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Writing the History of Dynamical Systems and Chaos: *Longue Durée* and Revolution, Disciplines and Cultures[1]

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Between the late 1960s and the beginning of the 1980s, the wide recognition that simple dynamical laws could give rise to complex behaviors was sometimes hailed as a true scientific revolution impacting several disciplines, for which a striking label was coined—"chaos." Mathematicians quickly pointed out that the purported revolution was relying on the abstract theory of dynamical systems founded in the late 19th century by Henri Poincaré who had already reached a similar conclusion. In this paper, we flesh out the historiographical tensions arising from these confrontations: *longue-durée* history and revolution; abstract mathematics and the use of mathematical techniques in various other domains. After reviewing the historiography of dynamical systems theory from Poincaré to the 1960s, we highlight the pioneering work of a few individuals (Steve Smale, Edward Lorenz, David Ruelle). We then go on to discuss the nature of the chaos phenomenon, which, we argue, was a conceptual reconfiguration as much as a sociodisciplinary convergence.

Entre la fin des années 1960 et le début des années 1980, la reconnaissance du fait que des lois dynamiques simples peuvent donner naissance à des comportements très compliqués a été souvent ressentie comme une vraie révolution concernant plusieurs disciplines en train de former une nouvelle science, la "science du chaos." Rapidement, les mathématiciens ont réagi en soulignant l'ancienneté de la théorie des systèmes dynamiques fondée à la fin du XIXème siècle par Henri Poincaré qui avait déjà obtenu ce résultat précis. Dans cet article, nous mettons en évidence les tensions historiographiques issues de diverses confrontations: l'histoire de longue durée versus la notion de révolution, les mathématiques pures versus l'utilisation des techniques mathématiques dans d'autres domaines.

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Après avoir passé en revue l'historiographie de la théorie des systèmes dynamiques de Poincaré jusqu'aux années 1960, nous soulignons les travaux pionniers d'un petit nombre d'individus: Steve Smale, Edward Lorenz, et David Ruelle. Nous poursuivons en discutant la nature du phénomène du chaos qui constitue, selon notre analyse, tant une reconfiguration conceptuelle qu'une convergence sociale et disciplinaire.

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INTRODUCTION

In this article we offer a sociohistorical analysis of a scientific domain which mathematicians prefer to call "dynamical systems theory" while others, including most physicists, still talk about "deterministic chaos theory" or simply "chaos." To us, this terminological disagreement, which, to be sure, is a reflection of disciplinary tensions, also suggests *historiographical* challenges. In particular, we would like to argue that common binary oppositions often useful in the task of historical interpretation (*longue durée* vs rupture, pure mathematics vs so-called applications, epistemological reconfiguration vs social convergence at disciplinary and institutional levels) here form a blockade against a proper understanding of the emergence and development of this prominent scientific domain.

Following the great surge of interest in dynamical systems theory after 1975, scores of scientists—soon joined by vocal science popularizers—were quick at proclaiming a major scientific revolution: "I would like to argue," Ruelle [1992, xiii] wrote, "that we have witnessed (around the decade 1970–1980) a change of paradigms." Chaos brought "a new challenge to the reductionist view that a system can be understood by breaking it down and studying each piece;" it provided "a mechanism that allows for free will within a world governed by deterministic laws" [Crutchfield *et al.* 1986, 48–49]. This was the period when the domain saw its popularity skyrocket and reached wide lay audiences. Faithfully reporting some of the scientists' most enthusiastic opinions, the journalist James Gleick

[1987, 6] went as far as writing in a worldwide bestseller:

> Twentieth-century science will be remembered for just three things: relativity, quantum mechanics, and chaos. Chaos . . . has become the century's third great revolution in the physical sciences. Like the first two revolutions, chaos cut away the tenets of Newton's physics. As one physicist put it: "Relativity eliminated the Newtonian illusion of absolute space and time; quantum theory eliminated the Newtonian dream of a controllable measurement process; and chaos eliminates the Laplacian fantasy of deterministic predictability."[2]

In parallel with these overblown assessments, a more sober reevaluation took place, which was largely due to some mathematicians' reaction against the break sketched above. This time, continuity with the past was emphasized. Contrary to some claims, Poincaré's legacy had never been forgotten and suddenly rediscovered, *not* at any rate by mathematicians. But the swing of the pendulum generated new excesses. Denying any specificity to the sciences of chaos, some suggested that a better way of looking at the so-called revolution might be to see it merely as the application of a mathematical theory, or as the adoption of a "dynamical systems approach to differential equations."[3]

For the historian, to adopt either of these terms—dynamical systems or chaos—therefore is equivalent to espousing either of opposing conceptions of history according to which what happened in the 1970s was done in rupture (chaos) or in continuity (dynamical systems) with the past. The terminological choice moreover encapsulates epistemological models about historical processes involved in the mathematization of science, mere adoption of mathematical techniques by various scientific communities, or interactive reappropriation in which both mathematical tools and theoretical concepts are reshaped. Oscillating between continuity and rupture, mathematics and various other domains of science, epistemology and the social and cultural history of science, the historiography of chaos has been rife with tensions that have still to be carefully analyzed.

In the following we have deliberately adopted a definite point of view, which has both a temporal and a methodological component. We take the emergence of "chaos" as a science of nonlinear phenomena in the second half of the 1970s, not as the mere development and wide application of a certain mathematical theory, but as *a vast process of sociodisciplinary convergence and conceptual reconfiguration.* This position stems from both our experience in dealing with and writing about the history of this domain and our conviction that it is best suited for capturing its historical essence and significance. We adopt this point of view because we think that it helps to cut the Gordian knot posed by other historiographical apprehensions of the domain.

To take an example, it should be obvious to anyone at all familiar with the domain that the roots of "nonlinear science" (whatever this might mean) extend far beyond mathematics alone. In order to come up with an exhaustive historical analysis of these origins one needs to be able to deal at once with domains as varied as fluid mechanics, parts of engineering, and population dynamics. Each of these moreover has variegated histories, in

[2] The three-revolution picture is quite common; for a recent example, see Parrochia [1997]. For an extreme view of chaos as a momentous revolution in the history of humanity as articulated by a pioneer of the domain, see Abraham [1994].

[3] This is the title of a useful survey article by Hirsch [1984]. A nice, if incomplete, illustration of the issues is provided by the debate over Gleick's book and fractals involving John Franks, James Gleick, Morris W. Hirsch, Benoît Mandelbrot and Steven G. Krantz which took place in the *Mathematical Intelligencer* **11** (1989), No. 1: 65–71; No. 3: 6–13; No. 4: 12–19.

constant interactions with plenty of other domains and involving scientists from a variety of backgrounds. Within hydrodynamics alone, the stability of laminar flows, the onset of turbulence, and fully developed turbulence all form important chapters. In the 1940s, for example, Russian mathematician Andrei N. Kolmogorov, German physicists Werner Heisenberg and Carl Friedrich von Weizsäcker, and Swedish physicist and chemist Lars Onsager quasi-simultaneously elaborated various statistical models of developed turbulence. (Let us note here the underdevelopment of the history of fluid mechanics—in itself a telling manifestation of the general neglect of these areas; see, however, Goldstein [1969], Tokaty [1971] and, specifically on turbulence, Battimelli [1984, 1986]).

Another crucial domain where nonlinear phenomena abound is automatic control and regulation. Of concern to scientists as diverse as the Russian mathematician Aleksandr Andronov, the Dutch electrical engineer Balthasar van der Pol, the French physicist Yves Rocard, and the American mathematician Norbert Wiener, the study of stabilization of oscillatory phenomena, resonance, and feedback loops has played a determining role, in particular for cybernetics. (On the history of automatic regulation and control theory, one may turn to Bennett [1993]; on Wiener and cybernetics, see Heims [1980, 1991] and Masani [1990]). In population dynamics, the study of the logistic equation, for instance, in the work of a mathematician like Vito Volterra, a zoologist like Umberto D'Ancona, and a statistician like Alfred J. Lotka is the starting point of a disciplinary lineage that can lead to population biologist Robert May, who figured prominently in the formation of a community of "chaologists" in the mid-1970s (see Israel [1996], Milan-Gasca [1996], as well as May [1973, 1974, and 1976]). And the above listing has by no means any pretense of being exhaustive.

Clearly, the ample and bushy genealogy that we will simply be content of suggesting here for the period before 1960 crucially hinges on our own analysis of the more recent past which is expounded in this article. When viewed as sociodisciplinary convergence and conceptual reconfiguration, "chaos" in the sense of the science of nonlinear phenomena has a prehistory whose accurate analysis must include the narratives sketched above. Total history looms as a tempting, but obviously unachievable, goal. In the rest of this paper, therefore, our focus will strictly remain that domain which was constituted in the 1970s on the basis of a triple confrontation: (1) the mathematical theory of dynamical systems; (2) the study of nonlinear, disordered, turbulent phenomena in the natural and/or technological world; and (3) a technology, that is, the computer. *A priori* our main goal will be to give conditions for making intelligible this very moment and configuration. As a result our essay suffers from imbalance: in the first section, dealing with the history to 1960, the main emphasis will be put on mathematics while for the later period we also insist on the role of scientists, problems, and works emerging from outside the mathematicians' discipline or professional community (most notably mathematical physicist David Ruelle and meteorologist Edward Lorenz).

In our task we are faced with three main methodological difficulties. First, by the numerous historical references they regularly mobilized, the actors themselves compel us to embrace duration of more than a century. The *longue-durée* history to which they appeal, however, cannot be subsumed under large-scale overviews unfolding uniformly in time, and this for two main reasons: (1) not surprisingly, the history mobilized by actors often is upon closer examination much more textured than they make it appear; and (2) more interestingly, their frequent appeal to the past has an active role to play in the development of the field, and history loops back in a fascinating way. We must therefore insist on these few intense short periods when the configuration of large domains, together with their relationship to the past, shifts radically. The second methodological difficulty that we face is the necessity of grasping the history of, not a theory within pure mathematics, but a mathematical theory in *constant* interaction with a large number of disciplines belonging to the physical sciences (physics, fluid mechanics), to the engineering sciences (automatic regulation), and even to the life sciences (population dynamics, epidemiology). Finally, a third difficulty lies in the consideration of the distinct sociocultural configurations shaping the manners in which interactions take place and thus sometimes strongly imprinting the unfolding history. Without taking these into account, the numerous instances of neglect of fundamental concepts, delays in adopting fruitful methods, sudden rediscoveries, and reconfigurations of the domain cannot be fully understood. A last, but not least, difficulty consists in handling these constraints together.

Au: as meant? often, "natural sciences" "life sciences."

Let us be more precise about these difficulties which we feel are not often directly addressed by the historiography of mathematics. The historical analysis of a proper mathematical theory from Poincaré to the present raises the general problems of long-term history. The history of mathematics provides an abundant corpus of such studies: history of the resolvability of algebraic equations and Galois theory, history of the concept of function, history of calculus, history of groups, and so forth (see, e.g., Kiernan [1971], Youchkevich [1976], Bottazzini [1986], and Wussing [1969]). In these examples, the viewpoint always is that of a conceptual history, in which the *Leitfaden* is the evolution of concepts, tools, and methods usually relatively independently from the wants of science or socio-disciplinary constraints. In our case, concepts and results of dynamical systems theory have repeatedly stemmed from specific problems arising in various domains: the three-body problem in celestial mechanics; equilibrium configuration of rotating fluids; resonance in electronic circuits; predator–prey equilibrium; not to mention questions emerging from the encounter of mathematics with statistical mechanics such as ergodic theory [Lo Bello 1983]. We must therefore write the history of a mathematical theory (or loose set of theories) that is used in—and not "applied" to—other domains. We must also follow how these other domains receive the theory and how the theory itself responds to outside requests. Finally we must take into serious consideration the sociodisciplinary configurations in which these interactions take place. These are more than absolutely necessary demands; this exercise leads us to a totally different epistemological picture of the mathematized sciences and the way in which they are constituted.

If the model of a *pure science* that progresses autonomously, and—only downstream and subsequently—generates applications has been largely abandoned by historians of science, few alternative models are available for our purpose. One that offers itself, often labeled by the catchall "science studies," has shown its fruitfulness in many exemplary studies dealing with short temporalities. This line of research is renowned for its study of controversies, especially in the case of the laboratory sciences.[4] In general, it has exhibited the co-construction of the epistemological and social aspects of science—the parallel, simultaneous reconfiguration of knowledge and socio-disciplinary groups. (For example, Kay [2000] has shown how in the 1950s the general context of World War II and the Cold War shaped genetic research programs and led to their formulation in terms of codes and information theory, which was not *a priori* obvious.) But this historiography has not convincingly shown that

[4] From a huge literature, let us note Shapin and Schaffer [1985]; Collins [1985]; Pickering [1984]. But one may also mention the beautiful study of Martin Rudwick [1985] whose subject matter is a historical science: geology.

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it can take up the challenges of long-term histories. In particular, the difficulty remains of describing how such "co-constructed" results are stabilized and integrated in scientific edifices that withstand the ravages of time (theories, practices, etc.).[5] Adapting the methods and questionings of general history, a few attempts have been made to integrate the history of mathematical knowledge with that of mathematical practices, whether within or outside the academic milieu.[6] For the topic at hand, the work done on statistics and probability theory, which is inspired by this tradition, gives to us to ponder. (About probability theory, see Coumet [1970], Hacking [1990], and Daston [1988]; on statistics, see MacKenzie [1981], Porter [1986, and 1995], Desrosières [1993], Brian [1994], and Armatte [2001]). In particular, it shows that since statistics—whose very name derives from the "State"—is not merely a pure mathematics theory, its history is incomprehensible without an analysis of public and medical statistics, and even eugenics and demographic statistics, too. (On demography, see Le Bras [2000], which details how this field was tied to statistics as early as 1662 and how this encounter changed the nature of science and its relation to politics.) To us, these works are suggestive of the way in which some socio-disciplinary interactions and institutional contexts have molded certain aspects of its theoretical development. Similarly, the attention paid recently to the notion and history of mathematical modeling appears promising of new directions. (After a long period of neglect, the historical literature on modeling and computer simulations seems promised to rapid expansion: see, e.g., Morgan & Morrisson [1999], Sismondo and Gissis [1999]).

In this paper, we have taken up the challenge of combining the *longue-durée* history of dynamical systems theory, which itself is in constant transformation and unfolding on distinct geographic and cultural terrains, with that of the multiple interactions and reconfigurations among mathematics and the physical, life, and engineering sciences, which in the 1970s accelerated in a speedier, denser chronology, and led to the emergence of "chaos." Our argument is divided in four roughly chronological parts. The first part discusses the historiography of dynamical systems theory over the relatively long period up to the 1960s. The second part focuses on three pioneering figures (Steve Smale, Edward Lorenz, and David Ruelle) who in their own field introduced elements of reconfiguration. In the last two parts, the rupture which gave rise to chaos is discussed first (in part 3) by assessing the role played by dynamical systems theory, in relation to other factors, in the social convergence and conceptual reconfiguration, and second (in part 4) by emphasizing new elements coming from physics especially. In conclusion, the question of the cultural impact of chaos is raised. Since we cannot be exhaustive, we have chosen to ease the clarity and synthetic nature of our text by introducing each of the next section with a thesis that summarizes in a schematic way the main characters of each period.

THESIS 1: THE *LONGUE-DURÉE* HISTORY OF DYNAMICAL SYSTEMS THEORY

From Poincaré to the 1960s, the mathematical study of dynamical systems developed in the course of a longue-durée history that cannot be unfolded in a cumulative, linear

[5] See MacKenzie [1981]. This question is discussed by Hacking [1999]. Concerning the *longue-durée* history of 20th-century particle physics, we have, however, the example of Galison [1997], an ambitious study dealing with cognitive, practical, contextual, and cultural aspects simultaneously.

[6] In Bottazzini & Dahan [2001], for instance, the notion of image or representation of mathematics provides a key for articulating the proper disciplinary level, socioinstitutional levels, and finally, a more diffuse cultural level.

fashion. In particular, this history is not reducible to that of a mathematical theory (which might be called "dynamical systems theory" or the "qualitative theory of differential equations") made by academic mathematicians who would have all contributed a stone to the final edifice. In fact, this history unfolds along various geographic, social, professional, and epistemological axes. It is punctuated by abrupt temporal ruptures and by transfers of methods and conceptual tools. It involves scores of interactions among mathematics, engineering science, and physics along networks of actors with their specific research agendas and contexts. Finally, it is characterized by countless instances of looping back to the past, to Poincaré's work in particular, which are so many occasions for new starts, crucial reconfigurations, and reappreciation of history.

1.1. An Undeniable Point of Origin: Poincaré

Due to the novelty, the variety of tools, concepts, and methods deployed by Henri Poincaré, there can be no doubt whatsoever that his *œuvre* is the point of origin of the domain under consideration here—dynamical systems and chaos—and the cornerstone on which it was built. Whatever conflicts have arisen between historiographical viewpoints, the recognition of Poincaré as the true founder and major theoretician of the domain has been unanimous. In his scientific lifework, he indeed articulated four especially important themes for our concern: (1) the *qualitative theory* of differential equations; (2) the study of *global stability* of sets of trajectories (instead of focusing on single solutions); (3) the notion of *bifurcation* together with the study of families of dynamical systems depending on a parameter; (4) the introduction of *probabilistic* concepts into dynamics, with respect to ergodic theory and the exclusion of exceptional cases. In the course of the following century, each of these four broad themes was mobilized either jointly or separately. Associated with fundamental concepts and methods, they would set the outline of the domain.[7]

In his series of memoirs "Sur les courbes définies par une équation différentielle" published between 1881 and 1886, Poincaré forged the elements of a qualitative, *geometric* analysis making it possible, when differential equations are not solvable, to know the general look of the solutions, i.e., to know their *phase portraits* and state global results. (The *phase space* is the space of the bodies' position and momentum (velocity). It has thus $6n$ dimensions when n is the number of bodies under consideration. This dimensionality can, however, be reduced by symmetry considerations. The *phase portrait* is the set of solution curves traced in phase space.) As a first step, he established a general classification of solutions in two dimensions in terms of *singular points* (centers, saddle points, nodes, and foci). Using the topographical analogy, he developed the notion of the index of a curve providing a first local qualitative result. Discussing in phase space, he introduced the notion of transverse arc, allowing the reduction of a curve to a series of points on the real line. His fundamental result was the following: among all the curves not ending in a singular point, some are periodic (they are *limit cycles*), and all the others wrap themselves asymptotically around limit cycles. Starting from behavior in the neighborhood of singular points, limit cycles and transverse arcs therefore provide a rather precise knowledge of trajectories.

[7] The literature on Poincaré's contribution to dynamical systems theory is immense; surveys can be found in Chabert and Dahan Dalmedico [1992] and Bottazzini [2000]. Among the studies devoted to qualitative theory of differential equation, let us mention Hadamard [1912a], Gilain [1991], Gray [1992], Mawhin [1996], and Parker [1998]; concerning celestial mechanics, see Hadamard [1912b], Anderson [1994] and Barrow-Green [1994; 1997].

Unfortunately this theorem, which constrains the possible behavior of a system, has no equivalent in higher dimensions; and its generalization would for decades to come pose a major mathematical problem. Let us moreover emphasize that, mobilizing his geometric intuition, the qualitative theory would be progressively transformed by Poincaré himself into a whole new branch of mathematics particularly useful for the passage from local to global knowledge. In a series of memoirs on *Analysis situs*, he thereby established the basis of modern topology [Poincaré 1895].

From the start, Poincaré conceived the qualitative theory of differential equations with an eye set on celestial mechanics and in particular the stability problem for the solar system (conceived as the global stability of planetary trajectories). He tackled the questions in his famous essay "Sur le problème des trois corps et les équations de la dynamique" [Poincaré 1890] and his monumental treatise [Poincaré 1892–1899]. In these works, he developed methods analogous to those elaborated in the plane: to look at what happens in the neighborhood of periodic planar solutions, he used the transverse section method and the first-return map, or *Poincaré map*. In order to study the behavior of trajectories in the neighborhood of a periodic trajectory C going through a point M_0, Poincaré considered their intersection with a plane normal to C at point M_0. Let P_0 be a point of this plane (called *Poincaré cut*) very close to M_0; then the trajectory going through P_0 after having made a full orbit in the neighborhood of the curve C will intersect the normal plane at a point P_1 a priori distinct from, but close to, P_0. The next orbit similarly defines a point P_2 as it intersects the plane, etc. The (first-return) Poincaré map is the map from P_i to $P_{\mathrm{i}+1}$. It makes it possible to reduce the study of a set of trajectories to that of a point sequence in the normal plane. The first discrete recurrence to appear in dynamical systems (where time, no longer continuously varying, is symbolized by integers), the Poincaré map would turn out to be a crucial tool to reduce the problem to lower-dimensional space.

