CHAPTER V: STABILITY

A vector field X on M^2 is said to be structurally stable if there is a neighborhood Δ of X in $\mathcal{B}$ such that whenever $Y \in \Delta$ there is a homeomorphism of M^2 onto itself transforming trajectories of X into trajectories of Y.
—Mauricio M. Peixoto.[1]

L'hypothèse de stabilité structurelle des processus scientifiques isolés apparaît comme un postulat implicite de toute observation scientifique.
—René Thom.[2]

A system which completely lacks stability would be a poor model for reality, as reality is *always* a perturbation of what we think it is. Thus some kind of stability is crucial.
—Robert F. Williams.[3]

1. INTRODUCTION: A HISTORY OF STRUCTURAL STABILITY

While director Léon Motchane desperately struggled to achieve financial stability for the Institut des hautes études scientifiques, René Thom, as soon as he joined its faculty, launched an ambitious program in the hope of discovering what was at the basis of the world's stability. The mathematical notion he judged could best be used for this task was *structural stability.* Thom's efforts at building around him a research school will be

[1] M. M. Peixoto, "Structural Stability on Two-Dimensional Manifolds," *Topology,* 1 (1961): 101-120, 103. First defined by A. A. Andronov and L. Pontrjagin in 1937.
[2] "The hypothesis of structural stability of isolated scientific processes is implicit in all scientific observation." R. Thom, *SSM,* 16.
[3] R. F. Williams, Review of *Dynamical Systems on Surfaces,* by C. Godbillon, *American Mathematical Monthly,* 92 (1985): 70-71; quoted by M. W. Hirsch, "The Dynamical Systems Approach to Differential Equations," *Bulletin of the American Mathematical Society,* 11 (1984): 1-64, 33.

described in chapter VI. Meanwhile, I present here a partial history of the concept of stability in the study of differential equations, which is essential background for an understanding of Thom's and, later, Ruelle's work.

The concept of structural stability was introduced in 1937 by Russian mathematicians Andronov and Pontrjagin, and taken up in the United States by Lefschetz and his younger collaborators after World War II. From the very beginning, as I describe below, a long time before Thom invested it with so much philosophical weight, the notion of structural stability had been perceived as embodying a general codification of the practice of building mathematical models of natural phenomena. Its mathematical formulation was seen as condensing general metaphysical assumptions.

As in almost every piece of historical writing on chaos, I include a chapter titled "Stability." However, my point is *not* that people were once blindly looking only for stability and overlooked chaos because of preconceived dogmas, as is often argued. This explanation is much too simple-minded and fails to explain the reasons why scientists emphasized periodic solutions in their investigation of nonlinear differential equations. More importantly, it totally misses the most important point: the very emphasis on stability, and structural stability in particular, *actually prepared the emergence of chaos in that it set up the line of questioning to which features of chaos appeared as long-sought, if somewhat surprising, answers.*

Therefore, the exploration of the stable descriptions used to model natural phenomena was a major impetus behind the modeling practices I study here. To start with, structural stability has always been conceived as a mathematical translation of philosophical assumptions about the physical stability of real systems. As we have seen, Thom's program for catastrophe theory was greatly inspired by structural stability. Similarly, American topologist Stephen Smale's whole career in the field of dynamical systems was an offshoot of his concern for finding the right kind of stability, so that "most" dynamical systems be stable under this definition.

Initially, Thom and Smale hoped that mathematical arguments could substitute for philosophical ones in the choice of systems susceptible of being used for modeling.[4] Around 1970, largely because of work they themselves had done or inspired, it became clear that mathematics could not in general guide the choice of which stable systems to use for modeling. Both Smale and Thom therefore stopped working directly on the mathematical theory of dynamical systems, and rather focused on modeling issues. They developed a new field that one might be tempted to call "applied topology" which I will address in more detail in chapter VI below.

Dealing with the conceptual history of (structural) stability, this chapter does not aim at providing original accounts of the social and cultural contexts within which each of the contributions discussed was made. Rather, noticing that both Thom and Smale made important use of work done decades earlier, I intend to provide my own re-reading of their sources as seen through their eyes. Still, I shall inject some elements of contextual analysis when necessary. But one should keep in mind that, only after Smale had

[4] That mathematics can bypass philosophical impasses was a common theme of the 1950s; it was at play in the story of structuralism; it also figured prominently in other contexts; see, e.g. D. Lerner, "Introduction, On Quantity and Quality," in *Quantity and Quality,* ed. D. Lerner (New York: Free Press of Glencoe, 1961), 11-34, esp. 20-23.

achieved his own synthesis, could the various contributions I discuss be reinterpreted as belonging to a unified, conceptual and disciplinary setting.

In section 5 below, I discuss Smale's synthesis and the bridges he built to previous works. This section should therefore be read differently from the previous ones as it deals with the work of one of the main actors of my story. It is only from Smale's standpoint indeed that the works discussed in the first sections of this chapter become 'precursors' of Smale's own accomplishment. Finally, this chapter provides an answer to the main conundrum of the young historiography of chaos, namely the fact that chaotic behaviors seem to have been overlooked for so long. I argue that the development of the computer and of topological methods for the study of differential equations provide causes for the chaos burst that began around 1975.

2. MATHEMATICAL LAG EXPLAINS SPUTNIK, OR THE COLD WAR ROOTS OF CHAOS THEORY ?

In 1959, Princeton University topologist Solomon Lefschetz (1884-1972) presented his Final Report to the Office of Naval Research (ONR), which, for more than 13 years, had sponsored a "Project on Nonlinear Differential Equations and Nonlinear Oscillations." In his final remark, he listed the most significant mathematical contributions made by members of the project. In first place, came the work of Henry DeBaggis and of Marilia and Mauricio Peixoto on structural stability.[5] Indeed, while always a marginal concern for

[5] S. Lefschetz, "Nonlinear Differential Equations and Nonlinear Oscillations." Final Report (August 15, 1946 - September 30, 1959), Contract NONR-1858(04), Project NR043-942, p. 30. Fine Arch. For a more complete history of Lefschetz's group and some biographical information, see A. Dahan Dalmedico, "La renaissance des systèmes dynamiques aux États-Unis après la deuxième guerre mondiale: l'action de Solomon

the Project as a whole, the mathematical concept of structural stability nonetheless constantly remained on the mind of some of its members. And the story of its "ascension," as Amy Dahan Dalmedico put it, parallels the history of Lefschetz's Project itself.

