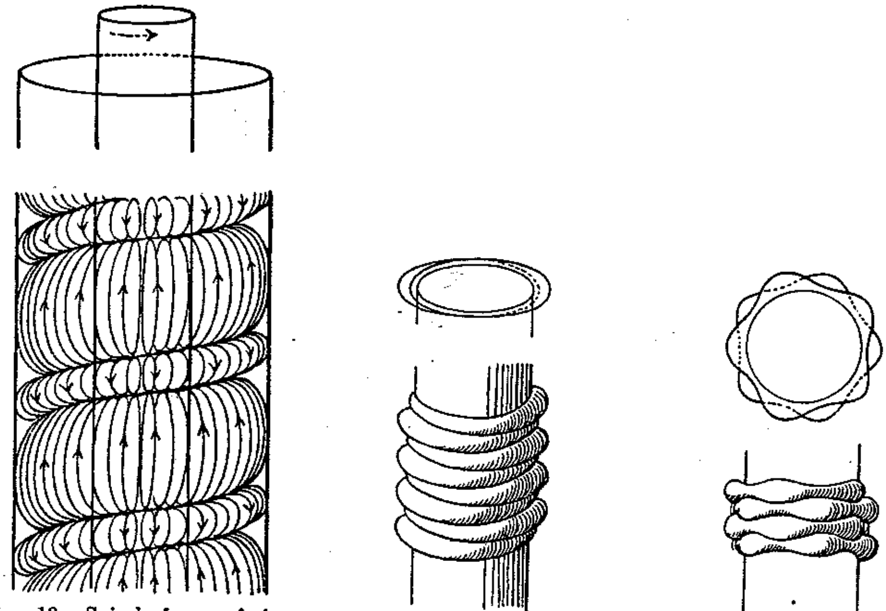

Fig. 19. Spiral form of instability which appears when steady motion is not strictly limited to two dimensions before instability sets in.

Fig. 21. Sketch illustrating appearance of vortices when they begin to break up; case when $\mu = -1$ approximately.

Fig. 22. Appearance of vortices when they begin to break up immediately after their formation: case when μ is less than

Figure 14: Secondary Oscillation Observed by G. I. Taylor in the Couette Flow. Repr. with permission from G. I. Taylor, "Stability of a Viscous Liquid," 326 and 343. Copyright © The Royal Society in London.

disturbances could appear. These experiments provided the ground for Hopf's and Landau's works in which they considered instabilities as involving appearances of frequencies in a sequence (Fig. 14).

Taylor's observation was crucial in undermining the arguments for an oscillation between the two regimes (steady and turbulent) which had been proposed by Couette in 1890.[204] In 1898-1899 and 1899-1900, Marcel Brillouin gave a course on fluid mechanics at the Collège de France where he took up Couette's proposal.[205] Carefully reviewing previous experiments and even repeating them and performing new ones with the help of Henri Bénard—it was at this occasion that Bénard famously studied convective flows[206]—Brillouin had to acknowledge that the theory explaining the passage from one regime to another "was scarcely sketched." His admiration for Reynolds notwithstanding, Brillouin did not insist on critical Reynolds numbers but was rather inclined to believe that the transition was progressive.[207] G. I. Taylor partly confirmed this view. The transition to turbulence was not sudden, but rather involved a succession of instabilities appearing one after the other.

Taylor's success with the Couette flow provided new grounds for feeling more confident about the theory of hydrodynamic stability. One telling example of this renewed interest was that at the celebration of the first fifty years of the American Mathematical Society this topic was the sole contribution on "application of mathematics." According to J. L. Synge, who delivered the address, its preparation "involved a difficult decision," because the subject of applied mathematics was so

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204 M. Couette, "Études sur le frottement," 478-480.

205 Marcel Brillouin, *Leçons sur la viscosité,* esp. Livre II, chapitre IV: "Le régime de Poiseuille et le régime hydraulique. Passage d'un régime à l'autre," 196-224.

206 H. Bénard, "Les tourbillons cellulaires dans une nappe liquide I: Description générale des phénomènes," *Revue générale des sciences pures et appliquées,* 11 (1900): 1261-1271; "Les tourbillons cellulaires dans une nappe de liquide transportant de la chaleur par convection en régime permanent," *Annales de chimie et physique,* 7th ser., 23 (1901): 62-144.

207 "Dans un même tube de verre le passage d'un régime à l'autre n'a pas lieu brusquement, à partir d'une vitesse déterminée, mais il existe une période troublée où les deux régimes sont possibles et alternent avec une fréquence plus ou moins grande." M. Brillouin, *Leçons sur la viscosité,* 196.

vast. He chose hydrodynamic stability because he felt this was a topic that was well formulated mathematically, interesting not only to the mathematician but also to the physicist and the engineer, and involved still unsolved problems.[208]

Taylor's work notwithstanding, many fluid dynamicists felt that the successes of stability theory were at best relative. In 1930, Henri Villat, a student of Brillouin whom we have met as Leray's mentor, wrote that the theoretical study of the transition to turbulence was in its infancy. "Here are huge fields of research where workers will be able to exert their sagacity and where experimenters will often open the way to mathematicians."[209] In the early 1940s, Caltech mathematician and engineer Theodore von Kármán (1881-1963), a Hungarian émigré, endeavored to draw the mathematicians' attention to many nonlinear problems, difficult on a mathematical level, but crucial for engineers. For him, therefore, hydrodynamics and aerodynamics, his main fields of research, provided clear examples of nonlinear engineering problems where advanced mathematical methods were required. But he did not deal with the turbulence problem, barely noting that:

> [it] has been discussed mathematically by several authors by means of linearized equations without reaching a satisfactory agreement with experiment. The adequate treatment of the nonlinear equations is bound to contribute essentially to the solution of this important problem.[210]

Similarly, John von Neumann contended in 1949:

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208 J. L. Synge, "Hydrodynamical Stability," 227. See G. I. Taylor, *Proceedings of the Fifth International Congress of Applied Mathematics, Cambridge, Mass., 1938* (New York: J. Wiley & Sons, 1939): 304-310.

209 Henri Villat, *Mécanique des fluides* (Paris: Gauthier-Villars, 1930), vi-vii. [Cours de l'École normale d'aéronautique.]

> The summing up of several decades of stability theory appears to be this: The stability theory proved to be mathematically much more difficult than might have been originally expected. . . . [Its relation] to experiment has so far not been a very satisfactory one.[211]

Manifestly, at the end of World War II, classical analytic methods had reached a limit. The theory of hydrodynamic stability was in need of some good cleaning-up. It would however be achieved at a crucial cost: in the process it lost much of its relevance as a way to understand turbulence.

(iii) Synthesis, but Insignificance?

In fact, Taylor's very observation of a sequence of instabilities indicated that stability theory was but the beginning of the theoretical study of the onset of turbulence. Like von Kármán, von Neumann believed that stability theory had to become fully nonlinear in order to answer questions about the nature of turbulence:

> The stability theory could at best only determine when the laminar flow breaks down and turbulent flow becomes possible. It will not describe, however, what the properties of the developed turbulent flow are. This linear, 'small perturbation' theory must obviously be complemented by a non-linear theory of large deviations from the laminar pattern. Or to put it more directly: *A complete non-linear theory of the general solutions of the Navier-Stokes equations is called for.*[212]

In 1944, Chia-Chiao Lin, a student of von Kármán, started to publish his work that would end up establishing solid foundations for stability theory, as summarized in his acclaimed 1955 monograph.[213] Building on Synge's work, he thereby confirmed

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[210] T. von Kármán, "The Engineer Grapples with Nonlinear Problems." *Bulletin of the American Mathematical Society,* 46 (1940): 615-683, 664n. See also "Tooling up Mathematics for Engineering." *Quaterly of Applied Mathematics,* 1 (1943):. 1-6.
[211] J. von Neumann, "Recent Theories," 441.
[212] J. von Neumann, "Recent Theories," 441. My emphasis.
[213] C.-C. Lin, *The Theory of Hydrodynamic Stability* (Cambridge: Cambridge University Press, 1955).

the results of Heisenberg's 1923 thesis and rehabilitated the field of stability theory. What is the most noticeable in his work is that he made crucial use of the first computers available in order to recover Heisenberg's prediction that the plane Couette flow was indeed unstable.[214] However, Lin clearly resented this state of affairs: "It would be highly desirable if the instability of the classical motion could be proved without resorting to such heavy calculations."[215] As with every previous success, Lin's synthesis only furthered belief in the validity of the Navier-Stokes equations.