The theme of stability thus acquired a new significance. For Lagrange stability indeed meant the strict periodicity of trajectories. Poisson had enlarged it to the case where trajectories circled back, not precisely to their starting points, but an infinite number of times in their vicinitses. Considering the deviation of a trajectory from those initially close of it, Poincaré defined stability in terms of *sets of trajectories*. "Stable" solutions were therefore distinguished from "unstable" ones, according to their *characteristic exponents*. These results could also be extracted from the study of the Poincaré map, on whose transverse sections phenomena of contraction and dilatation could be observed. This global notion of stability was the one that would be taken and developed further by Lyapunov [1892] and the Gorki school (see Section 1.4 below).

In the study of stability Poincaré introduced the notions of *asymptotic* and *doubly asymptotic solutions* (in the past and in the future) wrapping themselves around periodic solutions. Distinguishing two types of trajectories (since G. D. Birkhoff, called elliptic and hyperbolic), he explored the local situation around an elliptic trajectory. His analysis implied that families of periodic trajectories existed with larger and larger periods. Each of these gave rise to islets and nodes and revealed a highly intricate structure of regular and very perturbed areas, which repeated themselves into smaller scales. In [Poincaré 1892–1899, Vol. 3] he exhibited a type of trajectories, called *homoclinic*, too complex for him to draw. The rich but dense body of results would occupy generations of mathematicians. Ten years after, he again took up the search for periodic solutions but under broader and more difficult conditions. Poincaré [1912a] reduced the problem to a famous "theorem of geometry:" a continuous map of an annulus onto itself, which rotates the circles at the border in opposite directions and leaves areas invariant, has exactly two fixed points. Stated shortly before his death, Poincaré's "last theorem" would soon be proved by his scientific heir Birkhoff [1913]. (See also [Birkhoff 1927, 163–170]).

Poincaré also considered another type of global stability in the context of his study of rotating fluid masses and planetary figures of equilibrium [Poincaré 1885, 1902]. Among those shapes, Jacobi's ellipsoid of revolution with three different axes and annular shapes were already known. Raising the question of their stability, Poincaré established that there existed series of figures depending on a parameter; when a figure belonged to two such series, it was called a figure of bifurcation. In a family of dynamical systems depending on a parameter, the bifurcation corresponded to a branching point on the manifold formed by periodic solutions with an added dimension corresponding to the variation of the parameter. To each figure, an infinite sequence of stability coefficients was associated, and the condition for equilibrium was that they were all strictly positive. When one was zero, the corresponding figure was a bifurcation. In this case, Poincaré explored the relationship between stability and bifurcation, most notably in the case where series of equilibrium figures exchanged their stability by passing through a bifurcation (from a spheroid to an ellipsoid, from an ellipsoid to a pear-shaped figure, etc.).

Let us finally mention the probabilistic concepts repeatedly mobilized by Poincaré, which from our point of new ought to be included among his basic innovations in the theory of dynamical systems, since they have become essential for the understanding of chaotic phenomena. In the course of his study of the stability of trajectories, he showed that there were an infinite number of particular solutions, which were unstable in the sense of Poisson, but *exceptional* in the sense of probability theory (i.e., of measure zero). This was Poincaré's famous "recurrence theorem," stating that an isolated mechanical system would necessarily come back arbitrarily close to its initial state, *except* for a set of cases with probability zero of occurring. Probabilistic concepts also came up in the incompressible fluid model used to illustrate the notion of integral invariant (for which he invoked the density of molecular trajectories in phase space). Probability theory was also central to his discussing the kinetic theory of gases and the Maxwell–Boltzmann postulate at the foundation of ergodic theory: "whatever the initial situation of the system, it will always pass an infinite number of times, I do not say through all the situations compatible with the existence of the integrals but as close to any of these situations as one would wish" [Poincaré 1894, 252]. Finally, probabilistic concepts with respect to liquid mixing were still present in [Poincaré 1912b]. His reflections underscored the necessity of deepening the understanding of probabilistic concepts as well as the difference between conservative Hamiltonian systems from the many-particle systems of statistical mechanics. Nevertheless, ergodic theory would have to wait until the 1930s for crucial advances [Birkhoff 1931, von Neumann 1932].

Foreshadowing a great deal of their concerns and introducing some of their key concepts and methods, the above four themes in Poincaré's *œuvre* truly were the starting point of dynamical systems theory and neighboring fields up to the latest craze for chaos. Why is it then that the assessment of his contribution has posed such a historiographical conundrum? For some, the matter is captured in a simple statement: "Chaos was discovered by Poincaré" [Diacu & Holmes 1996, 78]. Indeed, in 1908, the French mathematician and physicist had famously claimed: “it may happened that small differences in the initial conditions produce very great ones in the final phenomena.... Prediction becomes impossible.” [Poincaré 1908, 68–69]. This is one of the passages of Poincaré’s most quoted by contemporary “chaologists” (see, e.g., Crutchfield *et al.* [1986, 48]). However, this claim leaves intact the problem of having to explain why the great burst of activity could only take place several decades after Poincaré’s death. If the essential features of chaos, chiefly sensitive dependence on initial conditions, had been known for so long, how are we to account for the “chaos revolution?” Thus has a theme focusing on the “nontreatment” of chaos emerged in the literature. Present in scientists’ account, this theme was best articulated by Kellert [1993, 119–128]. In order to explain it, Kellert listed several reasons but none very convincing: the physicists’ interest in other theories than classical mechanics (relativity and quantum theories); social emphasis on stability in the scientists’ training and research practices; and predilection for theories enhancing control over understanding, for reasons having to do with technological, philosophical, or even specifically masculine *a priori* dogmas about the nature of science. As we shall see, however, various results of Poincaré’s would be picked up by generations of successors, studied, developed, and extended in a great variety of directions. None, it can be safely said, was ever truly *lost* or *forgotten*.

What is true, on the other hand, is that Poincaré’s results, which we have summarized above, were not, until much later, mobilized in an integrated manner. Comprising dozens of books and hundreds of articles, his lifework never was once and for all, so to speak, digested by successors. In fact, whether they even appeared as such to Poincaré himself is not clear. The dynamical systems synthesis is a *post facto* construction that has to be accounted for on its own terms. As a matter of fact, Poincaré’s published papers and books have constantly been revisited until the present, and only through this process was their meaning fully fleshed out and dynamical system theory developed. And it is this very persistent back-and-forth motion between the master and later contributors that makes the *longue-durée* history an unavoidable, but especially slippery, task for the historian of chaos.

1.2. Key Moments in the History of Dynamical Systems

The above discussion has suggested why, even if Poincaré originated the domain, it remains nonetheless impossible to write a single history starting with him and unfolding linearly. In fact, it would seem that *several* contemporaneous histories loop back to his seminal work, interact, and unfold again along different directions. Under the marked influence of some major developers of the field who have repeatedly drawn attention, both in research papers and later commentaries, to historical sources, studies of various moments of Poincaré’s conceptual heritage have been produced. (For a history of Poincaré’ heritage, see Dahan Dalmedico [1996c]).

The field’s main contributors have almost always striven toward the construction of scientific “traditions” whose last heirs they would have been. In the following, we shall several times seize the opportunity of underscoring this process apropos Smale, the Russians, Mira, Mandelbrot, and others. In a simpler fashion, most specialists have repeatedly revisited earlier works in search of tools and methods. In [Dahan Dalmedico *et al.* 1992] several leading representatives of the domain (J.-C.Yoccoz, Jacques Laskar, Pierre Bergé, and Marie Farge) frequently cited historical sources and, generally, it seemed to them important to illuminate accounts of recent scientific developments with lengthy discussions of the works of C. Bernard, L. Boltzmann, V. Boussinesq, A. Cournot, J. Hadamard, and P.-S. Laplace, as well as crucial mentions of Lagrange, Maxwell, Rayleigh, etc. The resulting history of dynamical systems theory therefore often presents itself as a series of revivals of older results, as scores of half-forgotten authors from the 20th century were rediscovered by wide spectra of scientists. Let us here recall the principal moments, which have attracted special attention:

—at the turn of the 20th century, Aleksandr Lyapunov's work on stability theory, emphasizing quantitative evaluation of the divergence rate between solutions with different initial conditions;

—in the first half of the 20th century, the fundamental use of modern topological techniques by George D. Birkhoff concerning conservative dynamical systems (see Section 1.7);

—in the 1920s and 1930s, the study by Balthasar van der Pol and other radio engineers on so-called relaxation oscillations, or sustained periodic solutions to nonlinear dissipative equations with a few degrees of freedom (see Section 1.5);

—in the 1930s and 1940s, the theoretical developments in the study of such oscillations made by Aleksandr Andronov's school, with in particular their results concerning "coarse (or rough) systems" in two dimensions (see Section 1.4);

—in the 1940s, the careful analytical and topological study on differential equations by Mary Cartwright and John E. Littlewood and Norman Levinson (see Section 1.5), which are direct offshoots of the wartime scientific mobilization;

—from the postwar period to the 1960s, the Nonlinear Oscillation Project led by Solomon Lefschetz, who translated work available in Russian for the English-speaking word and, in the Cold War context, organized research along similar lines in the United States (see Section 1.6);

—in the same period, the work of Andrei N. Kolmogorov and his students on the stability of Hamiltonian systems, and celestial mechanics in particular, leading to the celebrated KAM theorem (see Section 2.1).

Of course, the above list has no pretense of being exhaustive or definitive. One could just as well insert Eberhard Hopf's and Kurt Otto Friedrichs's work concerning bifurcation theory: contemporary mathematicians having "rediscovered" it tried to recover its "prehistory" (see Marsden & McCracken [1976] including an English translation of Hopf's original paper [1942] and various mathematical commentaries). One could mention Marston Morse's symbolic dynamics whose seminal work before World War II was Morse & Hedlund [1938]. One could recall Arnaud Denjoy's paper [1932] on abstract topology where he took up a question unresolved by Poincaré concerning the curves defined by a differential equation of the surface of a torus and using maps from the circle to the circle as its basic tool. The success of fractals, finally, has directed attention to Gaston Julia's and Pierre Fatou's work on discrete recurrences [Chabert 1990].

1.3. Smale: At the Confluence of Three "Traditions"

It is not uncommon in the history of science to base the reconstruction of the past on accounts given by some of its most prominent actors—in other words, to inherit from the winner's history. In the 1960s, the American topologist Steve Smale clearly dominated dynamical systems theory by his stature. (Smale's specific contribution to the theory of dynamical systems, which was crucial, will be discussed in more detail in Section 2.1 below. For a biography of Smale, see [Batterson 2000]. Several autobiographical accounts and testimonies from friends and students are gathered in [Hirsch *et al.* 1993]). More than anyone else Smale has shaped the contemporary conception of the domain, as well as the perception of its history. In a seminal article published in 1967 he sketched the methods, tools, goals, and, above all, the broad vision that have thereafter underlain this field of mathematics. In this paper and a whole series of studies devoted to dynamical systems mainly published between 1960 and 1980, as well as in a few autobiographical pieces, Smale laid out a network of historical sources that, in 1998, he organized as follows:

> I was lucky to find myself in Rio at the confluence of *three* different historical traditions in the subject of dynamics (called ordinary differential equations at the time) [Smale 1998, 44].

The three "traditions" he referred to were: (1) the Gorki school starting with Andronov and Pontryagin in the 1930s and picked up by Lefschetz's group at Princeton after World War II; (2) the tradition coming out of studies of the van der Pol equation via Cartwright and Levinson (ca. 1926–1949); and (3) the somewhat forgotten tradition of Poincaré and Birkhoff from the turn of the century to the 1920s and 1930s. According to Smale, the first tradition had cultivated part of the mathematical arsenal first put in place by Poincaré and adapted it to dissipative systems, but it seemed to have lost the memory of the wealth and complexity of dynamical behaviors one could expect. The second tradition had hit upon such complex behavior, but lacking much of Poincaré's sophisticated qualitative theory, was at a loss in perceiving its deep significance in terms of topological classification of dynamical systems and, down the road, of mathematical modeling. Picking up the third tradition Smale could perceive the way to use the full arsenal of tools bequeathed by Poincaré and marry the complexity of dynamical behaviors to the simplicity of the topological approach. This view of the prehistory of Smale's dynamical systems theory has been very successful due in part to its convincing tracing back of the emergence and development of several key mathematical concepts.

From a very complex genealogy Smale has therefore extracted three main threads, for he saw himself as standing at their crossroads. His scientific stature together with the importance of his own contributions conferred retrospective legitimacy to the three filiations thus constructed. By and large, we will follow them. However, it is important to underscore that, on a long term, they have no obvious, absolute consistency. Adopting the viewpoint of other protagonists partaking in the 1970s reconfiguration, different histories might be possible, which would emphasize other threads and other nodes. And, for that matter, different histories of this kind *have* been suggested. Mandelbrot [1975], for one, offered a retrospective prehistory of fractals, starting from Hausdorff, Fatou, and Julia, via lesser-known figures (at the time) such as meteorologist Lewis Fry Richardson, probability theorist Paul Lévy, and the precursor of "econophysics" Louis Bachelier, up to himself [Chabert 1990]. Let us note that a complete "prehistory" of fractals might require to add to Mandelbrot's first sketch the specific contribution of Soviet mathematicians, esp. Pulkin [1950], as was suggested by Christian Mira (see below). To follow ergodic theory and the link between instability and statistical physics Russian physicist Yakov Sinai [1992 & 1993] has done, while agreeing on the importance of Poincaré, Birkhoff, Hopf, and Kolmogorov, leads one to insist on different aspect of their *œuvres*, as well as on the specific contribution of other physicists such as Ludwig Boltzmann, Willard Gibbs, Max Born, or the Russian statistical physicist Nikolai Sergeievich Krylov.[8] Quite visible and prevalent in some physicists' early works on chaos (prior to 1975), this viewpoint has been all but suppressed by the popular success of dynamical systems. By focusing on discrete recurrences, mathematicians closer to engineering concerns, as applied mathematician Christian Mira insisted upon, have thrown light on other contributions and traditions (see Mira's "Exposé introductif" in [Mira & Lagasse 1976]; see also Mira [1986; 1997] and the historical comments he sprinkled in his books [Gumowski & Mira 1980; 1982]). This last line of development will be discussed in Sections 3.3 and 4.1 below.

To summarize, the now classic historiography of the period prior to the 1960s is structured by three main elements: (1) an uncontested point of origin: Poincaré's multifaceted lifework; (2) the towering figure of the mathematician Smale who in 1959–1970 produced an astute synthesis reconfiguring dynamical systems theory; and (3) the three traditions reconstructed by him that give a coherent prehistory of his own synthesis. In the following, we have exploited Smale's synthesis as, so to speak, an entry point into the intricate history of dynamical systems theory and offer our personal contemporary rereading of these traditions. We will see that, by paying some attention to the sociohistorical context, the landscape becomes incomparably richer, and also more problematic.

1.4. Andronov's Gorki School

Let us first develop the case of Andronov's school in some details, since it probably is the one with which the reader will be least familiar (see Diner [1992], Dahan [1994;1996b] for some aspects of Andronov's school, and also Dahan & Gouzévitch [forthcoming]). Andronov's roots and his self-acknowledged sources are twofold: (1) his mentor, physicist L. I. Mandelstham, whose work concerning optics, radiophysics, and the theory of oscillations contained an actual program of unification for the study of nature on the basis of the "physics of oscillations;" and (2) Poincaré, whose work he referred to as early as 1928 and never ceased to study, recommend to students, and publish in the Soviet Union. After having acquired a strong training in mathematics and physics, in his thesis Andronov attacked an engineering problem suggested by Mandelstham: to take self-induction into account in the case of the electromagnetic switch. Analogous to van der Pol's relaxation–oscillation (see Section 1.5), this oscillator is a dissipative system whose vibrations are sustained by a nonoscillating external source of energy. In phase space, Andronov [1929] noticed, this motion is analogous to Poincaré's limit cycles. Using results from *Méthodes nouvelles de la mécanique céleste*, he developed a "storing method" to study the stability of periodic solutions. It was far from self-evident that one could transfer Poincaré's methods and results from Hamiltonian mechanics to dissipative systems involving a few degrees of freedom; but it was a crucial move. Henceforth, by using, transposing, or extending Poincaré's arsenal Andronov would endeavor to develop Mandelstham's program. Also reaping Lyapunov's heritage, Andronov focused on the problem of stability. Combining Poincaré's small-parameter method with Lyapunov's stability theory, he established a

[8] A student of V. A. Fock's, N. S. Krylov, defended in July 1941 a thesis on "Mixing processes in phase space." Published posthumously in 1950, his ambitious monograph on the foundations of statistical mechanics came as a surprise in the West when it was translated in 1977. On its crucial influence in the Soviet Union, see Sinai's postface in Krylov [1977]. On N. S. Krylov, see Diner [1992].

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method for finding periodic solutions and studying their stability. (No doubt because of its quantitative aspects, Lyapunov's theory has tended to be neglected in historical given by dynamical systems theorists influenced by Smale).

In 1931 Andronov settled far from Moscow, in Gorki, where there already was a small radiophysics institute, the lead of which he took. This institute, which he decided to develop, allowed him to found his own research school devoted to the study of nonlinear oscillations. From a scientific and strategic point of view, the context was favorable to the study of self-sustained oscillations. In this period, questions concerning oscillations in electric, thermionic, and electronic circuits, and later electronic scanning and television attracted much more interest than the study of mechanical vibrations ever did. A reason for this was that, undesirable in mechanical devices (such as trains and airplanes), vibrations were parasitic effects one sought to eliminate or at least control rather than study and indeed calibrate precisely (e.g., in radio receivers). (On the history of electrical and radio engineering, see Bennett [1993] and Dunsheath [1962]). In the 1930s, the stabilization of nonlinear vibrations and resonance phenomena seemed vital for Soviet military power. At the First All-Union Conference on Auto-oscillations, held in Moscow in November 1931, the scientific and technological importance of the problem was underscored, work was organized, and Andronov and his Gorki institute were put in charge of research efforts. Gathering a team of pure and applied mathematicians, physicists, and engineers, he intended to tackle theoretical and practical problems simultaneously and in close cooperation.

In the course of 1930s, nonlinear oscillation theory—that is, the study of differential equations involving a small number of degrees of freedom principally stemming from radio technology—formed Andronov's privileged field of inquiry in close contact with applications: electrical circuitry with vacuum tubes, neon tubes, relaxation oscillations in radiophysics and electrical engineering, oscillation in vehicle wheels, machine regulation and control, etc. The several highly theoretical tools then used and developed by Andronov and his collaborators (point maps, recurrences, bifurcations, critical cases, stability, and the famous notion of "*systèmes grossiers*" discussed below) were all subservient to applications. In particular the point-map method—explored by Poincaré in the case of the first-return map—was, for a time, essential. Well adapted to engineering problems (especially automatic regulation) where discrete equations were useful, this method made it natural to think about the *states* of the system considered as *points* in phase space. It also made it easier to extend the theoretical framework common to both Hamiltonian and dissipative systems.

Most of these results, together with their context of application, were gathered in Andronov, Vitt, and Khaikin's *Theory of Oscillations*, first published in 1937.[9] The structure of the treatise reflected the research program from which it emerged: the basic topic was nonlinear systems with one degree of freedom and related oscillations and attention was especially focused on concrete examples treated as fully as possible (Froude's pendulum, clock theory, circuit with vacuum tubes, ship stabilizer, Prony's break, and so forth). Contrary to celestial mechanics (where Newton's law was supposed to be exactly true), in considering "real physical systems [which] we are always forced to simplify and idealize"

[9] Victim of Stalinist purges in 1937, Aleksandr Adol'fovich Vitt had his name removed from the original Russian edition. The book was first translated into English by Lefschetz in 1949 in an abridged form, and then again in 1966 in its entirety.

[Andronov *et al.* 1966, xv]. In a lengthy introduction they explained that:

> It is evident that since small random perturbations are inevitable in all physical systems, processes which are possible only in the absence of any random deviations or perturbations whatsoever cannot actually occur in them [Andronov *et al.* 1966, xviii].