> During most of World War II, the undersigned [Solomon Lefschetz], a consultant at the David Taylor Model Basin [of the US Navy], had frequent interviews with Dr. Nicholas Minorsky, in connection with the latter's production of his well-known *Introduction to Nonlinear Mechanics.* Dr. Minorsky voiced repeated regrets at the impossibility of creating in this country anything resembling the well known Institute of Oscillations in Moscow.[6]

Of course, Lefschetz recalled, a full-fledged Institute would have required immense resources. Lefschetz and Minorsky approached ONR with a more modest proposal to initiate a Project on Differential Equations, total cost to be $25,000.00 (for the first year).

This Project, as well as Minorsky's initial reports to the US Navy, explicitly emphasized the Soviet advance in the study of nonlinear differential equations which, with the notable exception of George David Birkhoff, had to a large extent been neglected by American scientists. "Many hold the opinion," Lefschetz thus wrote in 1946, "that the classical contributions of Poincaré, Liapounoff and Birkhoff have exhausted the

---

Lefschetz," *Rendiconti dei circolo matematico di Palermo,* ser. II, Supplemento, 34 (1994): 133-166.

[6] S. Lefschetz, "Nonlinear Differential Equations," 1. Nicholas Minorsky wrote an extensive, four-part report to the David W. Taylor Model Basin of the US Navy. Titled *Introduction to Nonlinear Mechanics,* reports #534, 546, 558, and 564 were published from December 1944 to September 1946. They can be found at Princeton, call number SK 8230.6445. Minorsky later published a book version: *Nonlinear Oscillations* (Princeton: Van Nostrand, 1962). In both the reports and the book, however, Minorsky did not emphasize structural stability.

possibilities. This is certainly not the opinion of a large school of Soviet physico mathematicians." [7]

The objectives of Lefschetz's proposal "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."[8] Educated as an engineer in France, Lefschetz possessed a sensitivity for applied problems. But above all cold war competition against the Soviet Union remained one of Lefschetz's main stated drive for his important implication in the Project. Its high level of abstraction notwithstanding, none of the technological and military consequences of the mathematical work on nonlinear dynamics were lost on him. In 1950, while looking for other sources of support, Lefschetz wrote: "I have become interested in . . . the applications of the methods of non-linear mechanics to Air Force problems in guidance and automatic controls."[9]

"Curiously enough," Lefschetz candidly acknowledged in his final report, while the goal had been to train mathematicians that could apply their skills to industrial and military technological problems, "nearly all its [younger] members remained in the academic world."[10] It therefore seems that most of the members' motivation, including

---

[7] S. Lefschetz, *Lectures on Differential Equations* (Princeton: Princeton University Press, 1946), iii. For a history of Soviet research on nonlinear dynamics, see S. Diner, "Les voies du chaos déterministe dans l'école russe," *Chaos et déterminisme,* ed. A. Dahan Dalmedico et al. (Paris: Seuil, 1992): 331-370.

[8] S. Lefschetz, "Nonlinear Differential Equations," 2.

[9] S. Lefschetz to Colonel Frank J. Seiler (Oct. 19, 1950). Princeton Arch.

[10] S. Lefschetz, "Nonlinear Differential Equations," 22.

Lefschetz's, was above all academic, but they never failed, in good faith probably, to mobilize defense arguments in favor of their Project.[11]

On October 16, 1959, Solomon Lefschetz was awarded a honorary degree by the Sorbonne, in Paris, in the presence of President Charles de Gaulle. The retired mathematician took advantage of this occasion to attract people's attention to the "mathematical gap" he saw between Russia and the West. Until recently, the American lag in the study of nonlinear oscillations seemed a sorry thing, which to be sure needed to be redressed. After 1957, it became an urgent matter of national security. "Then the first Sputnik came out," Lefschetz declared in Paris. "That's when I got scared." Princeton University's press release 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.*" For Lefschetz, the Soviet success in guiding space rockets meant that they were even more ahead in nonlinear dynamics than he previously thought; he estimated their lead to be of "10 to 15 years." This reason, he claimed, persuaded him to leave his retirement to set up the Research Institute in Advanced Study (RIAS) in Baltimore, at the Martin Company, an aviation and missile manufacturer.[12] Of course, this only reflected

[11] It should be noted that during the McCarthy era, Russian-born Lefschetz hardly was above being considered a suspect character by some. In 1955, one his students, John G. Kemeny, came under scrutiny for "close association with individuals alleged to be sympathetic to Communism and/or members of Communist front organizations, namely . . Solomon Lefschetz." J. Douglas Brown and Albert W. Tucker's affidavit deposition (March 2, 1955). Princeton Arch.

[12] Lefschetz's file. Princeton Arch. My emphasis. On February 7, 1960, the *New York Times* published an article about Lefschetz's worries titled "Mathematical Lag in Missiles Noted." RIAS moved to Providence in 1963; see A. Dahan Dalmedico, "La renaissance," 141.

Lefschetz's personal worries. Nothing inherent to the study of nonlinear differential equations led to such an emphasis on military and space technology. As we shall soon see, in a different time and place, namely Paris and Berkeley in the late 1960s, hopes were placed in the study of similar mathematical domains for quite the opposite reason.

Even before the start of the Cold War, as Lefschetz recalled, his proposal was quickly accepted by ONR.