Subramanayan Chandrasekhar, the famous Indian physicist from the University of Chicago, was the third "crew member" who, according to Onsager, was embarked in the 1950s on the boat of hydrodynamical stability theory. He was a master at combining delicate experimentation,[216] numerical computations,[217] theoretical derivation, and an uncanny physical intuition. But mostly, Chandrasekhar's work showed the ability of Lin's methods to account for a wide variety of cases. Above all, Chandrasekhar explored the stability of convective flows, when combined with rotational motion or electromagnetic fields.[218]

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214 See C.-C. Lin, "On the Stability of Two-Dimensional Parallel Flows." *Quaterly of Applied Mathematics,* 3 (1946): 117-142; 218-234; 277-301. Repr. *Papers,* 1: 1-68.
215 C.-C. Lin, *The Theory of Hydrodynamic Stability,* 31.
216 See a picture of his hydromagnetic laboratory at the Fermi Institute for Nuclear Studies, Chicago, in figure 12 in S. Chandrasekhar, "Thermal Convection: Rumford Medal Lecture 1957," *Daedalus,* 86 (4) (1957): 323-339; repr. *Selected Papers of S. Chandrasekhar,* 4 (Chicago: University of Chicago Press, 1989): 163-191, 180.
217 For example, Chandrasekhar acknowledged von Neumann's help in having numerical work done on the IAS machine in S. Chandrasekhar, "The Stability of Viscous Flow Between Rotating Cylinders in the Presence of a Radial Temperature Gradient," *Journal of Rational Mechanics and Analysis,* 3 (1954): 181-207; repr. *Selected Papers,* 4: 107-133.
218 The summary of his work is presented in *Hydrodynamic and Hydromagnetic Stability* (Oxford: Clarendon, 1961).

But at the same time as Lin settled the theoretical bases and Chandrasekhar explored their ramification, stability theory ceased to be as relevant as before, as von Neumann's remark above indicates, since it only dealt with the first instability. Lin himself was the first to acknowledge this: "The problem of the transition to turbulence . . . is in practice more important than that of stability of laminar motion."[219] But Lin admitted that he could not deal with this "more important" problem with the methods he had chosen to exploit. Indeed, a result of James Serrin's, painfully obtained following these methods together with energy methods, might be taken as symptomatic of the lack of relevance of hydrodynamic stability theory. In 1959, Serrin was able to determine a "universal stability criterion" showing that fluid flows were always stable for Reynolds numbers $Re < 5.71$, which should be compared with the values of the order of 1,000 to 10,000 obtained in most experimental situations![220]

Chia-Chiao Lin's modeling practice involved linear assumptions, reliance on computer calculations, and a refusal to interpret turbulence. In particular his basic assumptions for linear stability theory were two: first, he only considered infinitesimal disturbances and not finite ones and second, he "assume[d] that, for small disturbances, the [Navier-Stokes] equations may be linearized; that is, we shall neglect terms quadratic or higher in the disturbances and their derivatives."[221] This practice would be challenged by the next generation, who would endeavor to build a

[219] C.-C. Lin, *The Theory of Hydrodynamic Stability,* ix.
[220] J. Serrin, "On the Stability of Viscous Fluid Motions," *Archive for Rational Mechanics and Analysis,* 3 (1959): 1-13.
[221] C.-C. Lin, *The Theory of Hydrodynamic Stability,* 1.

nonlinear theory of hydrodynamic stability without resorting to the computer. These were the scientists who had to react to Ruelle and Takens's suggestions.

(iv) Nonlinear Stability Theory

"As engineering science progresses," von Kármán contended in 1943, "the need for more exact information and the necessity to get nearer and nearer to physical reality, forces us to grapple with many nonlinear problems."[222] The nonlinear study of hydrodynamic stability could be taken to have started with "a capital memoir" by J. T. Stuart, from the National Physical Laboratory, Teddington, Middlesex, in 1958.[223] In this work, his ambition was important; he hoped that by considering nonlinear disturbances, stability theory could again claim to provide an account for turbulent phenomena in Poiseuille, Couette, and Bénard flows.

> A more fundamental objective [of the theory of hydrodynamic stability] is to understand how, and under what circumstances, turbulence may arise from laminar instability. . . . It is clear that the stability problem in its general form must be considered to be non-linear, because the equations of motion [i.e. Navier-Stokes] are non-linear.[224]

Significantly, Stuart noticed the "interesting suggestion concerning the development of turbulence from the growth of small disturbances" which had been advanced by Lev Landau in 1944. Indeed, from the point of view of stability theory,

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[222] T. von Kármán, "Tooling up Mathematics," 5.

[223] J. T. Stuart, "On the Non-Linear Mechanics of Hydrodynamic Stability," *Journal of Fluid Mechanics,* 4 (1958): 1-21. Gérard Iooss called Stuart's paper "a capital memoir" in his doctoral thesis: *Contribution à la théorie non-linéaire de la stabilité des écoulements laminaires,* thèse, Paris-VI (1971), Jussieu Lib. Note that there will be an important blind spot in the account of nonlinear stability theory provided here, namely the contributions of several Soviet scientists, and in particular V. I. Yudovich. For some references, see D. Ruelle and F. Takens, "Note Concerning our Paper," *TSAC,* 83-84.

Landau's was the first method to tackle nonlinear disturbances.[225] Stuart believed that this suggestion could even account for experimental observations that seemed to belie it:

> "there are cases of flow in which turbulence develops rather suddenly as the Reynolds number is raised, and in these cases one might infer that the critical Reynolds numbers are close together."[226]

In the early 1970s, most of the more mathematical contributions to nonlinear stability theory appeared in the *Archive for Rational Mechanics and Analysis.* Since this journal rejected Ruelle and Takens's paper, it is interesting to look at what it was publishing concerning the onset of turbulence around the same time. "The editor did not like our ideas," Ruelle recalled, "and referred us to his own papers so that we could learn what turbulence really was."[227] Founded by Clifford Truesdell in the late 1950s, the *Archive* "nourishes the discipline of mechanics as deductive, mathematical science in the classical tradition and promotes pure analysis, particularly in contexts of applications."[228] In 1970, it was edited by James Serrin, from the University of Minnesota, who specialized in the study of the Navier-Stokes equations and the stability of their solutions.[229] The editorial board included hydrodynamic stability theorists, such as D. D. Joseph and C.-C. Lin, applied mathematicians, such as J.-L.

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224 J. T. Stuart, "On the Non-Linear Mechanics," 2.
225 G. Iooss, *Contributions,* 7-8.
226 J. T. Stuart, "On the Non-Linear Mechanics," 5-6. Let us note here that Stuart's work, like Lin's, involved numerical computations performed by Miss S. W. Skan (see P. 21).
227 D. Ruelle, *Chance and Chaos,* 56.
228 Statement of intent of the journal published in each volume.
229 Note that the University of Minnesota, whereto James Serrin attracted both Daniel Joseph and David Sattinger, seems to have played an important role in the

Lions, but also mathematicians coming out of Lefschetz's school on nonlinear differential equations, like L. Cesari. The journal mainly dealt with hydrodynamics, elasticity, and thermodynamics, as well as the theory of ordinary and partial differential equations. More occasionally, it also included papers on various fields of mathematical physics, such as electromagnetism, relativity, or celestial mechanics. Its general philosophy was to print papers dealing with mechanics, in an uncompromisingly rigorous fashion. The standard form of its articles was strictly mathematical, with definitions, theorems, and proofs. Differential equations were the most common expressions to be found in its pages. Apparently, this was an outlet well suited for Ruelle and Takens's article. So, why was it rejected?