These considerations had led Andronov and his collaborators (his wife E. Leontovich, A. G. Maier, N. N. Bautin, and others) to develop the theory of bifurcations, that is, the study of qualitative changes in phase portraits as parameters were slightly varied, and, from there, to the double stability of systems—that is, with respect to variations in initial conditions (in reference to Lyapunov's theory), and with respect to variations in a parameter, or, as he wrote, to "the mathematical model itself." The implication of this second type of stability was clear: "we have always to allow for the possibility of small variations of the form of the differential equations which describe a physical system" [Andronov *et al.* 1966, xxvii].

From such concerns emerged the notion of "coarse systems." First introduced in the literature as "*systèmes grossiers*" by Andronov and Pontryagin [1937], this term has been diversely translated in English, as "coarse" or "rough systems." In his 1949 translation [Andronov *et al.* 1949], Lefschetz called these systems "structurally stable systems." As Arnol'd [1994, 224] has emphasized, this notion appeared in Andronov's work as both a mathematically rigorous definition and a general idea about the type of systems useful for mathematical modeling in physics and engineering. In mathematical terms, it stated that a system was coarse if a small variation in the equation induced a "small" homeomorphism under which the phase portrait was qualitatively unchanged, mapping trajectories into trajectories, critical points into critical points, limit cycles into limit cycles, and so on, that is, in two-dimensions, the phase portrait of the modified system,

$$\frac{dx}{dt} = P(x, y) + p(x, y); \quad \frac{dy}{dt} = Q(x, y) + q(x, y),$$

was qualitatively equivalent to the unperturbed one (with $p = q = 0$).

The most important physical question concerned stationary states, either states of equilibrium or periodic motions, "the most typical [motions] over long intervals of time" [Andronov *et al.* 1949, xxvii]. This is why the search for limit cycles, in general very difficult, was crucial, and the treatise summed up the methods available for this task (Poincaré's index method, the criterion of Bendixson [1901], and Andronov's own methods). Under the constraints of double stability mentioned above, one was interested in classifying expected typical behaviors. Sophisticated mathematical methods were elaborated to answer the question: What is it necessary to know about a given system in order to be able completely to determine the qualitative structure of its orbits? In contemporary topological language, this amounts to identify a complete invariant of the system under topological conjugation. As early as 1937, it would seem, Andronov's school had developed the theory of two-dimensional coarse systems, including a characterization of possible bifurcations and the identification of a topological invariant (the scheme), the offshoot of which was that, in two-dimensional coarse systems, only stable limit cycles could represent self-oscillating phenomena.

At the end of the 1930s Andronov and his school turned their attention to automatic control theory (flying dynamics, clocks, geared regulators, etc.). A characteristic trait of such systems was discontinuous nonlinearity, due to dry friction, relay switches, and so forth.

Andronov considered multidimensional problems, which aimed the arsenal developed for the two-dimensional case at nonstandard situations. Andronov, Bautin, Maier, and others dealt with stabilization problems (e.g., for automatic-pilot airplanes, the so-called Mises–Vishegradsky problem) and other nonlinear oscillation problems. Questions concerning the theoretical physics of oscillations were also studied from a practical standpoint: diodes and magnetrons in particular, which led to the statistical study of high frequencies. Andronov's school therefore pioneered the study of fluctuations and the influence of parasitic noise on self-oscillating processes, on which relied modern electronics and, later, masers and lasers (Pontryagin, Andronov, and Vitt published their famous notice "On the Statistical Treatment of Dynamical Systems" in 1933; on masers and lasers, see Bromberg [1991]).

During the "Great Patriotic War," and even more in the Cold War, the Gorki institute's funding increased greatly. A Radiophysics School was set up which remained in close contact with Andronov's institute. Contracts with the military and "P.O. boxes" were plenty. (For top-secret contracts from the Defense Ministry in the old Soviet Union, a P.O. box number often was the only identity given to correspondents.) Several other domains, from various fields of physics (high-frequency physics, electronics, astrophysics) as well as engineering science, came into their realm: waves and radar, interactions between magnetic fields (crucial for various instrumentation problems), missile-launching problems, etc. After Andronov's death in 1951, his collaborators Zheliztsov and Neimark were connected with top-secret work on nuclear reactors and the control of explosion processes because of their expertise in regulation.

The global picture of the work conducted in the Soviet Union still remains rather obscure. The article giving the most complete survey of the diversity of Soviet research in the domain is [Diner 1992]. Let us mention that another school developed in Kiev, Ukraine, in the 1930s. Most famously represented by Nikolai M. Krylov and Nikolai M. Bogoliubov,[10] the Kiev school focused on "nonlinear mechanics" using—rather than qualitative approaches—analytic, quantitative methods, such as asymptotic methods, development in series, and averaging and approximation procedures mainly coming from Poincaré [1892–1899, Vol. 2]. In Kiev, just as in Gorki, practical and technological concerns were tightly mingled with fundamental theoretical or mathematical developments.

The second school that played a fundamental role in the Soviet Union was, of course, that of Andrei N. Kolmogorov, whose work on classical mechanics was influenced by Krylov and Bogoliubov. Comparable only to Poincaré's in scope and depth, Kolmogorov's *œuvre* crossed dynamical systems theory from different angles: probability theory, stochastic processes, information theory, turbulence, spectral theory, and above all the general theory of Hamiltonian systems in classical mechanics and ergodic theory. The editor of his *Selected Works*, V. M. Tikhomirov, had divided Kolmogorov's lifework into three realms: order (mathematics and mechanics), "chaos" (probability theory and statistics), and information and algorithmic theories, where these two realms had no natural border. As Tikhomirov explained, "the conception of randomness as algorithmic complexity, the attempt at discovering the essence of the notion of order and chaos filled the creative life of Andrei Nikolaievich, and so to speak tie all his creative efforts in a single knot" (quoted in Diner [1992, 353–354]). It is important to emphasize that, contrary to Poincaré, Kolmogorov

[10] N. M. Krylov is not to be confused with N. S. Krylov, mentioned above. For accessible surveys from the period, see Krylov & Bogoliubov [1933; 1943]. An influential exposé was Bogoliubov & Mitropolski [1961].

trained scores of students who would become world leaders in the field of dynamical systems theory once communication with the West was made easier in the early 1970s, among whom Manin, Arnol'd, Sinai, and Novikov. (On Kolmogorov see Diner [1992], Arnol'd [1993], and Shiryaev [1989], as well as the volume edited by the American and the London Mathematical Societies [2000]). Clearly, a detailed study of mathematics in the Soviet Union and its impact on the "chaos revolution" is still wanting.

1.5. Van der Pol Equation: From Radio Engineering to Topological Monsters

The second "tradition" to have influenced Smale focused more tightly on the study of differential equations. As he recalled it, it was through a letter from MIT mathematician Norman Levinson, received while he was in Brazil, that Smale first got into contact with it. Having co-authored the main graduate textbook [Coddington & Levinson 1955] on ordinary differential equations, Levinson—Smale [1998, 41] wrote—"was a scientist to be taken seriously." As he saw it, however, this tradition had its root not in the abstract theory of differential equations, but in the work of Balthasar van der Pol, a Dutch electrical engineer working for the Philips Research Laboratories in Eindhoven [van der Pol 1926, van der Pol & van der Mark 1928, van der Pol 1948]. Stemming from radio engineering, the problems he raised were very close to those tackled just a few months later by Andronov, who always acknowledged the former's decisive influence. By simplifying the equation for the amplitude of an oscillating current driven by a triode, van der Pol had exhibited an example of a dissipative equation without forcing which exhibited sustained spontaneous oscillations:

$$v'' - \varepsilon(1 - v^2)v' + v = 0.$$

In 1926, when he started to investigate its behavior for larger values of ε (where in fact the original technical problem required it to be much smaller than 1), van der Pol disclosed the theory of relaxation oscillation. He developed a theory of synchronization (of the proper frequency with external force) and studied the phenomenon of frequency demultiplication. Applying the mathematical arsenal familiar to the practitioners of electromagnetic theory, these engineers studied differential equations by craftily transforming them (e.g., by variable changes), and then using graphical methods (isoclines) and phase-space representations to discuss general trajectories.

Hitting upon the oscillatory behavior seemingly so common in the world (from "a pneumatic hammer" to "the menstruation"), van der Pol and his collaborator van der Mark built an electrical model of the heartbeat [van der Pol & van der Mark 1928]. From then on, whether for the sake of mathematics or applications, a great number of investigations focused on the van der Pol equation and various generalizations, the most important for our purpose being the forced equation introduced by Liénard [1928],

$$\ddot{y} + f(y)\dot{y} + y = p(t),$$

involving a forcing term $p(t)$.

In the 1930s it seemed that self-sustained oscillations, and relaxation oscillations in particular, became extremely lively research topics. According to van der Pol's own count, this had led by 1947 to "at least one hundred papers and books, particularly from the Russian and French quarters" [van der Pol 1948, 1150]. According to the engineer Philippe Le Corbeiller, author of a monograph on self-sustained systems [1931], a "method born by chance amongst engineers must constitute a . . . *Theory of Oscillations* whose theorems would in principle be independent from their mechanical, thermodynamic, electrical, chemical, physiological, biological, or economic applications" [Le Corbeiller 1933, 328]. In Germany the *Heinrich-Herz Institut für Schwingenforschung* was founded, headed by K. W. Wagner. This was "rather a curious branch of mathematics developed by different people from different standpoints, straight mechanics, radio oscillations, pure mathematics and servo-mechanisms of automatic control theory" [Cartwright 1952, 88]. In a rare consensus, postwar pure and applied mathematicians concurred that van der Pol's equation was "one of the few interesting problems which contemporary physics has suggested to mathematics" [Weil 1946, 332]. (See also von Kármán [1940]).[11]

During World War II, the conditions for a renewal of interest in this type of problem on the part of mathematicians were ripe again. As Freeman Dyson [1997, 66] has written:

> The whole development of radio in World War Two depended on highpower amplifiers, and it was a matter of life and death to have amplifiers that did what they were supposed to do. The soldiers were plagued with amplifiers that misbehaved and blamed the manufacturers for their erratic behavior.

As early as January 1938 a memorandum was issued by the Radio Research Board of the British Department of Scientific and Industrial Research urging mathematicians to assist in "solving the type of equations occurring in radio works, laying emphasis on the need to know how the frequencies of the periodic solutions varied with the parameters of the equation" [Cartwright 1952, 86]. Of interest to our purpose, serious work was done by Cartwright and Littlewood, and later Levinson on these "very objectionable-looking differential equations occurring in connection with radar" (McMurran & Tattersall [1996], 836; see also [Cartwright 1974] and [McMurran & Tattersall 1999]). In occupied France Rocard wrote the first version of his famous textbook [Rocard 1941] on nonlinear oscillations in July 1940. Reissued up to the 1960s, Rocard's textbook served as an introduction to the topic for many French "chaologists."

With respect to the van der Pol and Liénard equations, both mathematicians and engineers focused on finding stable periodic solutions. The emphasis was put on the frequency, rather than amplitude, of oscillation, leading to a focus on the (x, t) plane, where time is explicit, rather the phase plane (x, v), where v is the derivative of x with respect to time. From an electrical engineer's viewpoint, the stability of solutions was crucial. As Cartwright [1952, 84] has recalled:

> The radio engineers want their systems to oscillate, and to oscillate in a very orderly way, and therefore want to know not only whether the system has a periodic solution, but whether it is stable, what its period and amplitude content are, and how these vary with parameters of the equations.

Contrary to Andronov and his followers, however, this line of research approached problems, as Cartwright herself acknowledged, "with little knowledge of the classical work of

[11] van der Pol's work and the subsequent surge of research on relaxation-oscillations seriously deserve further historical investigation; for a discussion of it see [Israel 1996, 34–51].

Poincaré, Liapounov, and Birkhoff" [Cartwright 1952, 86]. A skilled analyst and an experienced mathematician nonetheless, Cartwright used keen topological methods introduced by Levinson [1944] to study the following Liénard equation:

$$\ddot{y} - k(1 - y^2)\dot{y} + y = b\lambda k \cos(\lambda t + a).$$

In the paper written with Littlewood in 1945, they showed that for $b > 2/3$, and k large enough, a periodic solution existed, toward which all other solutions tended. But for $b < 2/3$, they were faced with a "very bizarre" set K_0 of nonperiodic trajectories, which was connected, of measure zero, separating the plane into two regions, bounded and unbounded, but very complicated [Cartwright & Littlewood 1945, 182–184]. They were puzzled by this "bad curve" (a fractal set according to Abraham [1985]), and in a footnote they acknowledged: "our faith in our result was one time sustained only by the experimental evidence" provided by [van der Pol & van der Mark 1927]. They found comfort in a paper by Birkhoff [1932] on remarkable curves, which however was not self-evidently connected to dynamics. In technological terms, as Dyson [1997, 66] wrote, Cartwright and Littlewood had

> discovered that as you raise the gain of the amplifier, the solution of the equation become more and more irregular. At low power the solutions has the same period as the input, but as the power increases you see solutions with double the period, and finally you have solutions that are not periodic at all.

Mathematically speaking, the set exhibited by Cartwright and Littlewood was distressing, but what was its significance? Using the technique of the Poincaré map, Levinson [1949] showed what the pair has merely hinted at, namely that Birkhoff's curves could arise in dynamical systems. He defined a transformation in the phase space associated with the differential equation as such: given (y, v), where $v = y'$ and $y(t)$ is the solution of the forced Liénard equation, Levinson defined a transformation T from the plane to the plane as $T(y, v) = (y_1, v_1)$, where $y_1 = y(t + 2\pi)$ and $v_1 = v(t + 2\pi)$. Both he and Cartwright and Littlewood noticed the robust character of these "bizarre" properties; these were not exceptional "monsters," but stable behaviors that could not be perturbed away. These sets were those Smale interpreted geometrically rather than analytically to come up with the famous "horseshoe."

1.6. Lefschetz's Synthesis: Cold War Mathematics?

If the first tradition invoked by Smale—through which he was actually introduced to this field of mathematics—had its roots in the Soviet Union of the 1930s, it had been translated and heavily mediated by Princeton mathematician Solomon Lefschetz. Like for Andronov's school, this work bore the heavy stamp of the war (both WWII and the Cold War). Having met a Russian émigré, Nicholas Minorski, who had during WWII written a voluminous report on Soviet nonlinear oscillation research for the Navy, Lefschetz was convinced that this was a vital field of applied mathematics that had been neglected in the United States and ought to be publicly supported. "Many hold the opinion that the classical contributions of Poincaré, Liapounoff and Birkhoff have exhausted the possibilities. This is certainly not the opinion of a large school of Soviet physico-mathematicians" [Lefschetz 1946, iii]. In the context of heavy military support for basic research created by World War II, he set up a "Project on Differential Equations" most specifically devoted to nonlinear equations.

> [Its objectives] were stated to be, on the one hand, research in the field and, on the other, the development of a group of young men who could take their place as applied mathematicians in Industry or in an emergency, in various defense organizations [Lefschetz 1959, 2].

Repeatedly, Lefschetz would express his worry about the "mathematical gap" he discerned between the Soviet Union and the United States. In press releases, he often revealingly described nonlinear differential equations as "the involved mathematical systems which underlie almost every natural movement, including those which must be understood in order to develop more accurate rocket control systems." After his retirement, he set up the Research Institute in Advanced Study at the Martin Company, an aviation and missile manufacturer.

Having supervised the "free translation" of [Krylov & Bogoliubov 1943], Lefschetz went on to adapt the monograph [Andronov *et al.* 1949] and reprint Lyapunov [1907]. At Princeton, he oriented several Ph.D. students towards nonlinear oscillations and control theory; he headed a lively seminar where, in particular, Cartwright lectured for several weeks in 1949; he oversaw the publication of a subseries of Princeton's *Annals of Mathematics Series*, which gathered the result of his group's activity; finally, he organized several international conferences. (On the history of Lefschetz's group, see [Lefschetz 1959] and [Dahan Dalmedico 1994]). Before we discuss the orientation of the group toward the global approach to dynamical systems theory that prefigured Smale's, let us mention that it also led to the influential mathematical work of Joseph LaSalle, Jack K. Hale, and others, much closer to control theory. (On control theory, see for example LaSalle [1987]).

Contrary to what Smale seemed to imply, Lefschetz benefited not only from the crucial import from Andronov's school, but also from the Kiev school and Cartwright, Littlewood, and Levinson's topological approach. This partial synthesis helped Lefschetz in drawing attention to structural stability in conjunction with topological equivalence. In his English translation of [Andronov *et al.*, 1949] he had indeed, as we have seen, replaced coarseness by "structural stability," therefore shifting the focus from the system to its property. (In Section 2.1 below, the scientific translation work performed by Lefschetz's group, concerning especially structural stability, will be discussed in more detail). From a methodological guide for the study of nonlinear systems, Andronov's ideas became a useful, intriguing, but rather technical mathematical concept. To consider it in such terms, however, impelled Lefschetz to move from an early emphasis on the analytic study of specific nonlinear oscillators, perhaps catered to perceived engineering needs, to a more global program of classification. In his *Dynamical Equations: Geometric Theory* Lefschetz [1957] submitted Poincaré to a careful study and produced a much more ambitious synthesis of the domain than his previous monograph [Lefschetz 1946] had been. In the late 1950s, Lefschetz geared the field toward global analysis, in the sense that systems would henceforth only be handled as representatives of classes to which they belonged.

Cold War rhetoric notwithstanding, and albeit remaining close to engineering concerns, the work done in the project mainly consisted in the analytic study of specific classes of nonlinear differential equations without paying much attention to applications. The project was indeed successful in building an intellectual and institutional basis for the subject of differential equations in the United States and "nearly all its [younger] members remained in the academic world," Lefschetz [1959, 22] noted in his final report. When Smale joined this group around 1959, it consisted of an active community of practitioners moving toward more and more abstracts methods and concerns. Attracted by the prospect of using his topology background for some specific problems of general classification, he greatly accelerated this evolution.

1.7. Looping Back to Poincaré: The Homoclinic Tangle

In view of the above, it is somewhat surprising that Smale insisted that he found himself at the confluence of still another, independent "tradition," the older, more or less forgotten one going back to Poincaré and Birkhoff. Did not these authors already figure prominently in the traditions he was inheriting from more directly? Smale, however, recalled having realized the utmost relevance of Poincaré's work for dynamical systems theory not from differential equations specialists, but by chance, "from browsing Birkhoff's collected works" while he was in Rio de Janeiro in 1959–1960. Indeed, working on the structural stability of gradient systems Smale [1990, 32] "noticed how dynamics led to a new way . . . to attack the Poincaré conjecture, . . . and before long all my work focused on that problem." Not without difficulty, he proved the conjecture in dimensions greater than 5, which in 1966 "probably warrant[ed] his presence" at the Moscow Congress where he was awarded the Fields Medal [Thom 1968a, 25]. (On the Poincaré conjecture, see Volkert [1996].) Considering Smale's interest in topology, on the one hand, and his pursuit of complicated structures in dynamical systems, on the other, it is hardly surprising that he was attracted to Poincaré's results in the latter field. He went on:

> Unfortunately, the scientific community had lost track of these important ideas surrounding *homoclinic* points of Poincaré. In the conferences in differential equations and dynamics that I attended at that time there was no awareness (if the conferees had known of this work of Poincaré and Birkhoff, the conjectures in my rather publicized talks would have been answered earlier). Even Levinson never showed in his book, papers, or correspondence with me that he was aware of homoclinic points. *It is astounding how important scientific ideas can get lost, even when they are exposed by leading mathematicians of the preceding decades* ([Smale 1998, 44], our emphasis).[12]

Albeit having been trained at Chicago, George D. Birkhoff was the only true disciple of Poincaré in the Western hemisphere. Shortly after the latter's untimely death in 1912, Birkhoff established his reputation by proving a conjecture known as "Poincaré's last geometric theorem" (see Section 1.1 above). Having read the *Méthodes nouvelles* in 1912, Birkhoff started to work on the field he would call dynamical systems. That year, he introduced the notions of "minimal" and "recurrent" motions. In Chapter 7 of his *Dynamical Systems* [Birkhoff 1927], he developed a "General Theory" going further than Poincaré and Hadamard in the topological study of curves defined by differential equations. In particular, Birkhoff generalized Poincaré's limit cycles, by introducing several concepts that prefigured different facets of the *attractor* concept: nonwandering, minimal, alpha- and omega-limit sets, central and recurrent motions. In his own words, "the set W of wandering points of M is made up of curves of motion filling open n-dimensional continua. The set M_1 of *non-wandering points* is made up of the complementary closed set of curves of motions" [Birkhoff 1927, 192]. Now, finding the nonwandering set M_2 with respect to M_1, and constructing the sequence M_1, M_2, etc., we must at some point end the process with a set C of *central motions. Recurrent motions* are those that come back arbitrary close to every point of the curve of motion. They are in the set of central motions but the reverse is

[12] For surveys of the results of Poincaré and his followers concerning homoclinic points, see [Anderson 1994].

not necessarily true. α- and ω-*limit points* are defined as the sets of limit points as time (t) tends to $-$ or $+\infty$. Nonwandering sets are in general larger than limit sets. (For these, and other, definitions, see Birkhoff [1927, 191–200]. An attractor has been succinctly defined as "an *indecomposable, closed, invariant* set . . . which attracts all orbits starting at points in some neighborhood" by Holmes [1990]).