> The undersigned will never forget his (only) interview with Captain Conrad [the officer in charge of setting ONR up]. At first nonplussed and puzzled, he soon called in consultation his scientific adviser, Dr. Allen Waterman . . . [who] read our short memo and exclaimed at once - 'This is just what we want'. Whereupon the matter of the project was settled in a short quarter of an hour...[13]

The activities of the Project proceeded in various ways. There was a weekly research seminar initiated by Lefschetz in 1942, an advanced course on Differential Equations, a constant flow of invited professors, postdoctoral fellows and graduate students. From 1945 to 1953, Lefschetz chaired the Mathematics Department. True to his repute of "papa daddy" for graduate students, he remained at the disposal of the Project's participants. "Suffices to say," he stated, "that the Director's office was never locked, and that he was (and has remained) infinitely accessible to one and all."[14] The Project also oversaw the publication of articles written by its members in a sub-series of the famous Annals of Mathematics Studies.[15] One of Lefschetz's foremost students, Joseph LaSalle, summarized his mentor's accomplishments as such:

---

[13] S. Lefschetz, "Nonlinear Differential Equations," 2.
[14] S. Lefschetz, "Nonlinear Differential Equations," 4. It was George W. Brown who called Lefschetz a "papa daddy" in *The Princeton Mathematical Community in the 1930s: An Oral History Project,* pp. PMC3-4. Princeton Arch. AC#109 Box 40.
[15] *Contributions to the Theory of Nonlinear Oscillations, Annals of Mathematics Studies,* 20 (1950); 29 (1952); 36 (1956); 41 (1958), ed. Solomon Lefschetz (Princeton: Princeton

> It was Solomon Lefschetz who made the subject of differential equations both respectable and lively in this country, and who through his projects at Princeton and RIAS . . . made it possible with his boundless enthusiasm, inspiration, and guidance for many young people to establish deep roots in the subject.[16]

3. FACETS OF STABILITY IN THE INTERWAR: RADIO ENGINEERING, COARSE SYSTEMS, CELESTIAL MECHANICS

Most important for my purpose here was "a noteworthy adventure engaged in by the Project, [namely] the edited translation from the Russian of a classic: Andronov and Chajkin, *Theory of Oscillations.*"[17] In 1931, Aleksandr Aleksandrovich Andronov (1901-1952), a student of L. I. Mandelstam, had founded a research school at Gorki.[18] The above book summarized a decade of work on nonlinear oscillations. Making a wide usage of Poincaré's, Lyapunov's, and Birkhoff's works, this book mainly addressed *dissipative* systems, as opposed to *conservative* ones privileged by previous mathematicians.[19]

University Press). A fifth volume (*Annals,* 45) was published in 1960 and edited by L. Cesari, J. LaSalle, and S. Lefschetz.

[16] J. P. LaSalle, *IEEE Memorial,* 1973; quoted in *Dynamical Systems: International Symposium on Dynamical Systems, Brown University, 1974,* ed. L. Cesari, J. K. Hale, and J. P. LaSalle (New York: Academic, 1976), iii.

[17] S. Lefschetz, "Nonlinear Differential Equations," 7. Aleksandr A. Andronov, [A. A. Witt,] and C. E. Chaikin, *Theory of Oscillations,* abridged transl. Natasha Goldskaja, ed. Solomon Lefschetz (Princeton: Princeton University Press, 1949); *Theory of Oscillators,* transl. F. Immirzi (Oxford: Pergamon, and Reading: Addison-Wesley, 1966). The name of Aleksandr Adol'fovich Witt, who disappeared during Stalinist purges, was suppressed in 1937, but reinstalled in 1959 for the second Russian edition. S. Diner, "Les voies du chaos," 342.

[18] See Amy Dahan Dalmedico, "Le difficile héritage de Henri Poincaré en systèmes dynamiques," in *Sonderdruck aus Henri Poincaré: Science et philosophie, Congrès international de Nice, 1994* (Berlin: Akademie; Paris: Albert Blanchard, 1995): 13-33, esp. 20-23.

[19] *Conservative* dynamical systems are those for which the total energy is conserved; *dissipative* systems generally include a friction term, which dissipate energy into heat.

a) Dissipative Systems and the van der Pol Equation

For a long time, dissipative systems were thought to be less interesting than conservative ones for the reason that, in the long run, they tended toward rest. Or so it seemed. As George D. Birkhoff wrote in a famous book, a "dissipative system of this type tends in its unconstrained motion either toward equilibrium or, more generally, toward the motion of a conservative system with fewer degrees of freedom."[20] When in the late twenties, Andronov tackled this problem, however, dissipative systems witnessed a period of renewed interest mainly due to the work of Balthasar van der Pol (1889-1959), an engineer at the Phillips Company in Eindhoven, Holland. By simplifying to the extreme the equation for the amplitude of an oscillating current driven by a triode, he indeed exhibited an example of a dissipative equation without forcing which nonetheless sustained spontaneous oscillations, an example of what Ilya Prigogine would later call *dissipative structures.*[21]

(i) Mathematics and Radio Problems

In a lecture given on March 15, 1947, before the Dutch Mathematical Center in Amsterdam, van der Pol recalled his original problem and how it led to more abstract mathematical concerns:

---

[20] G. D. Birkhoff, *Dynamical Systems* (Providence: American Mathematical Society, 1927), 32. The type of systems he is considering here are those which are not subject to any external force, or to external force that do no work, i.e. systems that receive no external energy. Birkhoff's ideas on stability are presented below; see p. 267.

[21] B. van der Pol, "On 'Relaxation-Oscillations'," *Philosophical Magazine,* 2 (1926): 978-992; repr. *Selected Scientific Papers,* ed. H. Bremmer and C. J. Bouwkamp (Amsterdam: North-Holland, 1960): 346-360. For more on van der Pol, see G. Israel, *La*