In order to understand this rejection one must take notice of the fact that in 1970-1972 several papers appeared in the *Archive* that dealt with the problem of (nonlinear) stability of fluid flows. Their authors were Gérard Iooss, Daniel Joseph, and David Sattinger.[230] Both Sattinger and Iooss came from a mathematical background: the former from the University of California in Los Angeles and the

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development of nonlinear stability theory by maintaining contacts between engineers and mathematicians.

[230] G. Iooss, "Théorie non linéaire de la stabilité des écoulements laminaires dans le cas de « l'échange des stabilité »," *Archive for Rational Mechanics and Analysis,* 40 (1971): 166-208; D. H. Sattinger, "Bifurcation of Periodic Solutions of the Navier-Stokes Equations," *Ibid.,* 41 (1971): 66-80; "Stability of Bifurcating Solutions by Leray-Schauder Degree," *Ibid.,* 43 (1971): 154-166; D. D. Joseph and W. Hung, "Contributions to the Nonlinear Theory of Stability of Viscous Flow in Pipes and Between Rotating Cylinders," *Ibid.,* 44 (1971-1972): 1-22; D. D. Joseph and D. H. Sattinger, "Bifurcating Time Periodic Solutions and their Stability," *Ibid.,* 45 (1972): 79-109; G. Iooss, "Existence et stabilité de la solution périodique secondaire intervenant dans le problème d'évolution du type Navier-Stokes" *Ibid.,* 47 (1972): 301-329. See also D. D. Joseph, "Stability of Convection in Containers of Arbitrary Shape," *Journal of Fluid Mechanics,* 47 (1971): 257-282.

second from Paris.[231] They attacked the problems of hydrodynamic theory with the goal of providing solid mathematical foundations, first to the linear theory, then to show that the nonlinear theory fell back on the results of the linear theory as far as the first instability was concerned.[232] Vladimir Arnol'd's characterization of his own work in hydrodynamics may be well suited as a description of stability theory in the early 1970s: "following N. Bourbaki's call, I endeavored always to substitute blind calculations to the lucid ideas of Euler's."[233]

For Iooss, but also for Sattinger, if the long-standing controversy about stability theory had showed one thing, it was the need to be extremely clear about the functional spaces with respect to which a given flow was stable, or not.[234] They both developed heavy mathematical apparatuses that were extremely clear about the functional spaces they considered, and studied well-defined operators acting on these spaces. At the same time, they inserted themselves within the disciplinary tradition of stability theory, emphasizing Lin's synthesis and Landau's (and, in the case of Sattinger, Hopf's) suggestions as an indication of how to go beyond linear theory.

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[231] Sponsered by the ONERA, the French aerospacial research agency, G. Iooss defended his Ph. D. thesis on March 3, 1971 in front of jury composed of J.-L. Lions (president), J.-P. Guiraud (his main advisor), and A. Avez. See G. Iooss, *Contributions.*
[232] See, in particular, D. H. Sattinger, "The Mathematical Problem of Hydrodynamic Stability," *Journal of Mathematics and Mechanics,* 19 (1970): 797-819.
[233] V. I. Arnol'd, "Sur la géométrie différentielle des groupes de Lie de dimension infinie et ses applications à l'hydrodynamique des fluides parfaits," *Annales de l'Institut Fourier de Grenoble,* 16(1) (1966): 319-361, 319.
[234] D. H. Sattinger, "Stability of Nonlinear Hyperbolic Equations," *Archive for Rational Mechanics and Analysis,* 28 (1968): 226-244; "On Global Solutions of Nonlinear Hyperbolic Equations," *Ibid,* 30 (1968): 148-172.

Mainly, they set themselves the goal of investigating, with the most formal mathematical tools of functional analysis and topology, the stability of the Navier-Stokes equations typically written in the following form:

$$\frac{du}{dt} + L_\lambda u - M_\lambda(u) = 0;$$

where u represented the velocity field, and L_λ and M_λ respectively were linear, and nonlinear, operators depending on a parameter λ representing typically the Reynolds number.[235] Basing themselves on the theory of ordinary differential equations, which was the starting point of dynamical systems theory, they nonetheless insisted on the specificity of partial differential equations, such as Navier-Stokes.

During those years, Iooss's and Sattinger's most remarkable achievement was the study of the bifurcation of periodic solutions of the Navier-Stokes equations and their stability.[236] In Ruelle and Takens's language, this consisted in the second Hopf bifurcation. Without any contact with the flamboyant groups at Berkeley, or Bures-sur-Yvette, Iooss and Sattinger of course did not consider their problems in terms of attractors. Rather, they investigated the existence of a correctly behaved solution to the Navier-Stokes equations. It must be noticed, however, that Sattinger, who was knowledgeable about the work done at the Courant Institute of New York University, often framed his discussion in terms of bifurcation theory.[237] Finally, it should also be

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235 See G. Iooss, *Bifurcation et stabilité,* Lecture notes for a course at Paris XI-Orsay (1972-1973); Jussieu Lib. Of course, the exact form of the equation varied slightly from one author to another, and even for the same author.
236 Again one should mention Yudovich's name in this respect.
237 He cited J. B. Keller and S. Antman, *Bifurcation Theory and Nonlinear Eigenvalue Problems* (New York: Benjamin, 1969).

emphasized that this work always remained purely mathematical and that, as opposed to their predecessors in stability theory, it involved no numerical computation whatsoever.

Compared to the above, Ruelle and Takens's article, no matter how mathematically arduous for the average physicist, was rather informal. The Bures pair remained sloppier about the characterization of the functional spaces they used. Moreover, the theorems they proved were either already well known, or based on unjustified assumptions as far as hydrodynamic flows were concerned.[238] From this shaky mathematical basis, they then speculated much further from stationary solutions than any stability theorists dared to venture, whether working in the nonlinear domain or not.

The modeling practice of the stability theorists involved a clear identification of functional spaces and the operators involved, and not, like Ruelle and Takens, topological features of attractors. The mathematical methods used were part of the classical tradition of functional analysis, well represented in the pages of the *Archive,* as opposed to global analysis and dynamical systems theory. They were not interested in classifying systems, since they already had their firm starting point in the Navier-Stokes equations, which remained unquestioned. Finally, since they were looking at solutions and the stability of disturbances, even if they had had the mathematical apparatus available to go beyond the study of the stability of periodic solutions, which they did not have, they would have been incapable of seeing something like a strange attractor. Aperiodic solutions would have been very difficult to distinguish from quasiperiodic ones without the topological tool of the attractor and the practices that went with it.

Looking at fluid mechanics as a "deductive, mathematical science in the classical tradition," Iooss, Joseph, and Sattinger were hardly in a position to appreciate something like the Ruelle-Takens model. But mainly, one may suspect that the reason why the editor of the *Archive* rejected this paper was that stability theorists then showed little interest for turbulence. For years, stability theorists had endeavored to prove the stability of laminar flow because this was a problem it could address; its practitioners had all but forgotten about turbulence!