The stability of dynamical systems was the central concern of Birkhoff in his 1927 book. He introduced a large array of notions of stability for dynamical systems and their periodic solutions, some of which were already present in the literature, but some of which were new: complete or trigonometric stability, stability of the first order, permanent stability ("for which small displacements from equilibrium remain small over time"), semipermanent stability, unilateral stability (due to Lyapunov), and stability in the sense of Poisson (due to Poincaré). Birkhoff also systematically introduced topological arguments into the study of dynamical systems, proving the ergodic theorem and introducing formal methods leading to symbolic dynamics extended by Morse, Hedlund, and later Ulam.

Smale's independent rediscovery of this "tradition" raises once more the central conundrum of the historiography of chaos. According to Marston Morse (in his introduction to the 1966 reprint of [Birkhoff 1927], v), "History has responded to these pages on Dynamical Systems in an unmistakable way," in that it shaped much of the work done by Kolmogorov, Arnol'd, and Möser on the celebrated KAM theorem (briefly discussed below in Section 2.1.). For Smale, however, it was a revelation, for most of the concepts listed above had been downplayed in the literature familiar to Lefschetz's collaborators. But Birkhoff had been a prominent member of the American Mathematical Society, teaching at Harvard from 1912 to the end of his life in 1944. So how can we account for the fact that some of Poincaré's ideas, taken up and extended by Birkhoff had been forgotten? In fact, Lefschetz believed that conservative systems had been studied too much: like the Gorki and Kiev schools, one definitely had to turn one's attention to dissipative systems, much more useful as far as concrete applications were concerned. In the immediate postwar period, the separation between conservative and dissipative systems had become just as tight as the dichotomy between two competing models for the mathematical sciences: Next to the theoretical questions concerning mainly celestial mechanics pursued, by and large, individually by the Harvard professor before WWII, a new type of mathematical research was imposing itself—goal-oriented investigations sponsored by the Office of Naval Research and responsive to engineering needs and national interests.

Moreover, as Smale's quotation above shows, his own interest in Poincaré focused on a very specific topic—homoclinic points—and this was the idea he thought had got "lost," and *not*, obviously, Poincaré and Birkhoff's whole lifework. Emphasizing the complexity of dynamical systems, in his 1962 address at the Stockholm Congress, he quoted (in French) the dramatic description of homoclinic points given by Poincaré [1892–1899: 3, 389]:

> When one tries to depict the figure formed by these two curves and their infinity of intersections, . . . these intersections form a kind of net, web, or infinitely tight mesh. . . . One is struck by the complexity of this figure that I am not even attempting to draw. Nothing can give a better idea of the complexity of the 3-body problem and of all the problems of dynamics in general.[13]

[13] We follow the translation of Barrow-Green [1997, 162]. Only the last two sentences were quoted by Smale [1963, 494].

Homoclinic points provided him an example of a theorem stating that there existed structurally stable systems with an infinite number of periodic points (and minimal set homeomorphic to a Cantor set). Moreover, Poincaré's very definition of homoclinic points requires the study of (in anachronistic language, but in a form easy to recognize) stable and unstable manifolds [Hirsch 1984, 36–38]. Adopted by Smale in the 1960s, this approach was precisely what allowed him to innovate in the field.

1.8. Epistemological Reconstruction and History

At the end of this sketch of a historical cartography of dynamical systems theory before 1960, one is bound to be struck by the great heterogeneity among particular situations. Whether defined by epistemological proximity or socioinstitutional situation, each of the groups here described emphasized different aspects of the domain. Objects of study were sometimes opposite (conservative systems for Poincaré and Birkhoff vs dissipative systems for engineers); various central questions were favored (stability for Lyapunov, recurrent and central motions for Birkhoff, resonance for engineers, etc.); approaches and methods also varied (analytic methods for Lyapunov, point transformation for Andronov, ergodic theory and limit-set methods for Birkhoff, and so on). In addition, the actors' social insertion was very contrasting. In the 1930s, for example, what could the socioprofessional worlds of the mathematician Birkhoff (professor at Harvard), the "Grand Old Man of Radio" van der Pol (at the Philips Research Lab), and the Soviet "physico [engineer] mathematician" Andronov at Gorki have had in common? What, in the 1950s, had Kolmogorov's school in common with Lefschetz's? It is precisely this manifold character of social and epistemic landscapes that poses problem in this history.

Not only are the elements selected for this "prehistory" of dynamical systems theory as it emerged in the 1960s very heterogeneous, but it is full of blatant holes and omissions. Shifting the viewpoint, focusing on the problem of the relationship between order and disorder—a crucial theme for the conceptual reconfigurations of the 1970s among statistical physicists—would for example lead one toward a completely different reconstruction of the historical roads to chaos. We have already mentioned the fact that some prominent actors (Mandelbrot, Sinai, Mira) have indeed themselves sketched historical filiations that differ markedly from the one developed here. In the Introduction, we hinted that enlarging the framework of our study to the "nonlinear sciences" in general (turbulence, population dynamics, meteorology, etc.) introduces even more complexity and points to other historical chains. Is a prehistory of chaos, bridging from Poincaré to his wide rediscovery in the 1970s, at all possible?

The entry point we have ourselves chosen—emphasizing the conceptual road to Smale's synthesis developed over the years by Smale himself and his followers—has provided us with a consistent historical survey. Their effort goes a long way in explicating the source of many key mathematical concepts. As such, it forms a convincing epistemological reconstruction of the history of dynamical systems theory—but it is no more than that! To speak of "traditions," in addition, is here a source of perplexing confusion, blocking more refined understandings of the "chaos revolution" and even the topological brand of dynamical systems theory. The fascinating back and forth motion between Poincaré and his successors is unfortunately camouflaged. None of these "traditions" was ever hermetically sealed from the others, and interactions with other domains are thus underestimated. Finally, albeit representing important lines of transmission for specific problems, ideas, and results, they

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certainly did not share research programs, institutional continuities, or disciplinary identities. What deserves to be explained in this history is as much the fact that communication among the actors sometimes was difficult as the fact that reappropriation and transfers *could nevertheless take place*. Beyond the neglects and losses, the ruptures and "rediscoveries" so often celebrated or deplored in the history of Poincaré's heritage, only the analysis of these groups' various scientific programs and the specific contexts in which they operated can account for the meandrous exploitation of this heritage.

Each of the groups whose work we have reviewed (especially Andronov's school and Lefschetz's project) reaped several heritages and was itself at the origin of further developments in various directions.[14] The phenomena to which we shall now shift our focus occurred on a whole different scale. In the course of the 1960s and 1970s, the domain of dynamical systems theory "exploded" in a new environment from all points of view. In the technical realm, the development of computers and numerical-simulation methods started to be widely disseminated. On a scientific level, the role of models and the importance of the nonlinear domain were strongly affirmed. The rise of interdisciplinary work and its associated ideology changed research conditions. Culturally, one noticed a vogue for chaos and disorder, and a general motion back to the macroscopic and the concrete. More than for the previous period, in our study of the emergence of chaos we will perforce have to enlarge our angle of vision.

THESIS 2: LOCAL RECONFIGURATION (SMALE, LORENZ, AND RUELLE)

In the 1960s, the convergence and reconfiguration of chaos was prepared by the taking into consideration of instabilities in dynamical systems. This process emerged more or less independently from outstanding individual works in various scientific fields: most notably, Smale, who established the bases of a new mathematical branch—hyperbolic theory—and Lorenz, who in 1963 exhibited a simple low-dimensional formal model with complex trajectories. Over the decade, however, both series of results remained strictly confined to the professional and disciplinary milieus that had produced them. Out of them, the conscieousness of a global phenomenon only emerged slowly, eased by some applied topologists' conviction of commanding the elements of a global qualitative classification of dynamics. That this classification should have a tremendous importance for the modeling of natural phenomena was brilliantly illustrated by Ruelle's work by the end of the decade.

If the process we want to describe is to be characterized essentially as convergence and reconfiguration (Thesis 3, below), the period from 1960 to 1970 witnessed no movement at all comparable in scale to the processes at play in the following decade among the domains that concern us. However, crucial conceptual innovations in several disciplinary sectors led to important reorganizations; methodological reassessments were suggested with the aim of integrating new techniques—numerical or purely mathematical—with more traditional modeling practices; and limited movements of sociodisciplinary convergence were set into motion as new concepts, practices, and methodologies were seized upon as ways to start braiding together various hitherto independent threads.

Au: Nizhni Novgorod as meant?

[14] In July 2001, for example, a large conference at Nizhni Novgorod emphasized the three great lines of research emerging from Andronov's work: the mathematical theory of dynamical systems, control theory, and radiophysics.

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2.1. Smale's Topologization of Dynamical Systems Theory

The mathematical theory of dynamical systems, in particular, was totally reinvented by Smale's introduction of topological tools and methods.[15] In fact, his biographer has retrospectively viewed dynamical systems theory prior to his involvement as "not even a mathematical subspecialty, . . . [but rather] a mathematical wilderness lacking established problems" [Batterson 2000, 72–73]. Taking up this field intermittently throughout the decade, Smale indeed "set the agenda, made the important conjectures, and proved the big theorems" [Batterson 2000, ix–x], even while being interrupted by frequent changes of residence, intense political activities, and spectacular forays in pure topology. Even at the time, one could claim that "in a completely unexplored domain, in a mathematical jungle of inextricable wealth, [Smale] is the first to have shown the way and placed a few milestones" [Thom 1968a, 28].

Among the first areas where the topologization of dynamical systems theory took place was the study of "coarse systems." Picking it up from Andronov and Pontryagin, Father Henry DeBaggis [1952] detailed their characterization of two-dimensional structurally stable systems, establishing that they were, in some sense, *simple* (i.e., involved no strange attractor, but only fixed point and limit cycles). Using a topological approach, Mauricio Peixoto [1959] then proved that "most" dynamical systems in two dimensions were structurally stable or, in technical terms, that the set of structurally stable systems was dense in the set of two-dimensional dynamical systems. To use a term he borrowed from Thom (who himself picked it up from Italian algebraic geometers), this property was *generic*. This was a crucial step, replacing a mathematical statement with Andronov's methodological argument in favor of the usefulness of structural stability.

Au: as meant? "H. F." in References

Having met Peixoto at Princeton, Smale conjectured that several-variable systems followed a similar pattern. "I was immediately enthusiastic," he wrote, "with the possibility that, using my topology background, I could extend [Peixoto's] work to n dimensions" [Smale 1980, 148]. Using Thom's notion of *transversality*, he indeed was able to extend the classification used by Peixoto and DeBaggis to higher dimensions, thereby defining an important class of dynamical systems, which he called "hyperbolic." Smale then ventured two daring conjectures, stating that he believed that structurally stable systems were (1) generic, i.e., they formed a dense set; and (2) simple, in the sense that their attractors could only be a finite collection of fixed points or limit cycles [Smale 1960]. Quite soon, however, he himself would show that for $n > 2$ there are structurally stable systems with very complicated dynamics (with an infinite set of periodic trajectories), a counterexample he christened the "horseshoe." Introduced in Smale [1965], the horseshoe is one of the most celebrated exemplars of chaos. (For discussions, see, e.g., [Diacu & Holmes 1996; Ekeland 1984, 70–73]). The exhibition of this complicated dynamics—just as in Lorenz's famous "butterfly" (cf. Section 2.2 below), which was contemporaneous but independent from Smale's horseshoe—clearly displayed the unavoidable phenomenological complexity of even very simple systems. With the horseshoe, Smale's hope that every system could be approximated by one with simple dynamics was squashed. Even acknowledging the

[15] Smale's collaborators and students have produced excellent summaries of his work in dynamical systems [Hirsch 1984; Palis 1993]. We have ourselves offered our own interpretation of Smale's work in relation to that of his predecessors [Dahan Dalmedico 1994, Aubin 1998a].

existence of complex systems, he showed, structural stability was not a generic property of dynamical systems [Smale 1966]. Although both of his conjectures had been shown (by himself) to be "overenthusiastic," Smale had in the process succeeded in extending to higher dimensions a program whose implicit goal was to determine by mathematical means the class of systems that was useful for modeling.

When he published the seminal article "Differentiable Dynamical Systems" [Smale 1967], which offered a foundation on original topological ground for a domain hitherto dominated by analytical approaches, Smale presented a picture more complex than he had first expected. "A masterpiece of the mathematical literature, . . . this was exuberant in new ideas, new problems, new or unfinished paths of research. . . . Yet it was also much more solid, coherent and overwhelming: a theory, the theory of hyperbolic systems" [Palis 1993, 173]. Smale had switched earlier emphasis on structural stability to the notion of hyperbolic systems. A diffeomorphism is called *hyperbolic*, or satisfies "axiom A," if its nonwandering set Ω is hyperbolic and its set of periodic orbits in dense in Ω. An invariant set Ω is hyperbolic if the tangent bundle restricted to Ω splits into two subbundles, in which the derivative of the map operates, respectively, as a uniform contraction and expansion [Palis 1993, 173–173]. Smale's paper offered a layout for several decades of work. The goal, which has since proved elusive, remained to classify hyperbolic systems on the basis of topological criteria.

In addition to writing the foundational paper of the domain, Smale turned the Mathematics Department at Berkeley into a world-class research center on dynamical systems attracting faculty members, postdocs, and students, trained no less than 14 graduate students in four years, 1967–1970 [Hirsch *et al.* 1993, 59], and maintained close relationships with two other centers investigating related topics in similar ways—the Mathematical Institute at Warwick University headed by E. Christopher Zeeman, and the Institut des Hautes Études Scientifiques (IHÉS) near Paris, under René Thom's lead. (On the history of IHÉS, see Aubin [1998a, 1998b, and Jackson [1999]). Characteristically, Smale would on the whole leave it to others to carry out the "script" he had written, as he was also fond of doing in politics. (For a detailed account of Smale's political activities, see Batterson [2000]). Jerry Rubin, co-organizer with Smale of the 1965 Vietnam Day in Berkeley, would in 1994 recollect:

> In six months, he laid out the whole direction of the antiwar movement. He was almost like the Lone Ranger. He came in on his horse and gave us the message, and then dropped the silver bullet and went off (quoted in Batterson [2000, 126]).

In 1961 Smale traveled to the Soviet Union where he met Kolmogorov and "in his word, an extraordinarily gifted group of mathematicians: Anosov, Novikov, and Sinai" [Palis 1993, 170]. On this occasion Smale stated a conjecture according to which geodesic flows on compact manifolds of negative curvature and algebraic automorphisms of the torus were coarse. The announcement made public by D. V. Anosov at the 1962 International Congress of Mathematicians in Stockholm that he had proved Smale's conjecture led to important advances in dynamical systems theory. Indeed, with so-called Anosov systems (a large class of hyperbolic structurally stable systems), the widespread opinion holding that conservative systems could not be coarse was contradicted.

Similarly, the theorem presented by Kolmogorov at the Amsterdam International Congress of Mathematicians in 1954, and proved in the following decade by Arnol'd and

Möser—the so-called KAM theorem—established that Birkhoff's conjecture was mistaken: ergodic conservative systems are not dense in the set of all Hamiltonian systems; i.e., ergodicity is not a generic property (see [Arnol'd & Avez 1967, Abraham & Marsden 1967, Moffat 1990, Diacu & Holmes 1996, Chap. 5]). This meant that the relationship between order and disorder, stability and instability in celestial mechanics had to be rethought. Exhibiting the *homoclinic tangle*, a mesh of intertwined curves (also emphasized by Smale) picturing the intricate nature of possible solutions and the infinity of allowed scenarios, Poincaré and his followers had been too hasty in assuming that stable orbits did not exist. When perturbations were sufficiently small, KAM theorem showed that a majority of orbits were stable and quasi-periodic (albeit nonperiodic, the latter never diverged too far away from the periodic orbits of the unperturbed system); others were unpredictable; still others were caught in islets of stability within an ocean of chaos. In the course of the decade, work inspired by Kolmogorov's theorem turned Hamiltonian dynamics, from the "hopelessly obsolete, outmoded and purely formal branch of analytical mechanics" it once was, into "a fashionable branch of mathematics" [Arnol'd 1993, 132]. Among the scientists whose numerical work was triggered by KAM, we may count some who would extract, from simple systems, delicately complicated behaviors that would play crucial roles in the emergence of chaos, in particular astronomer Michel Hénon and physicist Joseph Ford (see [Hénon & Heiles 1964, Walker & Ford 1969]).

Each of these three results (Smale's, Anosov's, and KAM) challenged prevailing intuitions about the relationship between stability and order in systems describing nature. Previously, disorder had generally been associated with statistical methods, which, using the law of large numbers, generated mean values obeying simple laws. Although Poincaré's and Birkhoff's results, among others, showed this opposition between order and disorder to be not so clearcut, until the late 1950s only statistical methods seemed adequate to deal with "disordered" phenomena and were thus privileged in physics, meteorology, hydrodynamics, mechanics, etc. A significant example is the numerical experiment performed by Fermi, Pasta, and Ulam at Los Alamos in the 1940s and 1950s. While ergodicity (which was expected) would have justified a statistical treatment, the semiorder uncovered by the computer simulation resisted any mathematical formulation and remained very hard to interpret [Fermi, Pasta, & Ulam 1955].

In the 1960s, however, only Smale came up with a vast synthesis which not only set up an agenda for future research and provided the intellectual and personnel means for carrying it out, but also reconstructed the prehistory of this (re)nascent mathematical domain redirecting attention towards classical results by Poincaré, Andronov and Pontryagin, and Birkhoff and Morse. (See his paper "On How I Got Started in Dynamical Systems (1959–1962)" in [Smale 1980, 147–151], based on a talk at Berkeley circa 1976.) But contrary to the illustrious predecessors he claimed for himself, Smale's synthesis remained internal to mathematics: its goals were set by abstract questions of topological classification; its language (diffeomorphisms, homeomorphisms, etc.) remained largely incomprehensible to practitioners of other disciplines. "I cannot recall in my four years at Berkeley having seen many actual differential equations," one of Smale's students, Nancy Kopell, later remembered [Hirsch *et al.* 1993, 545]. Only when contacts with Thom's IHÉS school were definitely established around 1970 would Smale tackle applications in economics, celestial mechanics, circuit electronics, and biology (see Section 2.3).

2.2. Meteorology and Lorenz's Laboratory Models

Retrospectively, the second towering figure of the decade no doubt is Edward Lorenz, whose 1963 numerical integration of a simple equation exhibited the famous strange attractor to whom his name is now attached. In this period, Lorenz was neither alone in performing numerical simulation, nor even alone in exhibiting complex behaviors stemming from the integration of simple nonlinear equations with a small number of degrees of freedom (e.g., Rikitake [1958] or Ueda [1992]).[16] But the singularity of his work stems from its particular location at the confluence of several *problématiques*—new large-scale modeling methods in meteorology, methodological reflection on the nature of models, and original heuristic use of the computer. These three issues would later be characteristic of chaos [Dahan Dalmedico 2001a]. The fact that this model was widely taken up and studied by "chaologists" over the course of the 1970s increases its importance, even if the exact mathematical properties of the Lorenz attractor would only be completely understood some 30 years later [Tucker 1999, Viana 2000]. More than the elegant exhibition of a mathematical result, Lorenz's work is better construed in terms of its impact on mathematical modeling practices.