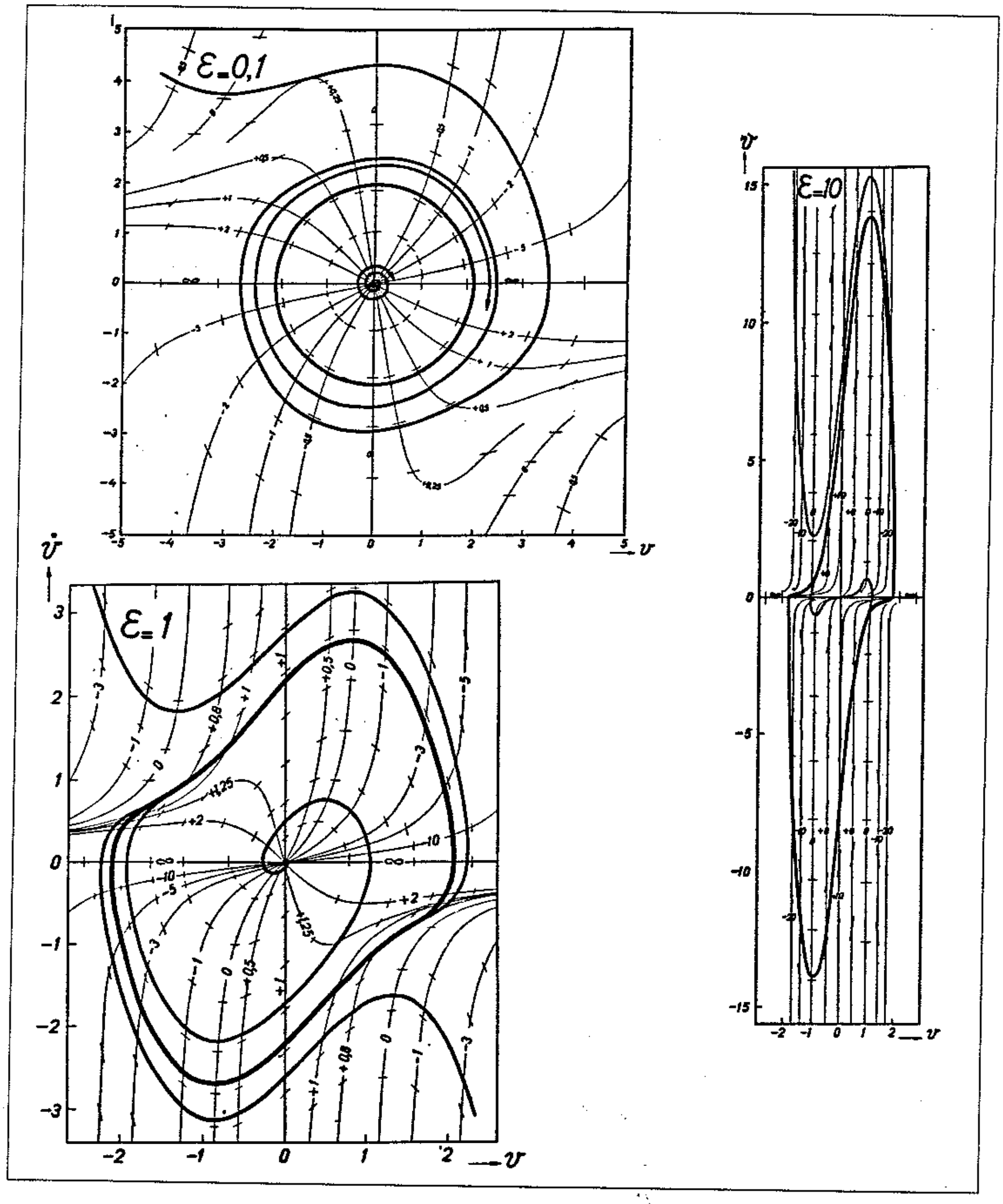

Figure 8: Flow in Phase Space for the van der Pol Equation for ε=.1; ε=1; and ε=10. Repr. with permission from B. van der Pol, "On Relaxation-Oscillations," 983-985. Copyright © Taylor and Francis.

Mathématisation du réel (Paris: Seuil, 1996), 34-51. About dissipative structures, see Chapter VIII below.

> How does a retroactive triode cause a simple electric circuit to oscillate? In 1920, I was able, by ignoring all secondary phenomena, to crystallize this perhaps most fundamental equation of all modern radio problems into the following, non-linear differential equation:
>
> $$v'' - \varepsilon(1 - v^2)v' + v = 0;$$
>
> in which the constant $\varepsilon > 0$, and the [primes] signify differentiation with respect to time, whilst v is the voltage across the oscillator circuit.[22]

This equation, van der Pol explained in 1947, had been worked out for the purpose of solving a technical problem, by making the explicit assumption that ε remained much smaller than 1.[23] There was great urgency in trying to understand this problem since triode vacuum-tube generators had then "become the standard low power source of alternating current for laboratory purposes."[24] A few years later, van der Pol impelled a new direction to his studies:

> in 1926, purely as a matter of mathematical interest, we asked ourselves whether this equation also led to interesting results in cases where ε is large, and this disclosed the theory of *relaxation-oscillations.*[25]

Analytic solutions for the van der Pol equation are extremely rare, but already in his 1926 article, van der Pol drew trajectories in phase space, that is, plotted v versus $z = v'$, for three values of ε, namely 0.1, 1, and 10. These plots clearly showed that

---

[22] B. van der Pol, "Mathematics and Radio Problems," *Phillips Research Reports,* 3 (1948): 174-190; repr. in *Selected Papers,* 2: 1140-1156. Originally published in Dutch in *Simon Stevin,* 25 (1947): 179-198. The quote is from *Selected Papers,* 2, 1154. The above equation is often called the van der Pol equation in the technical literature. See also B. van der Pol's review essay: "The Nonlinear Theory of Electric Oscillations," *Proceedings of the Institute of Radio Engineers,* 22 (1934): 1051-1086; repr. *Papers,* 1: 795-830.

[23] B. van der Pol, "A Theory of the Amplitude of Free and Forced Triode Vibrations," *Radio Review,* 1 (1920): 701-710; 754-762; repr. *Selected Papers,* 1: 228-246.

[24] E. V. Appleton and B. van der Pol, "On the Form of Free Triode Vibrations," *Philosophical Magazine,* 6th ser., 42 (1921): 201-220; *Selected Papers,* 1: 258-280. Quote on p. 258.