6. RECEPTION OF THE RUELLE-TAKENS MODEL BY STABILITY THEORISTS; RECEPTION OF STABILITY THEORY BY RUELLE

Nevertheless, stability theorists' concerns were already close enough to Ruelle and Takens's, so that some dialogue could be established. Even if it involved translation and misunderstanding, this dialogue was an important factor contributing to a wide recognition of the Ruelle-Takens model. In the following, the first confrontation between the two approaches is examined as it took place at the Battelle Research Center in Seattle during the summer of 1972. The consequences that his work with Takens had on Ruelle's career in the years following their paper, and its consequence for the stability theorists' later careers are drawn.

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[238] An exception to this was to so-called "Central Manifold Theorem." Cf. O. E. Lanford, "Bifurcation of Periodic Solutions into Invariant Tori: The Work of Ruelle and Takens," *Nonlinear Problems,* ed. I. Stakgold, et al.: 159-192.

a) Confrontation at Battelle

When Gordon Battelle died in 1923 he bequeathed his large fortune to found an institute devoted to the pursuit of "the social and economic benefits to be derived from scientific research and from the making of discoveries and inventions." Playing a key role during World War II in the metallurgy of uranium, the Battelle Memorial Institute expanded greatly in the postwar years, with a total staff of nearly 6500. In 1967, at its Seattle research center, a first meeting was held in which physicists and mathematicians were invited to exchange ideas, and which included an important French delegation, as well as addresses by Mather, Thom, and Smale.[239] It quickly became a tradition at Battelle to organize this kind of meetings with nearly equal representations from mathematics and areas where mathematicians might have something to contribute.[240]

From July 3 to 28, 1972, Battelle Seattle Research Center welcomed another Summer Institute, this time devoted to "the mathematical analysis of nonlinear problems in the physical and biological sciences." The meeting focused on four areas: biology, statistical mechanics, hydrodynamics, and chemical reaction engineering. Already in 1972, a "theme" emerged to the effect that "disparate branches of science generate common mathematical problems of nonlinear analysis."[241]

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[239] C. M. DeWitt and J. A. Wheeler, eds., *Battelle Rencontres: 1967 Lectures in Mathematics and Physics* (New York: Benjamin, 1968), x-xi.
[240] Note that a meeting on catastrophe theory was held at Battelle on April 21-25, 1975. P. Hilton, ed., *Structural Stability, the Theory of Catastrophes, and Applications in the Sciences: Proceedings of the Conference Held at Battelle Seattle Research Center 1975,* Lecture Notes in Mathematics, 525 (Berlin: Springer, 1976).
[241] I. Stakgold, D. D. Joseph, D. H. Sattinger, eds., *Nonlinear Problems in the Physical Sciences and Biology: Proceedings of the Battelle Summer Institute, Seattle,*

In particular, David Sattinger and Daniel Joseph spoke at great lengths about bifurcations and stability in hydrodynamics, while Oscar E. Lanford, III, introduced the Ruelle-Takens model. A student of Arthur Wightman, who had brought him to the IHÉS in 1963, Lanford was then working in the Mathematics Department at Berkeley. Collaborating with Ruelle on statistical mechanics, he had been invited to come to the IHÉS in 1966-1967.[242] In 1972, at Battelle, we encounter him as a missionary for Ruelle and Takens.[243] Besides introducing the Poincaré map, which apparently was unknown to Joseph, Sattinger, and Iooss (who was not present at Battelle), Lanford proved "Ruelle-Takens theorem." As opposed to Ruelle and Takens's own paper, however, Lanford's had little to do with turbulence and strange attractors.

But the message nonetheless got to Joseph and Sattinger. And they were in the best position to see both the interest and limitations of the Ruelle-Takens proposition. They both found the suggestion very stimulating. In fact, they could even claim to have seen a similar phenomenon, with a more careful identification of the circumstances in which it might happen than Ruelle and Takens's.[244]

> The transition to turbulence through repeating branching [succession of Hopf bifurcations] cannot, however, be the relevant description in the case of subcritical bifurcations. In this case, the time periodic solution which

Juley 3-22, 1972, Lecture Notes in Mathematics, 322 (Berlin: Springer, 1973), preface.

242 Lettres de Léon Motchane à Robert Oppenheimer (27/3/63); de David Ruelle à Léon Motchane (25/7/66); de Léon Motchane à David Ruelle (20/9/66); de Oscar E. Lanford à Léon Motchane (23/11/66). Arch. IHÉS.

243 O. E. Lanford, "Bifurcation of Periodic Solutions."

244 D. D. Joseph, "Remarks about Bifurcation and Stability of Quasi-Periodic Solutions which Bifurcate from Periodic Solutions of the Navier-Stokes equations," *Nonlinear Problems,* ed. I. Stakgold, et al.: 130-158, 151; D. H. Sattinger, "Six Lectures on the Transition to Instability," *Nonlinear Problems,* ed. I. Stakgold, et al.: 261-287, 268.

> bifurcates from the steady solution is unstable from the start and an arbitrary initial disturbance of the steady solution either decays or is attracted to something else, perhaps a stable 'turbulent solution'.[245]

Since, in the "subcritical" scheme, the periodic solution after the first bifurcation was unstable, this obviously was very different from what Ruelle and Takens had in mind. But, at the same time, the much more careful description of bifurcations that Joseph and Sattinger were able to provide had caught the eye of Ruelle. The appendix of Ruelle and Takens's paper was sent to be typed by an IHÉS secretary on July 15, 1970.[246] There, for the first time, Ruelle made the connection with stability theory, citing the work, much of it published in the *Archive for Rational mechanics,* done on the Taylor and Bénard problems by people such as Yudovich, Welte, Fife and Joseph, all well known stability theorists. A dialogue between specialists and applied topologists (such as hardly ever occurred with catastrophe theorists) could take place.

b) Ruelle and the IHÉS After Ruelle-Takens

On June 4, 1973, David Ruelle wrote to Kuiper to ask him to invite Serrin, Joseph, and Sattinger to the IHÉS, at the same time as he asked for Bowen and Lanford. Clearly, he was intending to spend more time on the theory of dynamical systems. But, this had not been the case prior to 1973.

In interview, Ruelle now says that his article with Takens was but a small incursion into a foreign field, which he was not sure he wanted to pursue much further. To a degree, the archives of the IHÉS confirm this. In 1970-1971, however,

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[245] D. D. Joseph and D. H. Sattinger, "Bifurcating Time Periodic Solutions," 106.
[246] Arch. IHÉS. See the appendix "Bifurcation of Stationary Solutions of hydrodynamical Equations," in D. Ruelle and F. Takens, "On the Nature," 189-191.

when Ruelle spent the academic year at the IAS in Princeton, he went on a tour in order to spread the word about his new model for turbulence. Most notably, he spoke at Boston, where he met Harvard physicist Paul C. Martin who remembers telling him about Edward Lorenz's work;[247] and at Indiana University in January 1971, where Eberhard Hopf was still teaching. He participated in the conference on "Statistical Models and Turbulence" held at La Jolla in July 1971. He also gave series of lectures on turbulence at Boulder and Brandeis. His model was not always well received, since Ruelle recalls C. N. Yang, from SUNY, Stonybrook, joking about his "controversial ideas about turbulence."[248]

But clearly, at that time, Ruelle hardly considered this new orientation in his research as something that should shape his invitation strategy for the IHÉS. Indeed, during most of the spring term 1971, he clashed by mail with Motchane's ambition of hiring as many as three more permanent professors of mathematics. Feeling that "invitations in domains that interest me are sacrificed," Ruelle voted against all three nominations.[249] Coming back to Bures after having spent the year at Princeton, Ruelle envisaged, with the presence of Eliott Lieb, a year 1972-73 with an emphasis on statistical mechanics. In January, he proposed a "grandiose program" for 1973-74; he planned to invite "the big people of constructive field theory, which is probably the

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[247] In the interview of Paul C. Martin conducted by the author (7 May 1996), it was not exactly clear when this happened. It might have been later (in 1973 or even 1975), since Ruelle did not start addressing the Lorenz attractor before the summer of 1975. Martin however remembers that it took a while for Ruelle to see the relevance of Lorenz's work.