As early as the late 1940s, John von Neumann promoted and organized the Meteorology Project in connection with his plans for building an electronic computer at Princeton. When Jules Charney joined the Project in 1948, the objective of the project was clear: "The development of a method for the numerical integration of the meteorological equations which is suitable for use in conjunction with the electronic computing machine now under construction at the Institute for Advanced Study;" and a single approach prevailed: "To consider a hierarchy of 'pilot problems' embodying successively more and more of the physical, numerical, and observational aspects of the general forecast problem."[17] So the guiding philosophy was to construct a hierarchy of increasingly complex atmospheric models, whose features at each successive step were to be determined by analyzing the shortcomings of the previous model. These models can be called *physical models*, in the sense that, although based on several more or less simplified physical assumptions, they are still trying to mimic atmospheric behaviors and adjusted by making them more complicated. Construed as a *giant calculator*, the computer made the treatment of increasingly complicated equations possible, provided better descriptions of the atmosphere, and therefore raised hopes for accurate long-term forecasts.

But Charney also suggested using the computer as an *inductive machine* capable of testing selected physical assumptions: "The machine, by reducing the mathematical difficulties involved in carrying a physical argument to its logical conclusion, makes possible the making and testing of physical hypotheses in a field where controlled experiment is still visionary and model experiment difficult, and so permits a wider use of inductive methods" (quoted in [Aspray 1990, 153]). A few years later, this same idea would lead to a very different kind of model in meteorology, which Charney called "*laboratory models*." In the late 1950s, it became obvious that equations which made crude numerical weather predictions possible with the first computers would no longer be able to provide the high quality now required for long-range forecasting, raising the very question of predictability.

[16] For a rather complete survey of numerical experimentation in conservative systems, see Hénon [1983].

[17] The above quotes come from Jules Charney's *Progress Report of the Meteorology Group at the Institute of Advanced Study, July 1, 1948 to June 30, 1949* (Jules Charney Papers, MIT Archives), quoted by Aspray [1990, 139].

Involved in the Statistical Forecasting Project at MIT, Lorenz had for several years used, like a majority of meteorologists, an array of statistical methods, the most widespread being linear regression methods. Since these often failed to yield accurate forecasts, it was debated whether the unpredictability was caused by a lack of available data or by the inadequacy of linear methods. In the MIT Project, meteorologists generated "weather maps" by dynamical prediction (i.e., through the numerical integration of a set of nonlinear differential equations), which they then tried to reproduce by means of linear regressions. Using a simple system of 12 equations, Lorenz exhibited a nonperiodic solution roughly mimicking the evolution of the atmosphere and investigated the significance of the instability. Running the simulation 40 times with different initial "errors" (small deviation in initial conditions), he observed very rapid divergences. On this basis, he concluded in 1960 that the statistical method was invalid. If this model captured something essential about the atmosphere, long-term predictability was furthermore definitively utopian.

Lorenz [1960a] presented his results during a Symposium held in Tokyo, a turning point for the community particularly instructive to us as it highlights the difficulty in locating the source of instabilities [Lorenz 1960b]. The crucial question was to assess the extent to which the model represented atmospheric evolution. With their small number of dimensions, "Lorenz-like systems," as Charney nicknamed them, could mimic reality only in a very rough manner. Asked why he assumed that sensitivity to initial data was connected with the small number of parameters in the model, Lorenz was unable to provide a more satisfying answer than that it was a "matter of feeling." Lorenz had worked hard to reach his result. It is not at all by pure chance, as many popular accounts would have it, that he discovered chaotic behaviors. In our opinion, many of these accounts greatly exaggerate an anecdote according to which, Lorenz left his computer to get a cup of coffee, only to find chaotic solutions flashing on the computer screen as he came back. (For his own personal account, see Lorenz [1993]).

Many people hoped that by adding a large number of degrees of freedom, one would stabilize the system and hence achieve long-term predictability. In 1960, Charney was still optimistic: "there is no reason why numerical methods should not be capable of predicting the life cycle of a single system," he declared; only current models did have "fatal defects" (his comment following [Lorenz 1962, 648]). Relentlessly, the same crucial question emerged: Should one impute forecasting difficulties to computers and computing methods, to models, or, more fundamentally, to the atmosphere itself?

That same year, Lorenz [1960b] also explained that the use of dynamical equations to further understanding of atmospheric phenomena, justified their simplification beyond the point where they were expected to yield acceptable weather predictions. Although they might appear as rough approximations, "maximum simplifications," Lorenz claimed, clarified our understanding of the phenomena and lead to plausible hypotheses to be tested by careful observational studies and more refined systems of equations. By judiciously omitting certain terms from the dynamical equations and by comparing the result of the prediction with reality, one could estimate the cost of these omissions and thereby discriminate between important and non-important terms. Capturing the alternative, Charney [1962b, 289] wrote that, faced with nonlinear problems, scientists will have to "*choose either a precise model in order to predict or an extreme simplification in order to understand*."

In the following years, Lorenz's system was further reduced to just three degrees of freedom. Studying the convective motion of a fluid heated from below (a very frequent atmospheric phenomenon), Barry Saltzmann, from the Travelers Weather Center at Hartford, Connecticut, used Lorenz's approach to develop a simple model involving 7 variables, four of which quickly flattened down while the other three wandered non-periodically. After hearing of Saltzmann's simulations, Lorenz concluded that the following system of three equations,

$$\begin{aligned}\dot{x} &= \sigma x + \sigma y,\\ \dot{y} &= xz + \gamma x - y,\\ \dot{z} &= xy - bz.\end{aligned}$$

might well exhibit similar nonperiodic solutions (see [Lorenz 1993, 136–160]).

In his seminal paper, Lorenz [1963] numerically integrated a special case of this system and proved that almost all solutions (of which there was a countable infinity) were unstable. This implied that quasi-periodic solutions could not exist, and that, in this system, irregular unstable trajectories were the general case. Hence a fundamental problem emerged: if nonperiodic solutions were unstable, two neighboring trajectories would diverge very quickly. Otherwise, the *attractor*—a term which appeared later and was obviously not used by Lorenz although he paid attention to the portion of phase space to which solutions tended—would have to be confined in a three-dimensional bowl, forcing two trajectories to come back very close to one another. Together with the requirement that two solutions cannot intersect, these constraints led Lorenz to imagine a special structure: an infinitely folded surface, the "butterfly" that has become familiar. Representing the attractor in terms of a two-dimensional projection, Lorenz resorted to Poincaré's first-return map to study his attractor.

Let us note that the concept of a fictive laboratory model linked to the numerical simulation methodology of this group of meteorologists (Lorenz, Charney, and others) is a significant rupture in modeling practices. These models need not be faithful representation aimed at *predicting* the weather, but rather drastically simplified models designed for *understanding* certain behaviors. And the computer is no longer merely construed as a giant calculator; no longer simply synthetic, its role becomes experimental and heuristic. Lorenz's use of the computer indeed was crucial at two levels: (1) the property of sensitivity to initial conditions—later to be widely known as the "butterfly effect"—is revealed by numerical instabilities; (2) the surprising image of the attractor exhibited on his screen by Lorenz has a much greater suggestive value than Poincaré's purely verbal—and somewhat confusing—descriptions.

The conclusions drawn by Lorenz in his paper were twofold: (1) From a theoretical standpoint, he showed that very complicated behaviors could arise from very simple systems—chaotic behavior could be generated with only three variables involved. In contradiction to the old, well-established tenet according to which simple causes gave rise to simple effects, he found that simplicity could indeed generate complexity. And (2) he also exhibited the property of sensitivity to initial conditions and thus opened a window on the understanding of turbulence. At the meteorological level (provided his model had anything to do with the atmosphere), hopes for long-range predictions were now doomed.

2.3. Catastrophe: Thom and Topological Modeling

As mentioned above, Smale's recasting of dynamical systems theory did not occur in a vacuum. If traditional specialists in differential equations might have remained skeptical of his naive conjectures, others, like mathematicians in the Soviet Union and, above all, French 1954 Fields Medal recipient René Thom, had their interest aroused by the questions his topological approach raised. The latter in fact played a crucial role in shaping not only Smale's approach to the problems of dynamical systems, but also the mathematical tools he mobilized for this task.[18]

From the late 1950s onward, Thom embarked on a research program in differential topology exhibiting many similarities with Smale's. He was actually responsible for introducing the notion of genericity in global analysis, had picked up structural stability from Peixoto and Smale, and shared with the latter credit for the attractor concept. Thom wished to classify the generic singularities of real structurally stable mappings from R^n to R^p. Like Smale with dynamical systems, Thom hoped that structurally stable mappings were generic and, on this basis, mobilized recent advances in topology to reform the classic theory of singularities of Hassler Whitney and Marston Morse. However, contrary to Smale, who more or less tried to guess the axioms that would lead to a dense set of dynamical systems, Thom chose to attack the more difficult program of classifying generic singularities and spent several years bogged down in extreme mathematical difficulties.

From his preliminary classification already published in 1956 and "whose exactitude [was] not guaranteed" [Thom 1956, 364], Thom developed the famous list of the seven "elementary catastrophes" in the following decade. By the end of the 1960s, when this program was completed for low-dimensional cases by the formal proofs of John N. Mather, Thom was already stretching its significance far beyond mathematics. While at Strasbourg, he had started to experiment with cusps in caustics in order to see whether he could find "the physical effect of a theorem of mathematics" [Thom 1991, 27]. Turning to biology, inspired by C. H. Waddington's notion of a "chreod," Thom elaborated a "dynamical theory of morphogenesis" which was presented in 1966 [Thom 1968b]. Based on the notion of catastrophes, this theory explored the appearance of forms in living organisms without paying attention to specific molecular or genetic substrates. In the following years, Thom presented models for various biological processes, as well as for linguistics.

In 1972, Thom published his *Structural Stability and Morphogenesis*, a manifesto for a new philosophical approach to mathematical modeling, which had essentially been written before 1968. The goal was to understand phenomena concerned with the apparition and destruction of form directly, that is, without relying on reductionist methods. The models built with the help of this topological approach were inherently qualitative, not suited for action or prediction, but rather aimed at describing, and intelligibly understanding, mundane phenomena. This approach would be widely—and infamously—known as *catastrophe theory*.[19]

[18] For a further development of the similarities between Thom's and Smale's programs, see Aubin [2001]. This topic is also treated at length in Aubin [1998a], while the relation between Thom's mathematical work and philosophy is analyzed in Aubin [forthcoming].

[19] Thom [1972] is the foundational text for catastrophe theory. Thom's most famous articles on this theory have been gathered by his most important follower, the English mathematician E. C. Zeeman [1977]. For histories of catastrophe theory, see Woodcock & Davis [1978]; Tonietti [1983]; Ekeland [1984] ; Arnol'd [1992]; and Aubin [1998a; 2001; forthcoming].

Beyond its failure to fulfill overblown ambitions, catastrophe theory provided a context that favored intense interactions between Thom's research school and dynamical systems specialists whose importance has often been misconstrued. As Thom [1973; repr. in Zeeman 1977, 366] himself recognized,

> We cannot consider catastrophe theory as a scientific theory in the usual sense of the term. . . . [W]e must consider it as a *language*, a method, which allows to classify, to systematize empirical data, and which provides these phenomena with the beginning of an explanation that makes them intelligible.

Albeit only properly concerned with gradient, and not general, dynamical systems, (elementary) catastrophe theory essentially was an attempt at drawing the consequences that recent topological approaches akin to Smale's hyperbolic theory had in store for the practice of modeling natural (and/or social) phenomena. It therefore provided a model for implementing topological modeling practices in various domains of science that, in particular, led Smale and his Berkeley school to applications. These "applied topologists" argued that their approach provided guidelines one could substitute for the "lucky guess" usually informing model-building [Thom 1968b]. In particular, they developed and exploited an arsenal of topologically informed notions (attractors, hyperbolic systems, genericity) that were well suited for a qualitative study of dynamical systems, which focused on identifying their global, structural, yet dynamical, features. They then dreamed of a more abstract applied mathematics that was directed "towards socially positive goals," and "accessible and attractive to the modern mathematician, one who has been brought up in the purist, Bourbakist style of education" [Smale 1972 [1980], 95 and 100].

In bitter controversies flaring out around 1977–1980, catastrophe theorists were blamed mainly for their tendency to claim too much, suggesting a willingness to let mathematics dictate how reality should be, but also for neglecting existing literature and personal contact in fields of application, for their refusal to be constrained by experimentation, and for using arcane, very sophisticated mathematical techniques without undertaking proper pedagogical efforts. The most hurtful critique was Sussman & Zahler [1978], as well as Smale's [1978] devastating review of [Zeeman 1977]. (For analyses of the controversy, in addition to the works cited in footnote 19, see Guckenheimer [1978] and Boutot [1993a]). As a result, catastrophe theory was largely discredited. But more than mathematical theory, more than philosophy, it was an actual *modeling practice* that circulated among a restricted group of mathematicians who started to build models in a great variety of domains. Reflecting on the fate of catastrophe theory, Thom [1991, 47] once declared:

> Sociologically speaking, it can be said that the theory is a shipwreck. But in some sense, it is a subtle wreck, because the ideas I have introduced gained ground. . . . Therefore, it is true that, in a sense, the ambitions of the theory failed, but in practice, the theory has succeeded.

We want to argue that catastrophe theory has indeed "survived" both in the topological approach that informed Smale's dynamical systems theory and in the modeling practices it promoted, making use of topological tools for the understanding of changes of regimes, such as in the paradigmatic case of the onset of turbulence.

2.4. Disciplinary Confluence: The Ruelle-Takens Theory of Turbulence

Let us come to the third dominant figure in the intellectual and disciplinary reconfiguration that we associate with the emergence of chaos—mathematical physicist David Ruelle. In what follows, we focus on the 1971 article coauthored by Ruelle and Floris Takens and deliberately neglect most of their later work, which had considerable importance for the development of dynamical systems theory and chaos (for reprints, see Ruelle [1994]). As will appear clearly, our reason for doing so is that we are mostly interested in the social process by which abstract mathematical theories came to be seen as immensely relevant to hydrodynamics, physics, chemistry and so forth. This, together with our interest in the IHÉS local context, explains why we single Ruelle out.

Before the controversy on catastrophe theory, to the intense collaboration network established amongst Berkeley, Warwick, and IHÉS, to the missionary atmosphere it generated, specialists from other disciplines were attracted. Ruelle, Thom's colleague at IHÉS, was among them. Having worked on quantum field theory and statistical physics, in October 1968 he was already "trying to look at some problems of hydrodynamics or, more generally, of 'dissipative phenomena' from a physical point of view analogous to Thom's" [quoted in Aubin 1998a, 358]. Less than three years later, he published with Takens an article titled "On the Nature of Turbulence" [Ruelle & Takens 1971] in which they suggested, but did not show rigorously, that turbulent motion in fluid could be explained mathematically by the existence of generic "strange attractors" in the system of the Navier–Stokes equations (NSE), rather than by an infinite accumulation of oscillatory modes as was commonly assumed by (part of) the physics community [Landau 1944]. In this explanation of turbulence, disorder stemmed from the topological character of the system of equations governing fluid flows, rather than external noise; it was a dynamical, not statistical, property of the system. *Nonperiodic*—not quasi-periodic—motion was the definition they offered for it.

A striking feature in Ruelle & Takens [1971] article underscoring the new status assigned to differential equations was the form they gave to NSE, the fundamental law for fluid flows. Indeed in their paper, the equations are nowhere more explicitly specified than in the following:

$$\frac{dv}{dt} = X_\mu(v).$$

"For our present purposes," they added, "it is not necessary to specify further . . . X_μ" [Ruelle & Takens 1971, 168]. A unique parameter depending on physical characteristics, μ represented external stress on the fluid (e.g., the Reynolds or Rayleigh number). When $\mu = 0$, the fluid tended to rest; for small μ, it tended toward a stationary motion in which the velocity field remained constant. At a critical value μ_1, the system went though a so-called Hopf bifurcation: the velocity field started to oscillate at a given frequency ω_1.[20] At a further critical value μ_2, a second bifurcation gave rise to a frequency ω_2, and so on. When μ increased sufficiently, "the fluid motion becomes very complicated, irregular and *chaotic*, we have turbulence" [Ruelle & Takens 1971, 168, our emphasis]. Contrary to most hydrodynamicists, Ruelle and Takens were not interested in computing particular critical values, but only looked at general features of motion as the parameter increased. Based on their topological knowledge, Ruelle and Takens claimed that since the quasi-periodic

[20] Building up on Poincaré's intuitions, German mathematician Eberhard Hopf had studied the details of the passage from stationary solutions (corresponding to fixed points in phase space) to periodic ones (*viz.* limit cycles), a pioneering work of *bifurcation theory*, which, albeit noticed by Thom, Mira, Möser, and a few others, remained nearly confidential until commented upon by Ruelle and Takens close to 30 years later. For his original papers, see Hopf [1942; 1948].

motion was not generic for general dissipative systems, it had no chance of being observed. One had to look elsewhere for a "mathematical explanation" of turbulence [Ruelle 1972].

The introduction of the notion of strange attractor was not so much a mathematical innovation as it was an acute perception of the physical significance of Smale's horseshoe and similar systems which had generally been dismissed as "mathematical deductions" that could "never be utilized"—to use Duhem's characterization of the sensitivity to initial conditions exhibited by Hadamard for the geodesics of manifolds with negative curvature [Duhem 1906 [1954], 138]. Ruelle's accomplishment is therefore best interpreted as a crucial rapprochement between a modeling practice inspired by Thom and the tools of dynamical systems theory mainly developed by Smale.

As opposed to most catastrophe-theoretical speculations and forays into applied areas, two major distinctive features of Ruelle's work were the prediction from the Ruelle–Takens hypothesis of a measurable difference from earlier schemes for turbulence—an experimental test—and the establishment of some contacts with the hydrodynamics community. At first timidly, a process of sociodisciplinary convergence between two different traditions, dynamical systems theory and fluid mechanics, was launched. Except for references to old works by Leray, Hopf, and Landau, Ruelle's initial inspiration was hardly built upon a disciplinary tradition proper to fluid mechanics. Only with an appendix written more than a year after the main body, and in which he packed up hydrodynamic references, did Ruelle actually get involved with this field. In the early 1970s, fluid mechanics was a huge, disparate constellation at the confluence of mathematics and engineering. A small community existed that focused on problems of loss of stability by fluid flows submitted to increasing stress. Its practitioners were heirs to a technical mathematical tradition that proved capable of accommodating the topological approach championed by Ruelle.[21] Slowly, several hydrodynamicists would be attracted to the dynamical systems approach, while several other sociodisciplinary groups taking up problems of hydrodynamic instabilities for various reasons would adopt the dynamical systems language as adapted by Ruelle and Takens (Section 3.2).

At the very end of the 1960s, therefore, the limitations of older statistical methods in the task of understanding the nature of disorder had become obvious; elements were in place for the recognition of the inescapable role that complex ("chaotic") dynamical systems had to play in the understanding of the world. Indeed, deterministic chaos was made relevant for the study of natural phenomena by the immediate simplicity of Lorenz's results. But a correct comprehension of them depended on a careful exploitation of Smale's topological approach to dynamical systems theory. Inspired by Thom's similar attempts with catastrophe theory, Ruelle independently from Lorenz showed why this new approach was significant physically. For this message to be driven home to large audiences, serious sociodisciplinary convergence processes needed to be launched. From this viewpoint, the confluence of two disciplines, the mathematical theory of dynamical systems and the theory of nonlinear hydrodynamic stability, constituted a major turning point.

[21] The standard exposition of hydrodynamic *linear* stability theory was by C.-C. Lin [1955]. In the early 1970s, Daniel Joseph, Gérard Iooss, and David Sattinger were busy developing *nonlinear* stability theory in a highly rigorous functional analytical style.

THESIS 3: CONVERGENCE AND RECONFIGURATION

From our point of view, the 1970s were a period of rupture in the domain under consideration here, "dynamical systems and chaos." This rupture was expressed by two simultaneous movements: (1) a socio-professional convergence among groups of scientists coming from a priori different disciplines who met and diversely interacted, and (2) a conceptual, intellectual reconfiguration centered around new research themes, conceptual objects, theoretical methods, and experimental systems. Symbolically, these two movements peaked in November, 1977, with the New York conference on bifurcation theory and applications. At the time, the rupture was diversely interpreted: scientific revolution, complete with the advent of a new paradigm, for some, vs a much greater feeling of continuity among mathematicians.