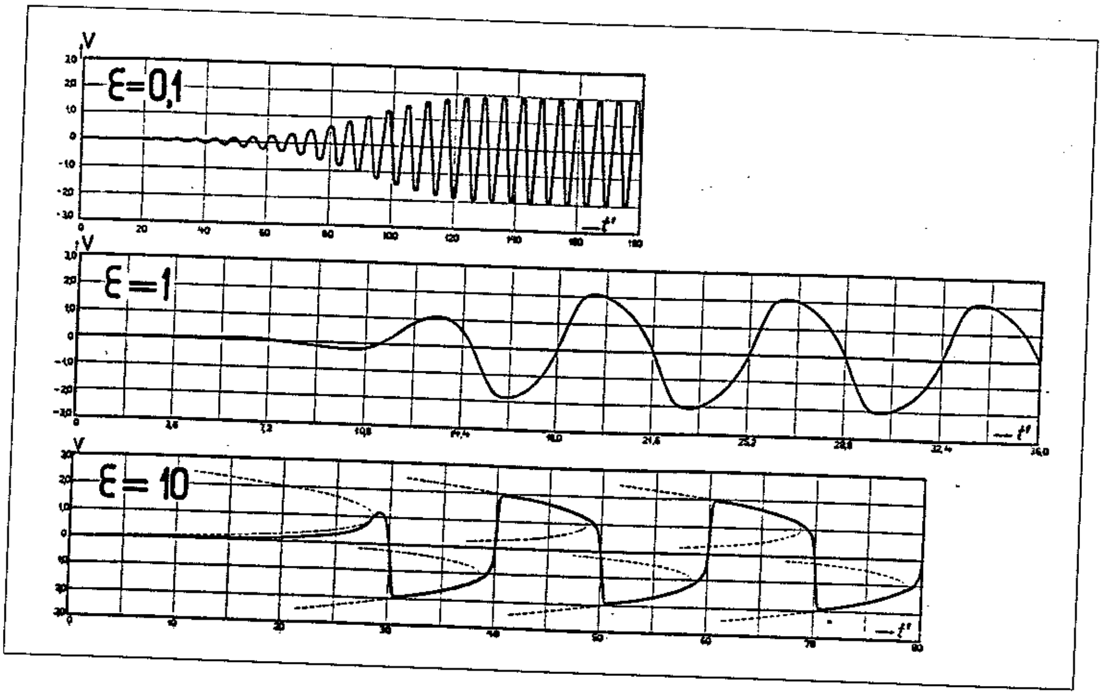

Figure 9: Solutions as Function of Time of the van der Pol Equation for ε=0.1; ε=1; and ε=10. Repr. with permission from B. van der Pol, "On Relaxation-Oscillations," 986. Copyright © Taylor and Francis.

solutions tended always to wind up around a periodic stationary solution, represented as a closed curve. In contemporary terms, this was one of the first concrete examples of an *attractor* in a physical system that was not a point (Fig. 8 and 9).

(ii) French Reception and Rocard's Insensitivity

The van der Pol equation had a stupendous career in scientific literature, among electrical engineers of course, but also among mathematicians, physicists, biologists, etc. According to van der Pol's own count, it had led, in 1947, to "at least one hundred papers and books, particularly from the Russian and French quarters."[26]

[25] S. Diner, "Les voies du chaos," 341.
[26] B. van der Pol, "Mathematics and Radio," 1150.

From Chapter II above, we may recall that, in 1948, the Bourbaki mathematician André Weil made one exception to his ideology of purity in mathematics, by listing the van der Pol equation as "one of the few interesting problems which contemporary physics has suggested to mathematics."[27] In 1928, French scientist A. Liénard generalized van der Pol's investigation by studying equations of the type:

$$\frac{d^2x}{dt^2} + \omega f(x)\frac{dx}{dt} + \omega^2 x = 0.$$

Weil's comment has to be understood in context. Indeed, a source for Liénard's interest was a memoir by Élie and Henri Cartan, to my knowledge the only one written jointly by the father and the son. Dealing with an equation very similar to Liénard's, the Cartans made it clear, with the notation they used, that the source of their inspiration lay in electrical circuit theory:

$$L\frac{d^2i}{dt^2} + (R - \varphi(i))\frac{di}{dt} + \frac{1}{C}i = 0;$$

this equation being easily recognizable as the one that describes a circuit with inductance L, condenser C, and resistance $R - \varphi(i)$ depending on the intensity of the current i.[28]

[27] A. Weil, "The Future of Mathematics," *Great Currents*, ed. F. Le Lionnais: 321-336, 332. The van der Pol equation seemed to have gather of rare consensus in its support, since, for example, applied mathematician Theodore von Kármán selected it as his first example of an engineering problem that should be of interest to a mathematician: see "The Engineer Grapples with Nonlinear Problems," *Bulletin of the American Mathematical Society*, 46 (1940): 615-683, esp. 619-624.

[28] A. Liénard, "Étude des oscillations entretenues," *Revue générale de l'électricité*, 26 (1928): 901-912 and 946-954. Élie and Henri Cartan, "Note sur la génération des oscillations entretenues," *Annales des PTT*, 14 (1925): 1196-1204; repr. Élie Cartan, *Œuvres complètes*, part III, 1: 71-82. P. Janet, "Note sur une ancienne expérience d'électricité appliquée," *Annales des PTT*, 14 (1925): 1193-1195; summarized in *Revue générale de l'électricité*, 19 (1926): 98D.

The main question that these French mathematicians sought to answer was the following: What form should the function $f(x)$ in the Liénard equation have so that it admitted one or several stable periodic solutions?[29] Clearly, other behaviors could happen, they knew it, but for obvious reasons stemming from the radio-engineering aspects of the question, these remained unexplored. As late as the early 1970s, Maurice Roseau's course of mechanics at the Université de Paris still strongly emphasized this aspect of the question.[30]

Far from being a blind neglect of a crucial flip side of the problem, this line of research proved extremely fruitful, leading in particular to the work of Mary L. Cartwright, John E. Littlewood, and Norman Levinson on the van der Pol equation with a forcing term $e(t)$ on the right-hand side of the equation. These important contributions will be examined below.

From the point of view of the history of nonlinear dynamics in France, widespread interest for van der Pol's and Liénard's equations led to important consequences. Indeed, Yves Rocard's famous textbook on nonlinear oscillations, written in July 1940, was highly focused on the above equations, which he had considered for highly practical

---

[29] See also J. Haag's work: "Sur les oscillations auto-entretenues," *CRAS,* 199 (1934): 906-909; "Sur l'étude asymptotique des oscillations de relaxation," *CRAS,* 202 (1936): 102-104; "Sur la théorie des oscillations de relaxation," *CRAS,* 204 (1937): 932-934; "Formules asymptotiques concernant les oscillations de relaxation," *CRAS,* 206 (1938): 1235-1237.

[30] See M. Roseau, *Vibrations non linéaires et théorie de la stabilité* (Berlin: Springer, 1966); *Solutions périodiques ou presque périodiques des systèmes différentiels de la mécanique non linéaire,* lecture notes (Département de mécanique, Faculté des sciences, Paris, 1970). Jussieu Lib.

purposes.[31] One of the important conclusions at which he arrived was that oscillators of a van der Pol type exhibited a behavior that was *independent from initial conditions.* After a few oscillations, they always tended towards a periodic solution, for which not only the period, as for harmonic oscillators, but also the amplitude were dictated by the equation and not the initial conditions. Knowing that Rocard's lectures at the École normale supérieure, as well as successive editions of his book, have been an important introduction to the subject for many of the French physicists who later dealt with chaos, we may conclude that sensitive dependence on initial condition was the more striking to them.[32]

(iii) A Model of Mathematical Models?