[248] D. Ruelle, *Chance and Chaos,* 66.

hottest thing in mathematical physics nowadays." In February, he was in favor of organizing an "astrophysical year" for 1974-75 in collaboration with the CNRS, contending that "one can think that, in the future, the most active developments of theoretical physics will be obtained in, or via, astrophysics." None of these projects had much to do with his work on dissipative systems.[250]

The only exception was the invitation Ruelle sent to Jerrold Marsden for the academic year 1971-1972.[251] A student of Wightman's at Princeton University, Marsden had written his thesis "with much inspiration from Ralph Abraham" in 1967-68. A great "note-taker," he helped Abraham prepare the publication of his famous lecture notes.[252] Having read one of V. I. Arnol'd's papers in fluid mechanics, he moved to Berkeley where he attended Smale's seminars, teamed up with David Elbin, and started to work on hydrodynamics.[253] This was not a popular field for theoretical physicists at the time; he remembers having been told to "stop wasting [his] time."[254] Like Lanford and Ruelle, Marsden was the kind of mathematical physicist who could

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249 Lettre de David Ruelle à Léon Motchane (25/6/71). Offers were made to Bomberi, Langlands, and Armand Borel, all of whom rejected the offer. See *Comité scientifique* (25/6/71). Arch. IHÉS.

250 *Rapport du Comité scientifique* (22/10/71); *Petit Comité scientifique* (10/1/72); *Petit Comité scientifique* (7/2/72). Arch. IHÉS.

251 *Comité scientifique* (28/6/70). Arch. IHÉS.

252 R. H. Abraham and J. E. Marsden, *Foundations of Mechanics* (New York: W. A. Benjamin, 1967). It was Wightman who, in interview, called Marsden a "great note-taker."

253 V. I. Arnol'd, "Sur la géométrie différentielle des groupe de Lie de dimension infinie et ses applications à l'hydrodynamique des fluides parfaits," *Annales de l'Institut Fourier de Grenoble,* 16(1) (1966): 319-361.

254 By Wigner or Wheeler, he was not sure. See J. Marsden's acceptance speech in "1990 Norbert Wiener Prize in Applied Mathematics Awarded in Columbus," *Notices of the American Mathematical Society,* 37 (1990): 808-811, 810.

build bridges across disciplines. But Marsden was not part of the stability theory community.

"I think that Marsden's visit will be very valuable from a scientific viewpoint," Ruelle wrote to Motchane; he "should in particular interest Thom."[255] Indeed, in the spring term of 1972, while Abraham was also at the IHÉS, Marsden gave several talks in Thom's seminar, in which he addressed the issue of the "Onset of Turbulence."[256] Marsden also used the opportunity of being at the IHÉS to work on papers in which he studied the Hopf bifurcation, and reviewed different models for the onset of turbulence including Ruelle and Takens's.[257]

That same year, the theoretical physics seminar of the IHÉS welcomed a talk by Paul C. Martin, who had showed an early interest for the Lorenz model, seeing it as an instance where turbulence set in suddenly without following Landau's scheme. Like Ruelle a specialist in axiomatic quantum field theory, statistical mechanics, and the many-body problem, Martin was spending the year at Saclay and Orsay in de Gennes's group. His study of phase transitions led him to believe that he could have something to say about the onset of turbulence. On April 12, 1972, he gave a talk in the Ruelle-Michel seminar entitled: "Schwinger-Feynman Techniques in Classical

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[255] Lettre de David Ruelle à Léon Motchane (8/12/70). Arch. IHÉS.

[256] R. Abraham's seminar in Thom's applied global analysis seminar (22/2/72) was "Hydrodynamic Bifurcations according to Ruelle and Takens;" D. Ruelle (28/2/72): "Bifurcations with Symmetry;" J. Marsden (13/3/72): "The Onset of Turbulence." *Rapport scientifique 1972.* Arch. IHÉS.

[257] J. E. Marsden, "A Survey of Some Recent Applications of Global Analysis to Hydrodynamics," *Quatrième rencontre entre mathématiciens et physiciens, 1-5 mars 1972* 4(2), Supplement 2 to *Publications du département de mathématique de l'Université de Lyon-1,* 9 (1972): 194-207; "The Hopf Bifurcation for Nonlinear Semigroups," *Bulletin of the American Mathematical Society,* 79 (1973): 537-541.

Fluid Dynamics."[258] One should further note the presence at the IHÉS in 1972 of O. E. Lanford, who however spoke on statistical mechanics.

Meanwhile, in 1972, Ruelle gave only two talks devoted to "turbulence et attracteurs étranges" (at Moscow and in Israel), while delivering many more on statistical mechanics. That year, he nonetheless wrote two papers dealing with the Hopf bifurcation.[259] Very different from one another, these two papers show that Ruelle's incursion in the field of dynamical systems was turning into an actual change in orientation. This time accepted by the *Archive for Rational Mechanics,* his paper on "Bifurcations" was a more formal treatment of the Hopf bifurcation, but significantly it scarcely dealt with phenomena which are today associated with chaos and turbulence. Formal bifurcation theory did not allow him to go further.

The other paper, on chemical oscillations, is much more interesting from the point of view of the history of chaos. There, a number of important themes recurrent in later works on chaos theory were raised and argued for the first time. Having visited Prof. B. Chance in his group on chemical oscillations at the University of Philadelphia, Ruelle grasped the possibility of applying the Ruelle-Takens model to a totally different case. One should note that Sattinger had also remarked on the similarity of hydrodynamic and chemical stability, as early as 1971.[260] These parallels

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258 Interview of P. C. Martin by the author (7 May 1996); *Rapport scientifique 1972.* Arch. IHÉS.

259 D. Ruelle, "Bifurcations in the Presence of a Symmetry Group," *Archive for Rational Mchanics and Analysis,* 51 (1973): 136-152; "Some Comments on Chemical Oscillations;" *TSAC,* 91-108 and 109-115.

260 D. H. Sattinger, "Stability of Bifurcating Solutions," 165; where he cited G. R. Gavalas, *Nonlinear Differential equations of Chemically reacting Systems* (New York: Springer, 1968).

were one of the themes of the 1972 Battelle Summer Institute, where Grégoire Nicolis was present representing Prigogine's School in Brussels, which was interested in the topic.[261]

But Ruelle went further than this; he also put forward two important characteristics of turbulence as he saw it. Whether manifested in fluids or chemical reactants turbulence involved nonperiodic solutions and "sensitiveness to initial conditions."[262] These properties opened the door for careful experimental confirmation of his theories. The conclusion was clear to Ruelle:

> The bifurcations that lead to such "turbulent" solutions are difficult to study mathematically, but turbulent time behavior should be easy to recognize when it occurs experimentally. This behavior might easily be overlooked in chemical systems as "messy, unusable data." The phenomenon is in fact respectable.[263]

Ruelle, it seems, was now expecting a confirmation of his model not from theory, but from experiments.

Only during the spring of 1973 did Ruelle finally propose, with Thom's support, that a year starting in the spring of 1975 be devoted to "Dynamical systems, statistical mechanics, and turbulence" at the IHÉS. Coming back from Moscow, Ruelle also suggested that Soviet mathematicians Arnol'd and Sinaï be considered for permanent professorships at the IHÉS, although under the conditions reigning in the

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261 See e.g. I. Prigogine, *Introduction à la thermodynamique des processus irréversibles* (Paris: Dunod, 1968), chap. 8, where chemical oscillations, Bénard oscillations, and the Lotka-Volterra equations were treated as instances of instability. About this, see Chapter VIII above.
262 Sensitivity to initial conditions was already mentioned by D. Ruelle, "Strange Attractors as a Mathematical Explanation," 293.
263 D. Ruelle, "Some Comments," 70; *TSAC*, 114.