3.1. Bifurcation in New York, 1977: The Mathematical Skeleton of Chaos?

By its very nature, the sociodisciplinary convergence forming the heart of deterministic chaos was heterogeneous. By 1975, several elements had fallen into place. Dynamical systems theorists, applied mathematicians, statistical physicists, hydrodynamicists, population biologists, etc., started sharing references, concepts, and problems. Experiments were performed with the stated aim of verifying the Ruelle–Takens hypothesis. American physicists Harry L. Swinney and Jerry P. Gollub from the City College of New York had observed a transition to turbulence seemingly following the path indicated by Ruelle and Takens [Gollub & Swinney 1975]. Numerical experimentation was slowly gaining credence as an acceptable path for theoretical, and even mathematical, research. In one such experiment on truncated Navier–Stokes equations, John B. McLaughlin and Paul C. Martin had shown a "semiquantitative agreement" with the Ruelle–Takens conjecture [McLaughlin & Martin 1975]. In the process, Lorenz's results were spectacularly revived by applied mathematicians and theoretical physicists. (For a citation analysis, see [Elsner & Honoré 1994].) At Berkeley, a yearlong seminar in 1976–1977 put specialists in hydrodynamics and dynamical systems in close contact with one another, and rigorously tackled the Lorenz attractor [Bernard & Ratiu 1977]. Investigating the complicated behavior of iterated maps (and rediscovering a special case of more general results previously obtained by A. N. Šarkovskij [1964]), applied mathematicians Tien-Yien Li and James A. Yorke gave a name to the emerging study of complicated, apparently erratic, motions in low-dimensional systems described by simple deterministic laws—"chaos" [Li & Yorke 1975]. Above all, perhaps, May successfully publicized their result and some of its consequences for modeling practices in various areas and concluded that "we would all be better off if more people realised that simple nonlinear systems do not necessarily possess simple dynamical properties" [May 1976, 467].

Although representatives from one or more of these various domains gathered several times at other conferences from 1973 to 1977 (see below), one is bound to consider the New York Academy of Sciences conference *Bifurcation Theory and Applications in Scientific Disciplines*, held from October 31 to November 4, 1977, as a special moment in the history of chaos [Gurel & Rössler 1979]. Organized by Okan Gurel of the IBM Corporation and Otto Rössler from the University of Tübigen, the conference proceedings were dedicated to Eberhard Hopf on his 75th birthday. Besides the old mathematician, Lorenz was also greeted by a triumphant ovation as his 1963 paper was definitely brought back into the limelight. The conference was moreover attended by Ruelle and Smale, several of his former students, theorists and experimenters of hydrodynamic stability, Mandelbrot, May, Yorke, physicists from Hermann Haken's "synergetics" school in Stuttgart, chemists from Ilya Prigogine's "dissipative structure" school in Brussels, economists, biologists, and many others (74 authors are listed in the proceedings).

The motive behind this large interdisciplinary gathering was twofold: (1) the need to confront analyses of disordered, unstable phenomenological behaviors distinct by their intrinsic nature, yet similarly describable by simple mathematical models, and (2) the need to diffuse and/or acquire mathematical tools that permitted a better understanding and formalization of these phenomena. On the fore thus was a constant juxtaposition, and sometime clash, between mathematical approaches and questions stemming from other domains. Emphasizing critical solutions as an area of future development, Gurel for example wrote of the contrast between mathematics and what he, conforming to tradition but inaccurately, termed "applications:"

> Mathematicians are proving theorems stating necessary and sufficient conditions for the existence of such solutions. . . . In turn, the new discoveries in critical solutions will open up possibilities for modeling complex systems with an increasing exactness. Applied scientists are incorporating these new mathematical findings in their analyses of applications to practical problems. In both theory and applications, the need for ingenious techniques adapted from applied mathematics will be undoubtedly felt [Gurel & Rössler 1979, 3].

Overall, this ecumenical conference was unquestionably held together by mathematical glue, namely bifurcation theory. History played an important role in introducing the theme in a manner that was at times strikingly basic. Reaching back to Poincaré and Euler, Gurel presented bifurcation theory as an analysis of "how the *parameters* effect the qualitative variations in the solution space" [Gurel & Rössler 1979, 1]. Besides elementary introductions recounting old mathematical techniques from Poincaré, Lyapunov, and Hopf, advanced research papers on strange attractors and homoclinic theory skirted pedestrian exposés of simplistic models, reports of actual and/or numerical experiments, and wild speculations. As Gurel acknowledged in his closing remarks, from the participants emerged a focus on phenomena other than bifurcation, "such as 'chaos' and the creation of 'strange attractors' [which] were discussed in not one but many papers presented in diverse sessions" [Gurel & Rössler 1979, 685]. From the head-on confrontation, new conceptual configurations emerged with force.

Au: effect [sic] or affect?

3.2. Convergence Before Revolution: The Case of Rayleigh–Bénard

The sole consideration of the above conference, or for that matter others similar to it, leave few doubts about the leading role played in the "chaos revolution" by the adoption, and possibly adaptation, of a "dynamical systems approach" to the study of complex behaviors [Hirsch 1984]. But it obscures the fact that the socio-professional convergence was prepared by several partial, overlapping junctions at the subdisciplinary level. In a few cases, indeed, it would be more accurate to view the adoption of dynamical systems rather as a consequence of—not a cause for—fruitful encounters among diverse scientific groups. Indeed, one of the most fascinating and intriguing features, striking to early "chaologists," was this communication across disciplinary boundaries. Emphasizing this aspect of chaos, Saclay physicist Pierre Bergé, who designed chaotic experiments in fluids, once declared:

> among the general qualities which I am pleased to recognize in the study of dynamical systems, there is a *very enriching collaboration* between theoreticians and experimentalists and—even more remarkable—between mathematicians and physicists [Bergé *et al.* 1984, 267].

It will not suffice to characterize this collaboration as the mere recognition of commonalties in the mathematical tools that were used. Instead of *tools*, one may focus on *problems*, for example the Rayleigh–Bénard (RB) system, a simple convection cell whose heating from below induces instability.[22] Prior to the late 1960s, except for a small number of hydrodynamicists very few people were at all interested in simple RB. A physicist from the Bell Laboratories, Günter Ahlers, later recalled the prevailing mood in the early 1970s before he took up the problem: "It seems difficult to imagine from our present vantage point; but to my knowledge none of us had ever heard about this phenomena as an interesting physical system" [Ahlers 1995, 94]. Ten years later in Grenoble, a symposium solely devoted to convection welcomed 57 papers presented by 65 participants from 15 countries [Hopfinger *et al.* 1979]. RB had meanwhile become a hot research topic whose theoretical understanding was naturally helped by making full use of dynamical systems theory. But this new grasp on the subject was not the reason for the sudden surge of interdisciplinary interest in RB. By focusing tightly on the history of this problem, one notices clearly that dynamical systems theory is introduced rather late in the story, and certainly *after* the moment when several groups belonging to various communities had already directed their attention to it. As a result, RB therefore appears as a *boundary system* ("boundary objects;" see [Star & Griesemer 1989]), which served as facilitator for both community convergence and exchanges of scientific practices.

In the first half of the 1970s four groups were concerned with the RB system:

(i) The variegated community of hydrodynamicists, including theorists, experimenters, and specialists in numerical computation, who had accumulated extensive knowledge about RB and maintained close contact with one another, but still felt somewhat unsatisfied with the overall theoretical situation, and were thus on the lookout for innovations allowing them to make progress in the study of the nonlinear stability of transitions to turbulence [Velarde 1977].

(ii) The statistical physicists specializing in the study of critical phenomena and phase transitions, who believed that the onset of turbulence could benefit from a theoretical attack making use of recently discovered universal renormalization methods [Wilson 1971]; as well as experimenters from the same domain who at about the same moment found that their own techniques and tools (thermometry, laser light-scattering, computers) could be applied to turbulence studies with a precision previously unheard of.

(iii) The physicists studying plasmas and liquid crystals, and especially those from Pierre-Gilles de Gennes's Orsay group, for whom problems of phase transitions and turbulence naturally occurred in a simultaneous and inseparable manner and who selected RB as a test case for their theories. After his appointment at the Collège de France in 1970, de Gennes thus made strenuous attempts to have physicists invest the field of fluid mechanics, which he felt they had for too long neglected.

[22] This approach has been followed in Chapter 8 of [Aubin 1998a].

(iv) The out-of-equilibrium thermodynamicists and chemists from the Brussels school set up by Prigogine who in the late 1960s emphasized the formal analogy between hydrodynamic instabilities and his "dissipative structures," i.e., the spontaneous organization of matter arising in out-of-equilibrium situations such as the Belousov–Zhabotinsky chemical reaction. Prigogine and his coworkers seized RB with the feeling that mathematical techniques were there more developed than elsewhere [Glansdorff & Prigogine 1971].

In 1973, all the above groups got into formal contact at a conference on instability and dissipative structure at Brussels [Prigogine & Rice 1975]. For an attack on the RB problem, many of the crucial interdisciplinary links were by then already well established. Remarkably, however, no one at the Brussels conference even mentioned the Ruelle & Takens [1971] paper, nor dealt seriously with qualitative dynamics. But their very desire to approach hydrodynamic instabilities in an interdisciplinary way was a fertile ground on which could later prosper the dynamical systems approach. Dynamical systems theorists (except for Ruelle) only joined the groups listed above in the second half of the decade (see [Bernard & Ratiu 1977]).

In 1975 at least four very disparate conferences dealt one way or another with questions of hydrodynamic instabilities and involved one or more of the groups identified above. The proceedings of these conferences were all published. They were held: (1) at Geilo, Norway, in April 1975, on the topic of "Fluctuations, Instabilities, and Phase Transitions" [Riste 1975]; (2) at Orsay, France, in June, focusing on "Turbulence and the Navier–Stokes Equations" [Tenam 1976]; (3) at Dijon, two weeks later, devoted to "Physical Hydrodynamics and Instabilities" [Martinet 1976]; and finally (4) at Rennes, in September, on the topic of "Dynamical Systems in Mathematical Physics" [Keane 1976]. In each the Ruelle–Takens hypothesis and strange attractors were now discussed and seen as major influences in reorienting instability research and in adopting a language inspired by Smale's dynamical systems theory. At one of these conferences held in Dijon, Martin, a Harvard theoretical statistical physicist, skillfully synthesized various threads [Martin 1976a, see also Martin 1976b]. For the very first time three of the now classic ingredients of chaos theory, namely the works of Lorenz, May, and Ruelle and Takens, were together interpreted as various scenarios for the onset of turbulence. In 1976, Martin together with Gollub convened a conference at Tilton, New Hampshire, which in many ways prefigured the 1977 New York meeting. From July 19 to 24, more than a hundred physicists, chemists, hydrodynamicists, and dynamical systems theorists attended some twenty-odd 45-minute talks at this meeting focusing on "dynamical instabilities and fluctuations in classical and quantum systems."[23]

3.3. Computer and Engineering Mathematics, Toulouse, 1973

To characterize the dual movement—convergence and reconfiguration—at the heart of chaos merely as the recognition of a mathematical skeleton common to all chaotic phenomena, namely dynamical systems theory, therefore appears quite reductive, especially when the latter is narrowed down to Smale's synthesis. Not only was the convergence process much more complex in sociological terms than the straightforward encounter of

[23] We thank P. C. Martin for sending us the program of this Gordon Research Conference. Another momentous conference, organized by Joseeph Ford and Giulio Casati, took place in Como, Italy in the summer of 1977. On the French side, let us note the Cargèse 1977 summer school and the Nice "Meeting between Physicists and Mathematicians" convened by J. Coste, P. Coullet, and A. Chenciner, held in September 1977.

applied communities with mathematicians, as we saw above, but even the conceptual reconfiguration cannot be construed simply as the slow recognition of the value of a topological theory *à la* Smale. Albeit quickly becoming hegemonic, this scarcely was the only mathematical or theoretical framework that was mobilized.

An international conference organized by Christian Mira, from the Laboratoire d'automatique et d'analyse de systèmes (CNRS), Toulouse, will make this point explicit [Mira & Lagasse 1976]. Held in September 1973, this meeting titled "Transformations ponctuelles et applications" gathered some of the pioneers in the nonlinear sciences (J. Ford, M. Hénon, J. H. Bartlett, C. Froeschlé, as well as several Soviet scientists including B. V. Chirikov). Intended as "a meeting of specialists from various disciplines but utilizing mathematical tools of a similar nature," this conference was thematically centered on the notion of two-dimensional recurrences of the type $x_{n+1} = f(x_x, y_n, \lambda); y_{n+1} = g(x_n, y_n, \lambda)$. The focus still was on dynamical systems, but with a quite different flavor. Rather than fluid mechanics or population dynamics, themes issued from celestial mechanics and engineering science here dominated (control systems with sample or pulse data, problems of mechanical control and regulation, etc.); the domains of application mentioned were astronomy and particle accelerator design. Traditional tools like Poincaré's homoclinic theory, Lyapunov's stability criteria, Birkhoff's recurrence set, bifurcations, differential equations, discrete recurrences were preferred to more directly topological notions such as genericity or strange attractors. Like Smale, Mira situated himself, both at the time and subsequently, at the confluence of several historical traditions and fought for the recognition of a concrete approach to dynamical systems joining numerical computation and sophisticated mathematical methods, that would be distinct, in particular, from Smale's abstract, topological approach. In several historical introductions he adjoined to his work, Mira displayed how he was following the steps of the masters Lyapunov, Poincaré, and Birkhoff, reviving more or less forgotten mathematical work on recurrences (Lattès [1906], Julia [1918], Myrberg [1962]; for more on this, see Section 4.1 below); directly picking up Andronov's legacy without Lefschetz's mediation; paying attention to his Gorki school (in particular, Yu. I. Neimark's work leaning heavily toward control theory, as well Chirikov's more abstract results), but also to other results from the Soviet Union which followed a more analytical approach (such as those from the Kiev school, and in particular the Malkin [1956] book on the method of small parameters).

But, in 1973, the most conspicuous characteristic of the Toulouse conference surely was the prominent position given to the computer. "The renewal of interest for the works of Poincaré–Birkhoff," Mira wrote in his introductory statement of the conference proceedings [Mira & Lagasse 1976, 24]," was made possible only through the use of high-speed and high-capacity computers." Analog and digital devices were mobilized for producing particular solutions and bifurcations analyzed using mixed numeric–analytic methods. Contrary to Smale, Ruelle, and Thom, most of those who participated to the Toulouse conference indeed had a long practice in numerical computing. From 1964 to 1974, Hénon and Heiles in France, McMillan, Ford, and Bartlett in the United States, as well as Gumowski at CERN, had used this tool for problems stemming from celestial mechanics or accelerator design and the study of homoclinic and heteroclinic structures. Collaborating with Gumowski especially, Mira had set up at the LAAS a whole research group, supervised the doctorates of a half-dozen students, and associated the analytic and numeric study of bifurcations for two-dimensional recurrences with numerous applications in the theory of automatic control, integral pulse frequency modulation, and ACDC rectifiers, as well as other mathematical fields such as differential equation theory (for a survey, see [Lagasse & Mira 1972]). Following Poincaré, Lyapunov, Andronov, and Malkin, they paid a special attention to questions of stability. Exhibiting "regions of stochastic motion," they produced strikingly beautiful pictures (recalling Mandelbrot's famous fractals) that were displayed at the 1973 Toulouse conference. In a later communication, they emphasized that in order to be "aesthetically appealing" the patterns "must possess simultaneously a sufficient amount of complexity and regularity" (Gumowski & Mira [1974], 851).

But the pertinence of such study was not obvious to everyone. Replying to Mira's inquiry about CERN's eventual interest for two-dimensional nonlinear recurrences, Gumowski for example replied on November 26, 1970:

> both on technological and operational levels, the study of such nonlinearity is not urgent [since] it does not affect the practical exploitation of accelerators. . . . I personally think that a systematic understanding [of such recurrences] can come out of the studies begun by your initiative at LAAS, but I fear that your fame, if there is fame, will be posthumous.[24]

Later to be perpetuated, albeit less visibly than Smale's synthesis, by the *International Journal of Bifurcation and Chaos in Applied Sciences and Engineering* founded in 1991, the social confluence of the Toulouse conference shows that the theoretical notions picked up from dynamical systems theory for describing chaotic behaviors were much more multicolored than the usual post-facto short shrift could lead us to believe. In each period from the end of the 1920s up until the very recent past, interactions between mathematics and engineering science have played a crucial role in stimulating the development of new mathematical tools and methods. Stemming from various technological domains (radio engineering, electronic engineering, particle accelerator design, regulated mechanical systems, etc.), the study of "concrete dynamical systems" gave rise to research work in nonlinear analysis which frequently looped back to the seminal works of Poincaré, Lyapunov, and, later, Andronov.

But in the context of the image war between pure and applied mathematics, which shook the international mathematical scene after 1945 and which globally saw the hegemony of the former in public representations of the domain [Dahan Dalmedico 1999a and 2001b], the notion of fundamental advances made on such a basis was, for a large portion of the mathematical community, difficult to swallow. After all, computer programming, Gumowski emphasized as early as 1963, was "an art, in the practice of which there is no substitute for sound judgment and a lot of experience" [Gumowski 1963, 36]. In total opposition with Bourbakist ideals, this research tradition has therefore tended to be occulted from the historiography of dynamical systems [Aubin 1997].

When we consider what has come to be labeled as "deterministic chaos," the picture becomes ever more complex. The intrinsic multidisciplinarity of the convergence meant that scores of theoretical tools coming from other disciplines than dynamical systems theory *per se* were explored and utilized in order better to understand various phenomena involving the apparition or disappearance of order. As was mentioned earlier, dissipative structures, catastrophes, and the renormalization group were all mobilized at one time or another and crossbred. Other mathematical domains also intervened such as ergodic theory, measure

[24] Gumowski to Mira (November 26, 1970). We thank Christian Mira for allowing us to consult some of his personal papers.

theory, and functional analysis. As a result the theoretical framework of chaos theory, while overlapping greatly with dynamical systems theory as expounded by Smale, reached far beyond it. The next section will make it clear that, as the chaos convergence and reconfiguration was pursued, emphases on notions stemming from parts of the confrontation other than dynamical systems theory played a prominent role.

THESIS 4: THE VIEW FROM PHYSICS

Between the end of 1970s and the end of the 1980s, the dual motion of convergence and reconfiguration was stabilized and reached out to other disciplinary domains. A striking characteristic of the domain remained the constant confrontation of theoretical, numerical, and experimental results. There however were two important new elements coming into play: (1) With Feigenbaum's work, the crucial relevance of renormalization methods for chaos theory, was finally established, revealing an intimate connection between critical phenomenon physics and dynamical systems theory. The encounter of renormalization group methods and dynamical systems theory signaled the emergence of a new type of physics, which one could label "universal."(2) Once chaotic dynamical systems were accepted for the study of natural phenomena, their emergence starting from equilibrium configurations became central question: stemming from bifurcation theory with the turbulence problem, notions such as scenarios and roads to chaos acquired a crucial significance.

4.1. Feigenbaum: Universality in Chaos

It has been discomforting—and indeed irritating—for those tending to identify deterministic chaos with dynamical systems theory that the work of Los Alamos statistical physicist Mitchell Feigenbaum linking chaos to universality and renormalization theory was widely hailed as momentous, fundamental, and revolutionary. Emphasizing this contribution, one of the first collections of reprints on this topic was thus titled *Universality in Chaos* [Cvitanovic 1983]. (Note however that Pedrag Cvitanovic, a particle physicist at the IAS in Princeton, had collaborated with Feigenbaum [Gleick 1987, 183].) While making him perhaps the most conspicuous Romantic hero of his popular account of chaos, Gleick [1987, 182] acknowledged that Feigenbaum's "semicelebrity" had indeed been "a special source of contention." No other contribution than his encapsulates so nicely the conflicting perceptions depending on disciplinary affiliations.

One way of understanding the impact of Feigenbaum's results is to notice that these finally established the fruitfulness of an extremely appealing, but often disappointing, analogy that had guided the research of many theoreticians and experimenters alike; that is, the analogy between critical phase-transition behaviors and instabilities at the onset of turbulence [Feigenbaum 1978; 1979a & b; 1980].[25] Moreover, this was accomplished through the study of recurrences that had been an early focus of investigation by vocal promoters of chaos.