This wide interest for van der Pol's work hardly stemmed from its mathematical interest alone, but because, as Giorgio Israel recently put it, it provided a "model of models."[33] It appeared to van der Pol that relaxation-oscillations could accurately describe almost every oscillating phenomenon of nature.

> Many instances of relaxation oscillations can be cited, such as: a pneumatic hammer, the scratching noise of a knife, the waving of a flag in the wind, the humming noise sometimes made by watertraps, the squeaking of a door, a steam engine with too small flywheel, . . . the intermittent discharge of a condenser through a neon-tube, the periodic reoccurrence of epidemics and of economic crises, the periodic density of an even number of species of animals living together and one species serving as food to the other, the sleeping of flowers, the periodic

---

[31] Y. Rocard, *Les Problèmes d'auto-oscillation dans les installations hydrauliques* (Paris: Hermann, 1937). See also Y. Rocard, *L'Instabilité en mécanique. Automobiles - avions - ponts suspendus* (Paris: Masson, 1954).

[32] See Y. Pomeau, "Préface," *Le Chaos. Théorie et expériences,* ed. P. Bergé (Paris: Eyrolles, and Éditions du CEA, 1988)). Yves Rocard, *Théorie des oscillateurs* (Paris: Éditions de la Revue scientifique, 1941); *Dynamique générale des vibrations,* 3rd ed. (Paris: Masson, 1960).

[33] G. Israel, *La Mathématisation du réel,* 34.

> reoccurrence of showers behind a depression, the shivering from cold, the menstruation, and finally the beating of the heart.[34]

About such a juxtaposition, the first thing to note is how ludicrous it appears! No underlying physical mechanism could unify all of the above in a single explanatory scheme. But this was not van der Pol's claim. Rather he suggested that oscillatory solutions of dissipative equations could account for all of the above. He tried to develop mathematical models for none of the above, with one notable exception. He did build a model of the heart. But, in my view, to suggest as Israel does that it actually was a *mathematical* model is somewhat misleading; it rather was an *electrical* model of the heart.[35] Van der Pol and his collaborator, van der Mark, built physical apparatuses made of electric circuits, the output of which traced curves resembling electrocardiograms. To use a fashionable term, they literally *blackboxed* the physiology of the heart, not with mathematical equations which would have been intractable, but with electrical tabletop models. That this electrical model could be expressed with differential equations, van der Pol and van der Mark hardly doubted. But as a mathematical model, their model of the heart remained the expression of a program rather than an actualization of this program.

In conclusion, van der Pol's work therefore exhibited the dual aspect characteristic of much of the mathematical research I look into in this dissertation. It combined an important advance in *qualitative* mathematical knowledge and a strong taste for *analogy* as method of mathematical modeling of the world. Historically, therefore, van der Pol

---

[34] Balthasar van der Pol and J. van der Mark, "The Heartbeat Considered as a Relaxation Oscillation, and an Electric Model of the Heart," *Philosophical Magazine,* 7th ser., 6 (1928): 763-775; repr. *Papers,* 1: 486-511; quote on pp. 491-492. Quoted in G. Israel, *La Mathématisation du réel,* 40.

[35] Cf. G. Isreal, *La Mathématisation du réel,* 34-51.

should not be seen as an example of those scientists who, dealing with nonlinearities—and they were many even then—remained blind to the manifestations of chaos when they should have discovered it, as Kellert would have it.[36] Rather van der Pol was someone who forged the tools for, and helped create the possibility of, looking at dissipative systems in a new light. As such, he went farther than Poincaré in a certain respect. His work was not only rediscovered by later-day chaologists but also spurred on the moment important developments contributing, at the level of both mathematical techniques and modeling practice, to the possibility of the emergence of catastrophe and chaos theories.

b) Stability in Mathematics and in Modeling Practice for Radio Engineering

As van der Pol noted in 1947, his work was well received by some Russian mathematicians, and especially Aleksandr Andronov, who was among the first to use powerful new topological techniques to study nonlinear oscillations such as those exhibited by the Dutch engineer.

(i) Coarse Systems

In a note published by the French Academy of Science, Andronov identified solutions of the van der Pol equation as examples of "self-oscillations," also found in chemistry, biology, and physics, described as such:

> These systems are ruled by differential equations that differ from those studied by mathematical physics and classical mechanics. The systems where these

[36] S. Kellert, *In the Wake of Chaos,* 125-127. See also, J. Gleick, *Chaos,* 49. For a historical and mathematical discussion of the strange attractors to be found in the forced van der Pol equation, see R. H. Abraham, "In Pursuit of Birkhoff's Attractor," in *Singularities and Dynamical Systems,* ed. S. N. Pnevmatikos (Amsterdam: North-Holland, 1985): 303-312.

> phenomena are produced are not conservative and sustain their oscillations by drawing their energy from nonperiodic sources.[37]

More generally than van der Pol and even Liénard, Andronov considered the stationary solutions of a class of two-dimensional systems of differential equations:

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

x and y being coordinates of the plane.[38] He claimed that when the system satisfied some conditions, the self-oscillations mathematically corresponded to Poincaré's limit cycles.[39] As Dahan Dalmedico emphasized:

> Andronov had the not-at-all obvious idea, almost forty years after their publication, of turning towards Poincaré's works. These had almost never been applied to concrete problems of physics or engineering.[40]

To get this result, Andronov imposed conditions on the above system of equations which he would come back to, since they amounted to structural stability. Formally, he would do this in a note, written with the blind topologist, L. Pontrjagin, and presented at the Soviet Academy of Sciences in 1937, in which they introduced what they named

---

[37] A. A. Andronov, "Les cycles limites de Poincaré et la théorie des oscillations auto-entretenues," *CRAS,* 189 (1929): 559-561, 559. In this note, he refers to the work of Volterra and Lotka in biology, of Kreman in chemistry, of Eddington in astrophysics, and of Lord Rayleigh in the theory of sound, in addition to van der Pol's.
[38] By setting $z = v'$, van der Pol's equation can be rewritten as such a system of equations, namely $v' = z; \quad z' = \varepsilon(1-v^2)z - v$.
[39] Introduced in H. Poincaré, "Mémoire sur les courbes définies par une équation différentielle," *Journal de mathématiques pures et appliquées,* 3rd ser., 7 (1881): 375-422; 8 (1882): 251-296; repr. *Œuvres,* 1: 3-84; see pp. 53-65 on limit cycles, where he introduces what is now known as the Poincaré section. See J.-L. Chabert and A. Dahan Dalmedico, "Les idées nouvelles de Poincaré" in *Chaos et déterminisme,* ed. A. Dahan Dalmedico *et al.* (Paris: Seuil, 1992), 274-305; and C. Gilain, "La théorie qualitative de Poincaré et le problème de l'intégration des équations différentielles," *La France mathématique,* ed. H. Gispert (Paris: SFHST and SMF, 1991): 215-242.
[40] A. Dahan Dalmedico, "Le difficile héritage," 22.