USSR, this was difficult to envision. Finally, his inroad into the theory of dissipative dynamical systems had begun to shape Ruelle's duties at the IHÉS.

Simultaneously, he came in contact with Rufus Bowen, whom we may recall as "Smale's best student." On June 4, 1973, Ruelle wrote to Kuiper in the letter mentioned above that he wanted to invite him to the IHÉS: "I also had useful scientific contacts [while in California, from April to August 1973], particularly with Bowen. . . . He impresses me very much, and I am in favor of making soon a firm and attractive offer [to him]."[264] In fact, the case of Bowen is revealing of the passing of the initiative from Thom to Ruelle in the pursuit of the best use of dynamical systems theory for the study of physical systems. Indeed, just the year before, it was to Thom that Bowen had written in order to be invited to the IHÉS (although, in the end, he chose not to come).[265] With Bowen, Ruelle would soon publish the article marking his full involvement in dynamical systems theory.[266] This paper marks the meeting point of his earlier work on statistical mechanics and his new concerns for dissipative systems, which would set the direction of his future research, and provide an axis around which the activities of the IHÉS in this domain would be built.

It was in the same letter to Kuiper that Ruelle suggested that James Serrin, Daniel Joseph, and David Sattinger be invited to the IHÉS. At the meeting of the Scientific Committee on November 16, 1973 the impulse was definitely given for

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[264] Lettre de David Ruelle à Nicolaas Kuiper (4/6/73); *Comité scientifique* (24/3/73); *Rapport scientifique 1973*. Arch. IHÉS.
[265] Lettres de Rufus Bowen à René Thom (24/2/72); and (24/5/72). *Comité scientifique* (14/4/72). Arch. IHÉS.
[266] R. Bowen and D. Ruelle, "The Ergodic Theory of Axiom *A* Flows," *Inventiones Mathematicae,* 29 (1975): 181-202; *TSAC,* 153-174.

developing the activities of the IHÉS around the themes of dynamical systems, turbulence, and statistical dynamics, but now under Ruelle's lead rather than Thom's,. Moreover, David Sullivan, from MIT, then visiting at Orsay and the IHÉS, was now considered as the more likely prospect for the creation of a new position.

> The range of interest and our expectation of necessary moves to new fields in the course of life seems greater with Sullivan [as opposed to B. Mazur, and J. Mather, also considered]. Sullivan attacks hard and most fundamental problems, and does aim at finding the e[ss]entials. He has a great technical power at the same time and uses it.[267]

With the offer of a position, he became interested in "the theory of singularities of complex manifolds and in dynamical systems. Both of these topics," Kuiper wrote, "presently are in full activity, notably in the Paris region and more especially at the IHÉS (Thom-Ruelle-A'Campo)."[268] Sullivan accepted the offer and began at the IHÉS in September 1974.[269] Clearly, the IHÉS was now launched as a major center in the theory of dynamical systems, still envisioned in its mathematical aspects, but also with respect to its relevance for mathematical physics, particularly turbulence and statistical mechanics.[270]

In the later part of the 1970s, the IHÉS thus became one of the breeding grounds from which chaos theory emerged by the end of the decade. But at the same time, the landscape became much broader with infusions from a wide array of backgrounds. This central issue in the emergence of chaos will be partly addressed in

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267 Mémo, by N. Kuiper; *Comité scientifique* (16/11/73). Arch. IHÉS.
268 Lettre de Nicolaas Kuiper à François Le Lionnais (24/4/74). Arch. IHÉS.
269 *Comités scientifique* (19/4/74). *Compte-rendu* (dated 8/5/74). Arch. IHÉS.
270 Let me furthermore note a talk by C. Foias, from Budapest, on November 5, 1974 on "Connections entre les équations de Navier-Stokes et la théorie de la turbulence." *Rapport scientifique 1974.* Arch. IHÉS.

Chapter VIII below. But before it will be useful to sketch out the activities of Joseph, Sattinger, and Iooss, who would play a role in this emergence.

c) Stability Theorists in the Age of Chaos

Gérard Iooss, Daniel Joseph, and David Sattinger had too fine a knowledge of the bifurcations involved in hydrodynamic systems, and of the differences of each particular situation, to convert wholly to the Ruelle-Takens model. Each of them would go on working on the problem and achieve imposing synthetic pictures of nonlinear stability theory. At the same time, they worked on building linkages between this theory and related ones which explored bifurcations and loss of stability in many physical systems. In this sense, they all became actors in the emergence of chaos.

With his interest in bifurcation theory, it was only fitting that David Sattinger, from the Mathematics Department of the University of Minnesota, was the first of the three to come to the IHÉS. He came as one of the "attractors" for the "year" on dynamical systems and ergodic theory planned for the spring semester, 1975, at the same time as Lanford and Bowen, among others.[271] Sattinger was also the first of the three to publish a monograph dealing with their common endeavor.[272] Consisting of lecture notes for a course given at the University of Minnesota in 1971-72, this book barely touched with the Ruelle-Takens model.

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[271] Sattinger spoke "On the Free Surface of a Viscous Fluid in Motion," on May 14, 1975. *Rapport scientifique, Année 1975 - Séminaires et conférences,* 8. Arch. IHÉS.
[272] D. H. Sattinger, *Topics in Stability and Bifurcation Theory,* Lecture Notes in Mathematics, 309 (Berlin: Springer, 1973).

As early as 1967, Clifford Truesdell had started pushing Daniel Joseph, from the Department of Aerospace Engineering and Mechanics at the University of Minnesota, to write a book on the *Stability of Fluid Motions.* "The theory of stability has developed so rapidly since 1967 that the book I might then have written would now have much too limited a scope."[273] In 1976, he finally published a two-volume compendium of hydrodynamic stability theory. Interestingly, Joseph acknowledged Sattinger, for reminding him that stability theory was part of mathematics, and Fritz Busse, for reminding him that it was part of physics as well. Restricting the Ruelle-Takens model to a very small part, Joseph's book dealt in detail with all simple cases of fluid flows: Poiseuille, Couette, Bénard, the flow past a sphere, etc.

His comments about Hopf, Landau, and Ruelle-Takens were particularly revealing of stability theorists' attitude in the face of the Ruelle-Takens model. In a long note, he deplored the fact that this model "left vague" many characteristics of the flow. Joseph observed that it rested on some restrictive assumptions, needing "justification at many points," especially since catalogues of attractors for ordinary differential equations, not to mention the Navier-Stokes equations, "still elude[d] analysis." In summary, Joseph thought that it was "a step forward to think of non-periodic, phase-mixing attractors in the description to turbulence." But it clearly was far from supplying the universal "mechanism for the generation of turbulence" that Ruelle and Takens had claimed. In particular, it led to no description of the spatial features of turbulence and forgot about the wholly different possibility of subcritical

[273] D. D. Joseph, *Stability of Fluid Motions,* Springer Tracts in Natural Philosophy, 27-28 (Berlin: Springer, 1976), 1: v.

bifurcations.[274] Already considered by Ruelle in 1973, Joseph was formally invited to spend the academic year 1979-80 at the IHÉS. He did indeed come in 1980-81, but spent most of his time, either at Orsay with Roger Tenam, or at Nice with Gérard Iooss.[275]

Their relative skepticism concerning the Ruelle-Takens model notwithstanding, both Sattinger and Joseph played a definite role in the emergence of chaos. Joseph, for example participated in the Gordon Research Conference on "Dynamical Instabilities and Fluctuations in Classical and Quantum Systems." Organized by Paul C. Martin and Jerry P. Gollub and held in New Hampshire on July 19-23, 1976, it was "aimed primarily toward the review of specific time-dependent non-equilibrium phenomena on which experiments are being performed, and an assessment of whether there are helpful mathematical techniques that can be brought to bear." This conference might be interpreted as still another of the birth places of chaos theory.[276] Both Sattinger and Joseph also spoke at the famous conference on