[25] Let us note here that Feigenbaum's priority claims have sometimes been contested, in particular by Charles Tresser and Pierre Coullet from the Laboratoire de la matière condensée in Nice; see [Coullet & Tresser 1978; 1980; Arneodo *et al.* 1979]. The universal properties of recurrences were also noticed by [Derrida *et al.* 1977; 1978]. A bitter comment about this controversy is found in [Bergé *et al.* 1994, n. 2, 269 & nn. 8–9, 289]. Such priority claims are less a concern for the historian, however, than the extensive networking between communities that followed the publicity given by Feigenbaum to his own results and in particular the close contacts he established with experimenter Albert Libchaber at the École normale supérieure in Paris.

Exhibiting their universal properties, Feigenbaum established the relevance of simple equations to the modeling of complex behaviors and quickly led to the actual measurement of critical exponents in physical systems, and, to start with, in a cell filled with liquid helium heated from below—in other words, a RB system (Section 3.2).

In the following years, many laboriously emphasized that Feigenbaum hardly was the first one to consider the bifurcation patterns of recurrences of the type $x_{n+1} = f_\lambda(x_n)$ as the parameter λ was varied. That this was an interesting mathematical problem had indeed not escaped the attention of a few isolated mathematicians. This was a classic topic, already touched upon by Julia [1918], Fatou [1919–1920; 1926] and others, mainly in the case of complex functions. From 1958 to 1963, Finnish mathematician P. J. Myrberg published a series of papers investigating the bifurcation properties of the quadratic map $x_{n+1} = x_n^2 - \lambda$ as λ went from $-1/4$ to 2. Myrberg was the first to study the limit cycles of this equation, thereby identifying the cascade of period-doubling bifurcations that have since made Feigenbaum's name famous. For the most part published in a Finnish journal, Myrberg's results received little notice. An article in the *Journal de mathématiques pures et appliquées* [Myrberg 1962] was, however, known to Mira, for example. In Kiev, A. N. Šarkovskij [1964] (discussed in [Stefan 1977]) proved more general theorems than Li & Yorke [1975] would a decade later. Exploiting the work listed above, Mira and Gumowski used numerical methods to exhibit sophisticated special cases of bifurcation, such as the "structure boîtes-emboîtées" (called "embedded boxes" by Guckenheimer [1979]) arising when two singular points of different natures, a critical point and a repulsive cycle, merged. First introduced by Gumowski & Mira [1975], embedded boxes, as well as the whole approach of the Toulouse group, was explained in their book [Gumowski & Mira 1980] and its English version [Gumowski & Mira 1982]. (For historical accounts of recurrence problems informed by this experience, see Mira [1986; 1996]. For another early mathematical survey from a point of view closer to Feigenbaum's and Ruelle's, see [Collet & Eckmann 1980]). Most importantly for Feigenbaum, this was also a domain investigated by his colleagues in the Theoretical Division at Los Alamos [Metropolis *et al.* 1973], who discovered that the period-doubling cascade of bifurcations occurred, not only for quadratic maps, but also in a large class of recurrence functions. Note that recurrences were used for the generation of "pseudorandom" numbers on which the Monte Carlo method designed by Metropolis and Ulam relied [Galison 1997, 702–709].

The relevance of such recurrences to the study of dynamical systems was already obvious from the technique of the Poincaré map. And, from a phenomenological viewpoint, recurrences had indeed been a major field of inquiry for early "chaologists." Already in 1964, Lorenz investigated the behavior of quadratic recurrences, going as far as hinting at their relevance to turbulence: "This difference equation captures much of the mathematics, even if not the physics, of the transition of one regime of flow to another, and, indeed, of the whole phenomenon of instability" [Lorenz 1964, 10]. Picking up on Lorenz's study, applied mathematicians Li and Yorke [1975] and May [1974; 1976] had drawn the attention of a wide audience to the fact that *even* the simplest equations, if nonlinear, could exhibit extremely complicated behaviors. But who was to say whether such simplistic equations captured anything essential about actual natural phenomena?

Following the great surge of publicity given to these studies, Smale underscored that the form he had given to dynamical systems theory was explicitly designed to encompass discrete systems. After having listened to a talk delivered by the Berkeley mathematician at Aspen, in the summer of 1975, Feigenbaum was inspired to pick up the problem of recurrence. With the help of a programmable HP-65 pocket calculator, he investigated the bifurcation sequence of the logistic map:

$$x_{n+1} = 4\lambda x_n(1 - x_n).$$

Like Myrberg, Feigenbaum noticed that the period of the stable attracting orbits doubled each time the parameter passed a critical value λ_i, with x oscillating between 2^i values. Unlike all his predecessors (except perhaps for Coullet and Tresser who, at about the same time, independently discovered the same result), he noticed that this series converged towards a critical value λ_∞ at a fixed rate defined as

$$\delta = \lim_{i\to\infty} \frac{\lambda_{i+1} - \lambda_i}{\lambda_{i+2} - \lambda_{i+1}} = 4.6692016\ldots.$$

This number was *universal*, that is, characteristic not only of the logistic map, but also of transcendental recurrences like $x_{n+1} = \lambda \sin \pi x_n$. In fact, a very large class of functions of the interval (with a single, smooth, locally quadratic maximum) exhibited the very same universal behavior.

More remarkable was Feigenbaum's use in his demonstration of renormalization group methods that had just been developed by Kenneth G. Wilson [1971] (a work for which he received the Nobel prize for physics in 1982). Adapting methods introduced in the 1950s for quantum electrodynamics, Wilson had conferred, to use Cyril Domb's expression, "respectability" on the study of critical phenomena in phase transitions. In October 1969 Domb had indeed written: "Unifying features have been discovered which suggest that the critical behavior of a larger variety of theoretical models can be described by a simple type of equation of state. But the rigorous mathematical theory needed to make the above development 'respectable' is still lacking" [quoted in Domb 1996, xi]. (For another book on the history of renormalization, esp. in the case of quantum field theory, see Brown [1993]. For renormalization in QED, see Schweber [1994]). A "vast domain of physics [which had] constituted itself 'horizontally"' [Toulouse & Pfeuty 1975, 7] explored similar transitions in magnetic materials, superfluid helium, superconducting metals, etc. Physicists suspected that such analogies that could be rigorously codified in terms of so-called "scaling laws."[26] As Cao & Schweber [1993, 60] have written, "Leo Kadanoff [1966] derived Widom's scaling laws using the idea—which essentially embodied the renormalization group transformation—that the critical point becomes a fixed point of the transformations on the scale-dependent parameters" of a physical system. Systematizing this procedure, Wilson's scheme related the various parameters characterizing the physics at different scales through the renormalization group transformations. At the critical point, the behavior of the system was scale invariant which provided a justification for the "hypothesis of universality," which expressed the idea that "apparently dissimilar systems show considerable similarities near their critical point" [Kadanoff 1976, 2]. Like scaling, the "semi-phenomenological concept" of universality was

[26] The scaling laws (i.e., relations among critical exponents) expressed by Widom [1965] provided the main starting point for Wilson. About scaling laws, see Cao and Schweber [1993, 60] and Hughes [1999, 111]. See also Wilson's Nobel lecture [1983] for his account of progress in statistical physics in the mid-1960s.

introduced by Kadanoff [1971], which Wilson turned into "real calculations of critical point behavior" [Kadanoff 1976, 2].

Noticing that portions of the twice-iterated map $f_\lambda(f_\lambda(x))$ when scaled appropriately were similar to the original function $f_\lambda(x)$, Feigenbaum realized that tools commonly used in renormalization theory could be usefully applied to recurrence problems. Starting from f_λ, one could exhibit a hierarchy of functions g_r, mapped into one another by a scaling T (for which $g_{r-1} = \mathrm{T}g_r$) and converging towards a scale-invariant function g, verifying $g(x) = -\alpha g(g(-x/\alpha))$, where α was another universal number equal to 2.502907875 . . . . The fixed point g was unstable (a saddle point in functional space), but, in perfect analogy with renormalization methods used by Wilson, the study of the linear operator $D\mathrm{T}$ (the derivative) in the neighborhood of the critical point g led to the expression of power laws. Thus could the equation defining δ be expressed as a power law for the period of limit cycles $P(\lambda_i) \sim (\lambda_\infty - \lambda_i)^{-\nu}$ where the critical exponent $\nu = \log 2/\log \delta$. A formal proof of Feigenbaum's conjectures was provided by Oscar E. Lanford III from Berkeley, incorporating them in the secure body of mathematics [Lanford 1982]. Symbolically, the proof turned out to be computer-assisted and to rely heavily on dynamical systems theory. (See also [Collet *et al.* 1980]).

4.2. Renormalization and the Nature of Modeling

To understand the excitement that greeted Feigenbaum's results, one has to remember that, for several years prior, attempts at exploiting the appealing analogy between phase transitions at critical points and hydrodynamic instabilities had mainly been met with bitter disappointment. This potent analogy took many forms. For experimentalists, the phenomenological analogy served as a basis for applying their setups to hydrodynamic phenomena and thus come up with data of greatly improved accuracy. (Nearly all the early experimenters on the onset of turbulence in fluid, Ahlers, Gollub, Swinney, Bergé, Dubois, Libchaber, were not at first specialists in hydrodynamics but came to it from an earlier interest in critical phenomena and phase transitions. On this, see Section 4.3 below). The theoretical analogy also guided the work of de Gennes and Prigogine and their respective groups. Similarly, Thom interpreted Landau's mean-field theory for phase transitions in terms of catastrophes.

Au: 4.3 as meant?

Wilson's original paper already signaled the relationship between renormalization group methods and dynamical system theory, going as far as suggesting that in many dimensions, "the solutions of the renormalization-group equations might instead [of the critical saddle point] approach a limit cycle . . . or go into irregular oscillations (ergodic or turbulent?)" [Wilson 1971, 3182]. In support of this suggestion, he cited Minorski [1962], a book mostly written during World War II. This suggestion was pursued by French physicist Gérard Toulouse who, impressed by Thom's program, used ebullient language to develop the "conceptual kinship" that constituted "a new framework for thought" [Toulouse & Pfeuty 1975, 8–9]. From scale invariance near the critical point, it followed that universal behaviors were independent from atomic and molecular details, or to use Thom's language, from the "physicochemical substrate." For renormalization theory, the dynamical systems approach had a "great heuristic value," since in both cases

> one is led to a global approach to phenomena, to an analogous classification of singularities, to a similar understanding of universality properties. This rapprochement may be noted, for it is perhaps indicative of a theoretical moment in the formation of a certain level of knowledge [Toulouse & Pfeuty 1975, 32].

The analogy with dynamical systems was, however, severely limited by the fact that, in renormalization theory, one was mainly interested in fixed points. Limit cycles, much less strange attractors, did not find immediate analogues in critical phenomena.

While the epistemological analogy thus had at first a limited effect, it nonetheless created the conditions for major sociodisciplinary realignments, in particular in directing the statistical physicists' attention towards the study of hydrodynamic instability. Summarizing in 1975 recent advances in the understanding of the onset of turbulence stemming from this new influx of people and ideas, Martin [1976b, 57] could thus only present a mitigated balance sheet:

> While the experimental techniques that have been invaluable in understanding phase transitions promise to be very useful in the study of hydrodynamic phenomena. I suspect that the recent addition to our theoretical arsenal [i.e., renormalization] may be less effective than many had hoped.[27]

The fact was that, with Feigenbaum's work, the theoretical analogy finally showed its usefulness in a roundabout but simultaneously very profound way. Indeed, renormalization group methods and modeling practices inspired from dynamical systems theory spectacularly reinforced each other. Starting with Poincaré and even more with Andronov, dynamical systems theory had made it natural to consider classes of equations rather than single fundamental laws (such as, e.g., the Navier–Stokes equation for fluids). Instead of focusing on a specific set of equations, Ruelle and Takens—and this was perhaps their most daring assumption—had looked at NSE as a dissipative system only endowed with generic properties. This was "a point of philosophy," a hydrodynamicist later argued: "without arguing about their relevance to physics and more specifically to the study of turbulence, I ought to confess we can forget about [NSE] here" [Velarde 1981, 210]. "The general lesson" of a decade of work on deterministic theories of turbulence, Ruelle [1981, 238] added:

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

This type of reasoning, one must emphasize, which led to an undervaluation of specific dynamical equations (whether discrete or differential) in favor of the consideration of families also was at the basis of the applied topologists' approaches discussed above. One of the most vocal promoters of catastrophe theory, Zeeman [1973, 704] had tried to mobilize topological arguments in favor of the pertinence of very simple equations in modeling, arguing:

> The topologist regards polynomials as rather special, and tends to turn his nose up at so crude a criterion of simplicity. . . . So perhaps we ought to consider all possible [systems]. Now comes the truly astonishing fact: when we do consider all [systems], . . . in a certain sense it is . . . the unique example. Herein lies the punch of the deep and beautiful catastrophe theory.[28]

By exhibiting the universality of simple recurrences, Feigenbaum went a step further in rejecting fundamental laws while providing additional reasons—and, in the view of many,

[27] Let us note, however, that noticing in Kolmogorov's theory of turbulence the use of cutoff procedures conceptually akin to those common in renormalization theory, DeDominicis and Martin [1979] pursued the analogy in the case of *developed* turbulence, which is to be clearly distinguished from the onset of turbulence much studied by "chaologists" (see Section 4.3).

[28] For a discussion of this model of the heartbeat and Zeeman's reasoning behind it, see Aubin [2001].

more solid than Zeeman's—to believe that simplistic models could say something meaningful about complex systems. Indeed, while the message extracted earlier had been that simple equations could exhibit complex behaviors, Feigenbaum [1980 [1989], 51] stated that universal behaviors firmly established their utmost relevance for the modeling of natural phenomena.

> In fact, most measurable properties of *any* system . . . can be determined in a way that essentially bypasses the details of the equations governing each specific system because the theory of his behavior is universal over such details. That is, so long as a system possesses certain *qualitative* properties that enable it to undergo this route to complexity, its *quantitative* properties are determined. . . . Accordingly, it is sufficient to study the simplest system exhibiting this phenomenon to comprehend the general case.

Cao and Schweber [1993, 68] argued that renormalization theory—together with effective field theories—implies "the denial that there are fundamental laws." In a more sober manner, this reasoning was common among statistical physicists: "The phenomenon of universality makes it plain that such [molecular and/or atomic] details are largely *irrelevant* to critical point behavior. Thus we may set the tasks in hand in the context of simple model systems, with the confident expectation that the answers which emerge will have not merely qualitative but also quantitative relevance to nature's own system" [Bruce & Wallace 1989, 242; quoted in Hughes 1999, 115].

Like the Ising model, the logistic map became in this view the "representation of a representation" [Hughes 1999], that is, the object one studied in order "to explore consequences of the modelling procedure itself" [Collet & Eckmann 1980, 24]. Far from being simplistic, nonrealistic approximations, models like this became "typical" or "representative" of a universality class—representatives easily amenable to analysis, numerical computation, or actual experimentation, but whose significance could be extended to a whole class of universality. (The term "representative"—with its self-conscious political reference—is adopted from Hughes's study of the Ising model [Hughes 1999, 127]. For a discussion of "typical" models, see [Collet & Eckmann 1980]).

As far as experimentation is concerned, finally, let us now note that Feigenbaum's above quote draws attention to two aspects which will be developed: (1) contrary to purely topological approaches, renormalization methods generated quantitative predictions—*numbers* verifiable in well-designed experiments; and (2) in order to identify universality classes, which was required for extracting those numbers, one needed to study qualitatively possible "routes to complexity."

4.3. Universal Physics, or the Triumph of Light Experimentation

From the attitude typified by Feigenbaum emerged a new type of physics, concerned with typical behaviors of classes of systems. "Universal physics," as we may label it, warranted the realization of delicate experiments (*in vivo* or *in silico*) on specific systems, as well as the tackling of mundane macroscopic questions, without nonetheless eschewing any pretense of being "fundamental." Let us, by detailing an example, examine the way in which techniques available to experimental statistical physicists were adjusted to the study of hydrodynamics. As we shall see, physicists studied hydrodynamic systems, not so much as such, but rather as a way of probing the universal, typical, and generic characters of dynamical systems.

In the early 1970s, Jean-Pierre Boon, an experimenter collaborating with Prigogine's Brussels school, was using light-scattering methods for the study of critical behaviors.

The expansion witnessed by this new experimental technique was allowed by both the renormalization breakthrough and the availability of new technologies. At a 1973 Brussels conference Boon characterized the analogy between phase transitions and fluid instability as "phenomenological;" it was a purely formal "translation:"

> at the level of microscopic analysis there is presently no evidence for an actual analogy between hydrodynamic or thermal instabilities and phase transitions for the very good reason that the microscopic mechanism governing the evolution of a system towards an instability point remains at present a totally open question [Boon 1975, 89].

Visiting the Bell Laboratories in 1970, Boon met staff member Günter Ahlers, whose Rayleigh–Bénard experiments would inspire key players such as Martin and Libchaber to take up the study of the onset of turbulence, but at the time, focused on critical phenomena arising at the superfluid helium transition. Insisting on the "serendipitous aspects of Jean-Pierre's visit," he recalled that his apparatus was at the time "ready and cold," enabling him to perform manipulations following Boon's suggestions. Within "a day or two" he was "able to obtain heat-transport data which were a great deal more precise than previous results in the literature" [Ahlers 1995, 94].

This, however, was no coincidence. The material conditions for experimental work were by then being revolutionized by the introduction of new technologies in the laboratory. Physicists who had been studying critical phenomena had at their disposal powerful tools which they brought to bear on fluid mechanics. As he recalled,

> although the conventional tools of solid-state physics, such as high-resolution thermometry, lock-in amplifiers, light scattering, and others played an important role, I believe that the most important *experimental development* of the 1970's was the advent of the *computer* [Ahlers 1995, 96].

To think of the computer as an experimental development may be surprising, but it was more than a new machine coming into the laboratory. Data acquisition systems and numerical fast-Fourier transforms "revolutionized the kind of project that could be tackled; . . . they also gave us *a completely new perspective on what types of experiments to do*" [Ahlers 1995, 96].

In the early 1970s, Ahlers's results were simultaneously highly exciting in the opinion of some hydrodynamicists and hardly worth a publication for their author. Koschmieder [1974] and Velarde [1977, 521] both lauded Ahlers' "impressive and most complete work." At the time, Ahlers presented his results at the annual meeting of the American Physical Society and only published a summary; they were developed in a later article [Ahlers 1974]. When their value started to be recognized, however, several other teams of physicists turned their attention (and light-scattering instruments) toward transitions to instability in fluids. Among these, one may mention Gollub and Swinney, who used light-scattering methods to find observations "perhaps consistent with proposals of Ruelle and Takens" [Gollub & Swinney 1975, 927], and Bergé and Dubois [1976], who used laser technologies to achieve one of the first precise, nonintrusive measurements of local velocity fields in fluids. The latter two also actively collaborated with Pomeau and Manneville in exhibiting experimentally the intermittent scenario, first observed in the course of numerical simulation on the Lorenz system by Pomeau and Manneville [1979; 1980] and later in fluid experiments [Bergé *et al.* 1980]. (This scenario is further discussed in relation to the dynamical systems approach in Aubin [2001] and in Franceschelli [2001]).

Finally, Libchaber's delicate experimental work, which dramatically confirmed Feigenbaum's intuition, was also inspired by his background in the study of the superfluid helium transitions. His was a classic RB experiment—very similar to that of Ahlers, with whom Libchaber was in frequent contact at the Bell Labs and who had aroused his interest in such problems—especially remarkable, however, for the great precision allowed by local probes, that is, resistors sensitive to heat. Carefully noting a "demultiplication of frequencies" Libchaber and his collaborator Jean Maurer concluded that this process "appeared as crucial for the germination of turbulence." After extensive collaborations with many theoreticians, including Feigenbaum, the experiment was interpreted as the first observation of the period-doubling cascade of bifurcations, providing a value for δ that was "quite bad," but not in contradiction to Feigenbaum's estimate: $\delta = 3.5 \pm 1.5$ [Libchaber & Maurer 1981; 1982].