"coarse systems [*systèmes grossiers*]."[41] A system of the above type was said to be coarse if a small variation to it did not alter the topological character of the trajectories of its solutions. They considered the altered system:

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

where p, q, and their derivatives remained small. The condition for coarseness expressed that there existed a one-to-one correspondence between the trajectories of the two systems, such that singular points were sent to singular points, limit cycles to limit cycles, etc. Andronov and Pontrjagin then gave, without proofs, a complete classification of two-dimensional coarse systems.

In a revealing footnote, they added:

> This definition of a system's roughness can be considered as that of the stability of a dynamical system with respect to small variations. . . . *This kind of stability is interesting for physics.*[42]

There lay the source of Andronov's concerns for stability. Van der Pol's equation came about by studying radio engineering and this source left its imprint on the field of nonlinear mechanics. The case of Mary Lucy Cartwright, a Cambridge mathematician who worked extensively in this field starting in 1939, will underscore this.

---

[41] A. A. Andronov and L. Pontrjagin, "Systèmes grossiers," *Comptes-rendus (Doklady) de l'Académie des sciences de l'URSS,* 14 (1937): 247- 250. See also the following note: E. Leontovich and A. Mayer, "Sur les trajectoires qui déterminent la structure qualitative de la division de la sphère en trajectoires," *ibid.,* 251-254. I preferred "coarse" rather than "rough," also found in the literature, as the translation to the French original term "*grossier*" since the former was used in the 1966 edition of Andronov et al., *Theory of Oscillators,* xxix.

[42] A. Andronov and L. Pontrjagin, "Systèmes grossiers," 247-248n. My emphasis.

(ii) Stability in Cartwright's Work

Although working within a totally different mathematical tradition, Cartwright nevertheless also studied, because of radio engineering concerns, the stability of equations of the type of van der Pol's. In 1952, she explained why this was a preoccupation for radio engineers:

> To me the work of radio engineers is much more interesting and suggestive than that of the mechanical engineers. The radio engineers *want their systems to oscillate,* and to oscillate in a very orderly way, and therefore they want to know not only whether the system has a periodic solution, but *whether it is stable,* what its period and amplitude and harmonic content are, and *how these vary with parameters of the equations.*[43]

This statement is the more striking considering that Cartwright's concerns and approach always remained quite different from Andronov's. In her account of the history of nonlinear mechanics, she did not mention Andronov's work, even though she paid attention to some of the Russian contributions, such as Krylov and Bogoliubov's.

Collaborating with John E. Littlewood, Cartwright started her research in the field of nonlinear vibrations after Britain's

> Department of Scientific and Industrial Research issued a memorandum appealing for the assistance of pure mathematicians in solving the type of equations occurring in radio work, *laying emphasis on the need to know how the frequencies of the periodic solutions varied with the parameters of the equations.*[44]

Beginning "with little knowledge of the classical work of Poincaré, Liapounov and Birkhoff," she never adopted topological methods such as Andronov and Pontrjagin's.[45] She studied specific systems, admittedly with parameters that could be

---

[43] M. L. Cartwright, "Non-Linear Vibrations: A Chapter in Mathematical History," *Mathematical Gazette,* 36 (1952): 80-88, 84. My emphasis.
[44] M. L. Cartwright, "Non-Linear Vibrations," 86. My emphasis.
[45] M. L. Cartwright, "Non-Linear Vibrations," 87.

tinkered with, but never ventured into topological classifications like Andronov and Pontrjagin's. Cartwright and Littlewood's case, which so markedly departs from Andronov's program, only shows how much the practical incentive for the mathematical theory could bend research programs towards studying the stability of solutions in presence of perturbations.[46]

Van der Pol certainly took notice of Cartwright's work. "A certain phase of this subject [the theory of relaxation oscillations]," did he write in 1947, "was concluded a few months ago by highly important investigations carried out by Miss Cartwright and Littlewood." That a new phase was dawning may be apparent from the fact that the van der Pol equation was one of the first one to be integrated with the help of Vannevar Bush's differential analyzer at MIT.[47]

(iii) Stability as Program and Philosophy

As opposed to Cartwright and Littlewood, Aleksandr Andronov approached the study of nonlinear vibrations not only with new mathematical tools, but also with a vast philosophical program. As Vladimir Arnol'd emphasized, the concept of coarseness appeared in Andronov's work as both a mathematically rigorous definition and a general

[46] See her most important work: M. L. Cartwright and J. E. Littlewood, "On Non-Linear Differential Equations of the Second Order: I. The Equation $y'' - k(1-y^2)y' + y = b\lambda k\cos(\lambda t + a)$, k Large," *Journal of the London Mathematical Society,* 20 (1945): 180-189; repr. in *The Collected Papers of John Edensor Littlewood,* 1 (Oxford: Clarendon Press, 1982): 85-94; and M. L. Cartwright, "Forced Oscillations in Nearly Sinusoidal Systems," *Journal of the Institute of Electrical Engineering,* 95 (1948): 88-94. These articles are important for the prehistory of chaos since they directly inspired Levinson's paper that exhibited an instance of strange attractor, that oriented Smale's discovery of the horseshoe. See below p. 287.

idea about the type of systems useful for mathematical modeling in physics and engineering.[48]

In the lengthy introduction to his book, Andronov set out to articulate his program, which he had previously presented in front the First All-Union Conference on Auto-oscillation in November 1931.[49] Concerned, as were van der Pol and Cartwright, with real physical systems, Andronov saw the problem of the fit between the model and reality as posing itself with urgency. "In any theoretical investigation of a real system we are always forced to simplify and idealize, to a greater or smaller extent, the true property of the system."[50] The question was: how far could one go?