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[274] D. D. Joseph, *Stability of Fluid Motions,* 58-60. Ruelle and Takens's quote is from the Abstract of "On the Nature," 167.
[275] Lettres de David Ruelle à Daniel Joseph (11/1/78); de Daniel Joseph à David Ruelle (10/2/78); de Roger Tenam, Orsay, à Daniel Joseph (2/5/78); de Daniel Joseph à Nicolaas Kuiper (24/5/78); de Nicolaas Kuiper à Daniel Joseph (5/4/79); *Comité scientifique* (23/9/78); *Comité scientifique* (10/3/79); *Assemblée générale* (8/5/78): "l'ouverture voulue par Monsieur Ruelle va dans la direction de la mécanique et de l'Hydrodynamique. La visite du Professeur Daniel Joseph (Minnesota) pour l'année académique 78-79 nous donne un espoir dans cette direction." Arch. IHÉS.
[276] Among those present, one notices G. Ahlers, R. P. Behringer, R. Bowen, B. Derrida, R. J. Donnelly, M. J. Feigenbaum, J. P. Gollub, J. Guckenheimer, H. Haken, D. D. Joseph, E. L. Koschmieder, A. Libchaber, P. C. Martin, J. B. McMaughlin, G. Nicolis, H. L. Swinney, G. Toulouse, R. Williams, and A. T. Winfree. Memo from P. C. Martin and J. P. Gollub to all speakers and suggested chairman (July 6, 1976). I thank P. C. martin for providing me this letter, the program of the conference, and a list of people who attended it.

"Bifurcation Theory and Applications," held in October 1977 under the auspices of the New York Academy of Sciences.[277]

Gérard Iooss, who was the most mathematically-minded of the three, never quite achieved the same international status as Joseph and Sattinger in early chaos conferences (perhaps because he often wrote in French). In 1972-73, he taught courses at Orsay where he did not consider the Ruelle-Takens model. In 1975, however, directly inspired by this model, he studied analytically the bifurcation of periodic solutions into an invariant torus.[278] Having moved to Nice, he started collaborating with Alain Chenciner, a student of Thom's, moving towards a dynamical systems analysis of the onset of turbulence. In Nice, a "meeting between physicists and mathematicians about non-linear problems and their applications" was organized in September 1977 by J. Coste, P. Coullet, and A. Chenciner. There, Iooss carefully reviewed the Ruelle-Takens scenario, noting that much computation and experimental work remained to be done in order to check it rigorously.[279] In a 1979 monograph on bifurcation theory, written for a course he taught at the University of Minnesota in the fall of 1977, he did not dwell on Ruelle-Takens. But in 1981, he was one of the

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[277] D. H. Sattinger, "Spontaneous Symmetry Breaking in Nonlinear Problems," and D. D. Joseph, "Factorization Theorems and Repeated Branching of Solutions at a Simple Eigenvalue," *Bifurcations Theory and Applications in Scientific Disciplines,* ed. O. Gurel and O. E. Rössler (New York: New York Academy of Sciences, 1979): 49-63; and 150-167.

[278] G. Iooss, *Bifurcation et stabilité;* "Bifurcation of a Periodic Solution of the Navier-Stokes Equations into an Invariant Torus," *Archive for Rational Mechanics and Analysis,* 58 (1975): 35-36; "Sur la deuxième bifurcation d'une solution stationnaire de systèmes du type Navier-Stokes," *Archive for Rational Mechanics and Analysis,* 64 (1977): 339-369.

organizers of a summer school in Les Houches devoted to the "Chaotic Behaviour of Deterministic Systems."[280] Like Joseph and Sattinger, Iooss had become a chaologist, albeit maintaining some level of skepticism.

For stability theorists, then, the Ruelle-Takens model remained at best an interesting suggestion which they sometimes exploited. But it did not solve the problem of turbulence because its mathematical foundations remained somewhat shaky. Their modeling practice received an infusion from dynamical systems theory and they started to look for attractors, but the problems were to be solved case by case, by a careful analytic study of the equations, still using the methods of functional analysis: existence and uniqueness theorems. I believe that this historical process was what Ruelle was describing when he wrote:

> the "theory of chaos" . . . is a specific bag of tools. . . . In the favorable cases, the new ideas are integrated and accepted (so that we know that turbulence is chaotic . . .). Then each domain of research resumes its own individuality, and can no longer be called "a branch of the theory of chaos." The whole process takes a few years. Very roughly I would say . . . from 1971 to 1986 for turbulence.[281]

For the general community of physicists, however, the lesson to be drawn from the Ruelle-Takens model was different. Numerical and laboratory confirmations seemed sufficient to show the fruitfulness of the approach. It had introduced an alternative

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279 G. Iooss, "Bifurcations successives et stabilité," *Journal de physique,* 39 (1978), Colloque C5 ["Rencontre entre physiciens et mathématiciens sur quelques problèmes non linéaires et leurs applications"]: 99-105.
280 G. Iooss, *Bifurcation of Maps and Applications* (Amsterdam: North-Holland, 1979; G. Iooss, R. H. G. Hellerman, and R. Stora, eds., *Chaotic Behaviour of Deterministic Systems: Les Houches Summer School, Session XXXVI, 1981* (Amsterdam: North-Holland, 1983).
281 D. Ruelle, "Introduction," *TSAC,* xiv-xv.

modeling practice for physicists that was there to stay. This process, a complex one, will be the topic of Chapter VIII below.

7. CONCLUSION: BOURBAKI AND THE COMPUTER

The above summarily traced back shifting commitments in favor of the Navier-Stokes equations by various groups of scientists working on the turbulence problem. Once it was recognized that even in very simple cases, some exact solutions of the Navier-Stokes equations could not be describing certain observed flows, this problem always posed a challenge to the belief that these equations were a fundamental law of physics. Successes confirmed this belief; difficulties and controversies made the foundation shakier. Ultimately, commitments to the Navier-Stokes equations stemmed from a variety of evidence, taken away from widely different theoretical approaches, experiments, and numerical computation.

But throughout this history, the final aim of finding an equation that would faithfully describe the dynamics of fluid flows was almost never questioned. One thought of tinkering with the Navier-Stokes equations, but never that such a description of fluid flows could be intrinsically faulty. What the model suggested by Ruelle and Takens suggested, however, was much more radical, and this was shared by some of the later stability theorists' approaches. In short, they proposed that topological methods might be powerful enough to bypass the problem. The Navier-Stokes equations might—or might not—be a totally faithful description of fluid flows. This did not matter anymore. The only portion of the prior beliefs that remained was that some differential equation, endowed only with the few necessary properties, did indeed accurately describe the flow, but one did not need to know it in order to extract fruitful information about turbulence. This was the meaning of "genericity," which would often be replaced by "universality" during the following decade.

Instead of being based on a specific equation, the modeling practice of some scientists working on turbulence began to be founded on experimentally observable topological features which were robust with respect to variations of the no-longer-so-fundamental differential equation. This entailed a change in the basic building blocks of mathematical models for the onset of turbulence. They now were topological features, as opposed to analytic solutions of an equation. This of course implied that the mathematical techniques used to study the onset of turbulence became topological, coming from dynamical systems theory or functional analysis. Finally, the consequences derived from a topological study of fluid flows were interpreted as a better understanding of turbulence, even while specific mechanisms for the generation of turbulence remained elusive. In short, this modeling practice dispensed with prior aims at avoiding turbulence. Understanding replaced action as the main goal of model-building for turbulence.