At a time when hierarchies in physics subdisciplines were in flux, when a certain disenchantment with reductionist approaches started to be felt, when big questions concerning the infinitely small and the infinitely large were, relatively speaking, losing prominence, this new type of "universal physics" felt like fresh air. A representative of this current in physics, Philip Anderson, thus 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" [Anderson 1972, 393]. For several physicists and mathematicians this result came as a revelation: it was "a kind of miracle, not like the usual connection between theory and experiment." For another, it was "the best thing that can happen to a scientist, realizing that something that's happened in his or her mind exactly corresponds to something that happens in nature" (Gollub and Kadanoff, quoted in Gleick [1987, 209 and 189]). On a more pragmatic level, Doyne Farmer noted: "The idea of universality was not just a great result. Mitchell [Feigenbaum]'s thing was also a technique that employed a whole army of unemployed critical phenomena people" [Gleick 1987, 268–269]. In terms of material, cost, and personnel, the experiments were relatively affordable and easy to perform. For Bergé, this was "the triumph of 'light physics'" [Bergé, Pomeau & Vidal 1984, 267]. All this increased the prestige of both statistical physical and hydrodynamics and reinforced the impression of epistemological rupture that we have talked about.

4.4. Scenarios: Dynamical Systems Theory and the Language of Chaos

In several talks delivered in the summer of 1975, Martin offered a first synthesis of the works of Landau-Hopf, Ruelle-Takens, May, and Lorenz applied to fluid mechanics. Already, then, it was made clear that the focus of these studies—still informed by the phase transition analogy rather than dynamical systems theory—was not so much the "nature" of turbulence, as Ruelle and Takens had claimed, but rather the transition from stationary flows to disordered states. The qualitative identification of the various "roads to chaos" became an urgent task at hand.

To shift the spotlight, from turbulence to the *onset* of turbulence, was extraordinarily efficient in narrowing the gap between widely diversified communities. First, this was one of the classic problems of hydrodynamics, reaching as far back as Osborne Reynolds and Lord Rayleigh, who at the end of the 19th century investigated the stability conditions of slightly perturbed stationary or periodic flows. A huge literature focused on two specific problems: Rayleigh–Bénard and Taylor–Couette.[29] Second, the approach in terms of "roads to chaos" could naturally be accommodated by the theory of bifurcations then thriving on both the topological basis pursued by mostly pure-mathematics-minded dynamical systems specialists and the analytic approach in terms of functional spaces favored by the applied mathematics tradition (a foremost representative of which was the work done at New York University's Courant Institute). Finally, as is obvious from the above, this emphasis on transitions allowed tapping into the rich theoretical arsenal and manpower of critical phenomena physics. As a result, the rapprochement of several communities was tremendously facilitated: hydrodynamic stability theorists, statistical physicists, and dynamical systems theorists, as well as more traditional applied mathematicians and computer simulation specialists.

The culmination of this viewpoint came with an article of Jean-Pierre Eckmann's from 1981, describing the various "scenarios" forming so many "roads to chaos." Having served as crucial personal link between Libchaber and Feigenbaum, Eckmann had been in frequent contact with Thom and Ruelle in the "stimulating atmosphere at Bures-sur-Yvette" [Collet & Eckmann 1980, vii]. His paper presented the general philosophy and theory behind this new "approach to the understanding of irregular (or nearly irregular) phenomena, which has been relatively successful recently," adding in a footnote that it could "be viewed as a concretization of Thom's catastrophe theory" [Eckmann 1981, 643]. Although it hardly contain anything new, after more than a decade of work on instabilities in fluids, this was one of the first syntheses to come out, which was explicitly based on dynamical-systems modeling practices:

Since the global classification of dynamical systems was far from being achieved, Eckmann turned to *experiments* as a guide for which bifurcations from simple attractors to nontrivial ones might be the most relevant for physics (and chemistry). He introduced the notion of a *scenario* to describe the most probable sequences of bifurcations. Three roads to turbulence deserved the label: the Ruelle–Takens scheme, Feigenbaum's cascade, and the Pomeau–Manneville intermittent behavior. Nothing insured that this list was exhaustive. Acknowledging that "this may be a somewhat unfamiliar way of reasoning," Eckmann [1981, 646] specified "the nature of the prediction which can be made with the help of scenarios." He characterized scenarios as "if . . . , then . . . " statements, "i.e., if certain things happen as the parameter is varied, then certain other things are likely to happen as the parameter is varied further." Linked with genericity, the definition of *likely* "in a physical context" was tricky.

> I do not intend to go to any philosophical depth but, rather, take a pragmatic stand. (1) One never knows exactly which equation . . . is relevant for the description of the system. (2) When an experiment is repeated, the equation may have slightly changed. . . . (3) The equation under investigation is one among several, all of which are very close to each other [Eckmann 1981, 646].

One therefore was faced, he concluded in another publication, "with the problem of isolating and if possible answering new types of questions which are more or less independent of detailed knowledge of the dynamics of any given physical system. Such questions have answers which are universal" [Collet, Eckmann & Koch 1981, 1].

[29] The Taylor–Couette problem consists in the study of the stability of a fluid flow contained between two rotating concentric cylinders. About the early history of this problem, see Donnelly [1991].

Out of a mathematical failure, a fruitful modeling practice was thus constructed by physicists for physicists. Although mathematicians had not succeeded in classifying generic bifurcations, Eckmann offered a scheme emphasizing the benefits of a dynamical systems approach coupled with experimental results and renormalization theory. This scheme exploited the robustness of experimental results rather than a general theory of hyperbolic systems to point to which bifurcations were the likeliest to occur. And it emphasized the universality of the results to argue in favor of using simple models to understands very complicated situations. "[W]e need an adequate *language* for describing deterministic evolution equations," Eckmann [1981, 643] wrote. This language would be that of dynamical systems theory.

Statistical physicists therefore adopted the tools and concepts of dynamical systems and adapted them for their own purpose. Characteristically, when asked why he devoted so much work to a particular model system—the implication being that it was "not so real as to be of practical interest, and perhaps not so deep as to have real intellectual interest"—a key player in the development of both renormalization methods and chaos theory, recalled to have thought: "I know that his implied criticism is right. So I resolve to learn something new. The new subject I find is dynamical systems theory" [Kadanoff 1993, 386]. With its unique capacity for dealing simultaneously with classes of systems, dynamical systems theory was a natural framework for the type of questions being raised by universality. Even for physicists, this would become the official language of chaos.

CONCLUDING REMARKS: THE CULTURAL IMPACT OF CHAOS

At the close of our study, we are facing the question with which we opened it: how to grasp the essence of the chaos phenomenon and what was its impact? Here, we are stepping onto shakier ground that has already given rise to the greatest exaggerations. In the following, we will suggest a few clues that may lead to future research and some definite conclusions. Compared to other moments in history, this one is clearly so close to us that the familiar process of continual historical rewriting has hardly had time to take place. The slow crystallization process that will ultimately attribute actors and pieces of work their definite place in history (only eventually subject to minor modifications) has not been completed. If every historical writing necessarily involves an *interpretation* of history, it is clearer if we provide the keys to our own interpretation. At every step in our narrative, we have taken great lengths to explain, granted that exhaustiveness remained out of reach, some of the most important choices we needed to make. We have tried in particular to justify the periodization corresponding to our theses or the privileged role attributed to some actors (Smale mainly, but also Lorenz or Ruelle).[30]

If temporal distantiation is no option, the proximity of recent history also is an opportunity, at least for the historian attentive to the interpretations and interests promoted by various "schools." The closer one stands from a historical moment the easier it is to see the variegated texture of the scene. From this close look, stems a dual tension, which we have already

[30] Unquestionably, there are other scientists whose work might have been more carefully examined here, such as Mandelbrot and fractals, or Prigogine and dissipative structures, to cite two coming to mind. We have chosen not to present, within the bound of this paper, a detailed analysis of their networks and influences—which remains to be done.

underscored in our introduction:

(1) the tension between long temporalities (the *longue-durée* history of mathematical theories or other branches of science such as fluid mechanics) and short ones, periods of acceleration and upheaval where intellectual and social configurations shift;

(2) the tension between the local point of view (what happens in a single location or a single discipline) and the global one, involving large-scale processes concerning many branches of science and its relationship with the rest of society.

As our chronological development unfolded, both of these tensions—in no way independent from one another—have been increasing and exacerbated. To confront these tensions is one of the most stimulating challenges of the historiography of recent science. (On the issues around historiography of contemporary science, see Söderqvist [1997]).

"Chaos" was a phenomenon that we have characterized as both intellectual and disciplinary reconfiguration and socio-professional convergence. Developed from the mid-1970s to the mid-1980s, it reached its high point a decade later, at the beginning of the 1990s. Our extensive study, we believe, has amply established that point. Now, whether—or not—it is called a "scientific revolution" complete with "paradigm shift," whether—or not—it is, on the contrary, considered as a mere fashion now on the wane, no one will argue with the fact that it was a large-scale thing. Several indicators, often mentioned in the above, support this claim:

—at the disciplinary and professional level, the massive character of chaos was manifest in the large number of disciplines involved, of publications, of conferences, etc.;

—at the epistemological and conceptual level, actors themselves often perceived chaos as a true philosophical revolution bringing forth new epistemic cleavages;

—at a social and cultural level, chaos was a phenomenon reaching beyond scientific milieus and taken over by philosophers, science popularizers, and even littérateurs.

From a renewal of Aristotelian philosophy ([Thom [1988] and his disciple Boutot [1993b]) to the birth of postmodern science [Hayles 1990], nay a global revolution in the history of humanity—"a major transformation from patriarchy/order to partnership/chaos" [Abraham 1994, 220]—the claims concerning chaos seemed to know no limits. From high-browed theater [Stoppard 1993] to the bestseller and blockbuster *Jurassic Park* [Crichton 1990], chaos theory *really* appeared ubiquitous in the early 1990s. In scientific communities, the vogue for chaos went hand in hand with significant shifts in the material, socioinstitutional, and cultural conditions of their enterprise. Chaos partook in a few general underlying processes: it was both influenced by and influential in shaping them. To speak of the "context" of chaos would therefore be wrong-headed. But a few elements emerge as having interacted in crucial ways with chaos. Let us, in the following, review them briefly.

For a start, the material conditions of the mathematical sciences were metamorphosed by the emergence and, above all, the wide diffusion of electronic calculating machines. Let us also mention the important role, already noted above, played by imaging techniques and lasers (in particular in phase transitions and turbulence experiments). The computer has without the hint of a doubt played a crucial role in the multidisciplinary convergence at the basis of chaos. There are, at the moment, still surprisingly few serious studies of the impact of the computer on mathematical and, in general, theoretical research. (For a first dent in this problem, see, e.g., Farge [1988] and Hénon [1983]). On a practical level, this tool provided theoreticians with original means of apprehending, and indeed manipulating, conceptual entities: equations could be solved with any degree of precision required; solutions, surfaces, and fractal sets could plotted on screens and paper; and, as we have seen, even in the laboratories, new experiments could be envisioned while others now became dispensable. Moreover, the diffusion of results, such as Lorenz's system, May's study of the logistic equation, Li and Yorke's "Period Three Implies Chaos," or Feigenbaum's universality, was mediated by calculators and analog computers and then digital computers. Finally, besides being highly suggestive, computer-generated pictures—of attractors (Lorenz's or Hénon's) or fractal sets systematically associated with chaos—eased the circulation of results by allowing their significance to be grasped *without* having to master fully some of the most abstract mathematical concepts of dynamical systems theory. In a word, the day-to-day practice of scientists was revolutionized, as simulations and numerical explorations became the predominant basic activity for theoretical work thereby blurring the experiment/theory and real/virtual categories. It is also obvious, one must acknowledge, that none of this was limited to chaos or nonlinear phenomena. As an illustration, let us mention the special issue of the *Cahiers de Science et Vie* (October 1999) "Comment l'ordinateur transforme les sciences," where a spectrum of examples, from the H-Bomb to biology, *via* astrophysics, medicine, or natural history, show that the computer transforms not only the practice of science, but also the very conception of the objects of science.

Second, the success of chaos is linked to the emergence of interrelated ideological discourses and socioprofessional realities. Amongst mathematicians, the crises of 1968 and the Vietnam War has given rise to a fundamental reevaluation of the nature of their profession, whose dominant cultural images were turned upside down. For the Bourbakist hegemony emphasizing the structural and axiomatic conception of pure mathematics disconnected from applications and the needs of society at large—in short, mathematics "for the honor of the human spirit" [Dieudonné 1987]—was progressively substituted a new conception of mathematics more self-conscious of its social role.[31] Among physicists, as was mentioned above (Section 4.3), one would find a similar evolution. These shifts in representations were no doubt part of larger mutations. The problem of the relationship between micro- and macrolevels, with the emergence of the sciences of the mesoscales, was reactualized at the same time as reductionist attempts seemed to falter (see below). Of course, such discourses precisely because they have an ideological component are especially slippery. But one need not agree with either the diagnosis they offer or the very pertinence of such global ideological discourses, in order to take their existence into account. As we have seen, Smale's involvement into the political arena was far from independent of his concern for directing his work "towards socially positive goals." To argue cogently for such cultural connections is, however, rather tricky and would require further studies. Similarly, Thom's and Ruelle's IHÉS, whose structure and spirit left a mark on research conducted within its walls, was considerably shoken by Alexander Grothendieck's political engagement about the "moral responsibility of the scientist" [see Aubin 1998a]. To draw links between intellectual climates and the content of science is often seen as highly problematic, as the debates that followed Forman's [1971] attempt at linking Weimar culture and acausality in quantum mechanics has abundantly demonstrated.

[31] On the Bourbakist hegemony over pure mathematics, see Aubin [1997]. The conflict of the two antagonistic images of mathematics is developed in Dahan Dalmedico [1996a; 1999a; 2001b].

Changes in disciplinary images corresponded both to desires expressed by scientists themselves and social demands vis-à-vis science. A further social phenomenon played a fundamental role in the emergence and success of chaos—that is, the high value put into interdisciplinary work. Just to take an example, one is struck by the fact that in the early 1970s de Gennes's program was carried out with considerable aid from the French state (the CNRS) intent on promoting interdisciplinary research on a few selected themes in "small," rather than "big," science. A true ideology of interdisciplinarity permeated this program that led to de Gennes's very deliberate move to have physicists plunge into fluid mechanics. (De Gennes's attempts are detailed in Aubin [1998a, Chap. 8]). Like all social phenomena, interdisciplinarity—which has up till now remained prominent—must be studied analytically: a history of its emergence and diffusion is called for; a study of its sociological underpinning is needed; an analysis of its ideological freight would be welcome. Chaos could only emerge against this backdrop: conferences, collaborations, and research and teaching programs generated a context favorable to interdisciplinary research. Chaos in turn played a role in—and symbolically reinforced—the generalization of this preexisting trend. It was at this time, for example, that contacts between mathematics and physics, which had hitherto been allowed to get looser, were rewoven in durable manner. The deepening economic crisis and important political changes of the 1980s made banal the need for science to justify its usefulness. In the United States, for example, the alliance conditions established after WWII between physicists, the military, and the government were disturbed by the fall of the Berlin wall and the Soviet breakup. More generally, interdisciplinarity appeared then—and appears even more today—as a cultural value that researchers needed to cultivate and that permeated science policy discourses. More and more, it seemed that representations of science and science policy discourses became intertwined. (For a recent example of policy-makers' prescriptions emphasizing interdisciplinarity and presented as a description of contemporary science, see Gibbons *et al.* [1994] and Nowotny *et al.* [2001]).

Finally, at a more diffuse cultural, philosophical level, chaos was accompanied by a discourse relative to deep epistemic ruptures and the emergence of a new paradigm. It was turned into a symbol of a new way of conceiving and practicing science. Once again, the question is delicate since we must address both the level of scientific theories and practices and that of *representations*, more or less diffuse in wide scientific circles and the public. This new image was for that matter produced together with a negative—and often caricatural—image of classical science according to which the main concerns of the latter were to exhibit order in nature and to reclaim the transparent intelligibility of a world entirely ruled by determinism.

Still, we feel one can now positively assert: chaos has definitely blurred a number of old epistemological boundaries and conceptual oppositions hitherto seemingly irreducible such as order/disorder, random/nonrandom, simple/complex, local/global, stable/unstable, and microscopic/macroscopic. This epistemic blurring is illustrated by the dialectics at play in some of the most famous statements of the domain. Sinai's [1992] "aléatoire du non-aléatoire" is the most obvious example, but one can also think of Lorenz's [1963] "Deterministic Nonperiodic Flow," of Thom's [1975] "science des modèles" and "modèle de la science," as well as Smale's [1966] "Structurally Stable Systems Are Not Dense," and of course of the *Ordre dans le chaos* of Bergé *et al.* [1984]. Since most of these terms only acquire a precise meaning within a mathematical framework, a complete demonstration of this claim would lead us astray. (See Dahan Dalmedico [2000 and forthcoming]). Let us simply note here that the "determinism quarrel" which vividly opposed Thom and Prigogine in the early 1980s has been quelled by establishing a new boundary between science and metaphysics under which the question of the ontological determinism of nature now seems to be clearly subsumed.[32] The epistemological blurring coextensive with chaos partook in the emergence of a new *cultural image* of science, even if it surely was not the only element to do so. Other intellectual factors, such as the epistemic dead end widely seen in reductionism, and several social, political, and technological factors that gave rise to this new image of science cannot even cursorily be examined here.

Just as an example, let us underscore the essential role played by the media, not only in the diffusion, but also in the very constitution of chaos, viewed as a social phenomenon. The emergence of this cultural space across disciplinary boundaries went hand in hand with a concern for pedagogy. Because of the diversity of audiences to which they were addressed, theoretical and experimental results were constantly explicated, reinterpreted, simplified, essentialized—in a simple but tricky word, *popularized*. As a result, a "standard" mode of exposition crystallized (to be found in introductory chapters of conference proceedings, in popularization articles whether intended for certain sociodisciplinary groups or lay audiences, in the first treatises [Guckenheimer & Holmes 1983; [Bergé *et al.* 1984; Schuster 1984]). Some stories—Lorenz stumbling on sensitive dependence on initial conditions, or the slowness of Feigenbaum's old HP-65 prompting his second-guessing the next bifurcation—acquired a quasi-mythical character. For an instructional or illustrative purpose, a few exemplars were constantly rehashed: the Hopf bifurcation, Smale's horseshoe, the period-doubling cascade of bifurcation, and of course Lorenz's butterfly. Key concepts of dynamical systems theory (bifurcation, attractors, genericity, etc.) were widely disseminated and perforce made rather simple. Striking to the actors, the simplicity of chaotic ideas therefore was in part the result of their own pedagogic concerns. As Ruelle [1992, xiv] stated, "The popular success of chaos is due to several factors: the simplicity and power of the ideas involved, the striking terminology (strange attractors, chaos), and of course Gleick's bestseller."

Moreover, the domain took a prominent part in the striking emergence of a new type of scientific book: the *theoretical manifesto* which deliberately mingled science popularization, exposés of new theories, and wild, if inspiring, philosophical speculations. As examples, one may cite Thom [1972, 1974] published in pocket paperback format, soon to be followed by Mandelbrot [1975] and Prigogine & Stengers [1979].[33] To these, one may add countless widely disseminated, programmatic essays written by the most prominent actors of the field, as well as popularization articles and books that have gathered such a large audience that they have had an influence on the representation of the field (esp. Gleick [1987]). All these aspects would require a subtle analysis that we cannot develop here, but one may

[32] Pomian [1990] has collected some of the most important contributions to the debate including the initial incendiary article by Thom. For an analysis of the debate, see Dahan Dalmedico [1992].

[33] In the French intellectual arena, this reinscription of scientific concerns in the wider culture could be attributed to the editorial success of the Nobel-prize winners François Jacob's [1970] and Jacques Monod's [1970] popular books, as well as the mutation of science itself, "the rather confused feeling that a new *problématique* is emerging" [Pomian 1989, 471]. Felt in the public appeal of Edgar Morin's [1973, 1977], Michel Serres's [1977, 1980, 1982], and Henri Atlan's [1979] writings, the feeling was that science, taken as a whole, has entered a new paradigm, the reference to T. S. Kuhn [1970] (translated in French in 1972) being not only explicit but emphasized.

wonder whether they are specific to chaos or rather characteristic of a new insertion mode of scientific knowledge in contemporary societies.

In conclusion, we can safely make the following two apparently paradoxical claims. (1) As a unified site of social convergence, the "science of chaos" does not exist any more and new "interdisciplines" such as complexity theory are staking claims on its social position. (2) As an outcome of conceptual reconfigurations, chaos brought forth changes that are irreversible; it has contributed significantly to the emergence a new epistemological framework; several of the ideas and results it has introduced have become commonplace. In short, chaos was a transition that was both ephemeral and irreversible.

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