"The general lesson" of a decade of work on deterministic theories of turbulence, David Ruelle contended in 1983,

> seems to be that hydrodynamical systems at the onset of turbulence behave very much as generic differentiable dynamical systems in finite dimension. Simple systems of differential equations with arbitrarily chosen coefficients, when studied by digital or analog computers, yield data so analogous to those of hydrodynamical experiments that it is not possible to tell them apart.[282]

[282] D. Ruelle, "Differentiable Dynamical Systems and the Problem of Turbulence;" *TSAC,* 238.

In this situation, what then was the use of the Navier-Stokes equation? This conclusion of Ruelle and Takens was the result of three main strains: earlier models for the onset of turbulence; the adaptation of concepts and practices from dynamical systems theory for which Thom was a major mediator; and Ruelle's earlier commitment to a theoretical physics relying on technical mathematical methods inspired by a Bourbakist attitude. Their suggested model departed in practice from earlier ones. A major lesson was that the topological, structural, yet dynamical study of bifurcations and attractors, as opposed to reduction to ultimate molecules or fundamental laws, could have a high reward.

The following testimony of Leo Kadanoff, a physicist from the University of Chicago who was rather a late-comer to the study of chaos, is revealing of the change in attitude with respect to specific models.

> [A]t a crucial moment, Bob Gamer, a colleague in Chemistry, asks why I am devoting so much work to a particular model system. His implication is that *the model is not so real as to be of practical interest, and perhaps not so deep as to have real intellectual interest*. . . I know that his implied criticism is right. So I resolve to learn something new. The new subject I find is dynamical systems theory.[283]

Significantly, this kind of remark had already been made more than fifty years before by George D. Birkhoff:

> At a time when no physical theory can properly be termed fundamental—the known theories appear to be merely more or less fundamental in certain directions—it may be asserted with confidence that ordinary differential equations of dynamical origin will continue to hold a position of the highest importance.[284]

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[283] L. P. Kadanoff, *From Order to Chaos. Essays: Critical, Chaotic, and Otherwise* (Singapore: World Scientific, 1993), 386. My emphasis.
[284] G. D. Birkhoff, *Dynamical Systems* (Providence: AMS, 1927), vi.

In the 1970s, a certain disenchantment with reductionist approaches started more and more to touch physicists, not all of them of course, but a significant portion of the profession. As Princeton physicist Philip Anderson once contended:

> The ability to reduce everything to simple fundamental laws does not imply the ability to start from those laws and reconstruct the universe. In fact, the more the elementary particle physicists tell us about the nature of the fundamental laws, the less relevance they seem to have to the very problems of the rest of science, much less society.[285]

Fundamental laws, some began to think, were not so certain anymore. But more importantly, they felt they could provide meaningful models that did not rely on a perfect knowledge of God's mind, to use one of Einstein's phrases. The dynamical systems approach, and soon chaos theory, could lead to profound understanding of natural phenomena, without much ontological commitment to fundamental equations. The question is why this happened.

No simple answer will suffice. In this dissertation, it is argued that new modeling practices stemmed from a wide variety of sources, ranging from technical innovation in mathematics to more diffuse cultural factors. From the above discussion two hypotheses surface, which hardly settle the question. First, it has been tempting, by studying Ruelle's career trajectory, to conclude that a certain 'Bourbakization' of theoretical physics allowed the emergence of a new alternative for the physicist's modeling practices. Second, we may think that a very important change in the material conditions for the practice of theoretical physics made itself felt, namely the rising availability and use of computers.

A feeling among mathematicians that their methods allowed them to understand scientific problems, sometimes more deeply than the specialists themselves, is hardly new, as the following statement of Garrett Birkhoff will make explicit:

> During the [second world] war I found that my ability to diagnose fluid mechanics even with a rather limited knowledge of it . . . was very useful to both the Army and the Navy. I decided that if opportunity permitted after the war I would try to see what could be done scientifically . . . to treat some of the questions.[286]

From the early 1960s to the early 1980s, at least, the Institut des hautes études scientifiques was a singular place where a fruitful dialogue between mathematicians and physicists, resulting in a sharing of concepts and practices, was established and sustained. Physicists used the latest mathematical tools available, organized in a Bourbakist way. An intense activity on the part of some physicists took place, aimed at studying physical systems with the greatest generality possible. Bourbaki wished to do away with most properties of the real numbers when he wanted to establish some theorem that only required, for example, the group structure. Similarly, Ruelle and Takens endeavored, in their famous paper, to uncover the structure (almost in Bourbaki's sense) of turbulence as such, without relying on unnecessary, superfluous properties of the Navier-Stokes equations. In this sense, one may speak of a Bourbakization of the physicist's modeling practice.

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[285] Quoted in M. M. Waldrop, *Complexity: The Emerging Science at the Edge of Chaos* (New York: Simon & Schuster, 1992), 81. See also P. W. Anderson, "La grande illusion des physiciens," *La Recherche,* 11 (1980): 98-102.
[286] G. Birkhoff's interview, *Mathematical People: Profiles and Interviews,* ed. D. J. Albers and G. L. Alexanderson (Boston: Birkhäuser, 1985), 13.

Of course, one cannot go too far in this direction. This new alternative for the physicist's modeling practice was not the consequence of just one paper by Ruelle and Takens, no matter how seminal it may have been. Many physicists did not flirt with Bourbaki. The next chapter, which examines the way in which Ruelle's innovations would be taken up by a variety of scientists, will plainly show this. Moreover, principal propagandists for alternative modeling practices, such as Thom, started very early on to break free from Bourbaki's dogmatism, and with good reason since they actually looked for original ways to escape the ivory tower of pure mathematics.[287]

On the other hand, changing conditions for the modeling of natural phenomena—the rising availability of computers—played a definite role even for those who chose not to rely on them. Again, the case of Ruelle provides an interesting perspective one this impact of computers. Even if he never used them in any crucial way, Ruelle clearly saw, as early as 1964, that theoretical physics had to change its ways confronted with

> an event which no doubt shall have profound repercussions on the evolution of statistical mechanics, [namely] the development of electronic computers. These allow not only to treat numerically problems up until now unreachable, but also to reproduce the evolution of systems [involving] a few hundreds of particles, allowing their "experimental" study.

The lesson Ruelle took away from this "event" was that techniques of approximation had to be adapted to the new situation, "or die." But what interested him most was that even "techniques for the rigorous study" of statistical mechanics

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[287] Let me mention that an attempt at formalizing physical way in a strict Bourbakist way was made by a philosopher: W. Stegmüller, *The Structuralist View of Theories: A Possible Analogue of the Bourbaki Programme for the Physical Science* (Berlin: Springer, 1979).

had to be adapted, "for calculators will be more and more able to provide convincing, if not rigorous, answers to the problems of statistical mechanics." That is, Ruelle felt that the study of the conditions in which numerical solutions were, or were not, meaningful was becoming a major task for the mathematical physicist. One had to ponder the way problems were posed in theoretical physics.[288] As Ruelle wrote much later, the computer as such was no solution:

> The use of modern computers . . . has had for example a considerable impact in the studies of hydrodynamic turbulence. But scientific progress requires specific ideas and methods like those constituting the "theory of chaos."[289]

Already in 1964, it seems that Ruelle geared his research program towards a better understanding of the mathematics fundamental to the modeling practices of theoretical physicists, so that the computer be used more efficiently in their discipline. "The question," Ruelle asserted, "will soon be raised to know whether it is the calculator or the researcher that is the tool of the other."[290]

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[288] All of Ruelle's quote above are taken from: Lettre de Léon Motchane à Lucien Malavard (17/11/64), which included Ruelle's own description of his project for a contract with the DRME. Arch. IHÉS.
[289] D. Ruelle, "Introduction," *TSAC,* xiv.
[290] Lettre de Léon Motchane à Lucien Malavard (17/11/64). Arch. IHÉS.