Clearly, many properties of physical systems were lost in any kind of idealization. Due to fluctuations, in particular, "real systems," Andronov stated, "*cannot in general be described with complete accuracy by means of mathematical relations.*"[51] This was a radical position that clearly distinguished mathematical models from physical systems, and flatly denied the possibility of a straightforward identification of the two. On top of this metaphysical reason, unavoidable fluctuations were bound to occur in any real physical system whatever their origins: quantum mechanics or the always imperfectly controlled environment.

---

47 B. van der Pol, "Mathematics and Radio," 1150. About Bush's analog computer, see A. G. Bromley, "Analog Computing Devices," *Computing Before Computers,* ed. W. Aspray (Ames: Iowa State University Press, 1990: 156-199, 179-185.
48 V. I. Arnol'd, "Catastrophe Theory," in *Dynamical Systems V: Bifurcation Theory an Catastrophe Theory,* ed. V. I. Arnol'd, Encyclopedia of Mathematical Sciences, 5 (Berlin: Springer, 1994 [1986]): 207-264, 224.
49 V. I. Arnol'd, "Catastrophe Theory," 260.
50 A. A. Andronov et al., *Theory of Oscillators,* xv.
51 A. A. Andronov et al., *Theory of Oscillators,* xxv. My emphasis.

> The presence of fluctuation in real systems must indirectly be taken into account even in the theory of dynamic models of real systems. 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.*[52]

Consequently, for the mathematical physicist the object of study changed. Beyond the study of a system of differential equations, and its solutions, beyond even the study of equations depending on a set of parameters, one had to deal with more general families of laws. The implication 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.*"[53]

For this reason, the notion of coarseness was crucial, since only coarse systems had a chance of being useful for physics. Only they could actually model the processes taking place in the real world. Andronov and coworkers indeed required that the processes they studied "be *stable both* in relation to *small variations of the coordinates and velocities, and* in relation to *small variations of the mathematical model itself.*"[54] They went on:

> The first requirement leads to the concept of stability of states of equilibrium of the model and of the processes taking place in it, and the second to the concept of coarseness of dynamic systems. . . . Systems that are such not to vary in their essential features for a small variation of the form of the differential equations, we shall call *'coarse'* systems.[55]

One should notice here an interesting analogy between Andronov and his colleagues' reasoning and a famous statement of Pierre Duhem's (1861-1916). In his famous book, the French physicist-philosopher set forth, along with some of Poincaré's

52 A. A. Andronov et al., *Theory of Oscillators,* xviii(note). My emphasis. See also pp. xxvii-xxix.
53 A. A. Andronov et al., *Theory of Oscillators,* xxviii. My emphasis.
54 A. A. Andronov et al., *Theory of Oscillators,* xviii(note). My emphasis.

work, Jacques Hadamard's (1865-1963) construction of geodesics on surfaces with negative curvature.[56] In this work, Hadamard exhibited a mathematical construction which displayed the property later called "sensitive dependence on initial conditions" by Ruelle, and thus described by Hadamard:

> Any change, no matter how small, brought to the initial direction of [any] geodesics . . . is enough to bring about absolutely any variation to the final outlook of the curve.[57]

For Duhem, this implied that this "mathematical deduction" could "never be utilized" in physics.[58] The reasons for which he stated this resemble Andronov's:

> One cannot go through the numerous and difficult deductions of celestial mechanics and mathematical physics without suspecting that many of these deductions are condemned to eternal sterility.
>
> Indeed, a mathematical deduction is of no use to the physicist so long as it is limited to asserting that a given *rigorously* true proposition has for its consequence the *rigourous* accuracy of some such other proposition. To be useful for the physicist, it must still be proved that the second proposition remains *approximately* exact when the first is only *approximately* true.[59]

Andronov and his coworkers apparently concurred with Duhem's analysis. They even went a step further and asserted that mathematical models, to stand a chance of faithfully representing physical systems, had to be stable, not only with respect to small variations in the initial conditions, but also in the very form of the differential equation

---

55 A. A. Andronov et al., *Theory of Oscillators,* xviii(note) and xxix.
56 On this, see J.-L. Chabert, "Hadamard et les géodésiques des surfaces à courbure négative," *Chaos et déterminisme,* ed. A. Dahan Dalmedico et al. (Paris: Seuil, 1992): 306-330; and the original paper: J. Hadamard, "Les surfaces à courbure opposées et leurs lignes géodésiques," *Journal de mathématiques pures et appliquées,* 4 (1898): 27-73; repr. *Œuvres,* 2 (Paris: Éditions du CNRS, 1968): 729-775.
57 J. Hadamard, *Œuvres,* 2, 772-773; quoted in J.-L. Chabert, "Hadamard," 325.
58 P. Duhem, *La théorie physique. Son objet et sa structure* (Paris: Marcel Rivière, 1914; 1ère édition, 1906); *The Aim and Structure of Physical Theory,* transl. Philip P. Wiener (Princeton: Princeton University Press, 1954), 138.

itself. As an important consequence, Andronov neglected unstable motions and emphasized the study of stationary ones, i.e. rest, equilibrium, periodic and quasiperiodic motions, in other words Birkhoff's *recurrent motions,* which Birkhoff himself characterized as "a natural extension of periodic motions."[60]

c) Birkhoff: Conventionalism for Stability

One of Poincaré's only "true disciples" in the qualitative study of differential equations, George David Birkhoff (1884-1944), offers another approach to be contrasted with Andronov's. Like the Russian mathematician and at about the same time, Birkhoff reflected on the role of stability for the mathematical modeling of the world.[61] Instead of focusing on a single concept, like coarseness, Birkhoff adopted the more supple view that different concepts of stability could be used for different purposes, depending on the questions one wanted to answer; the choice merely was conventional.

> The fundamental fact to observe here is that this concept [stability] is not in itself a definite one but is interpreted according to the question under consideration.[62]

---

[59] P. Duhem, *The Aim and Structure,* 143.
[60] G. D. Birkhoff, "Quelques théorèmes sur le mouvement des systèmes dynamiques," *Bulletin de la Société mathématique de France,* 40 (1912): 305-323; repr. G. D. Birkhoff, *Collected Mathematical Papers,* 1 (New York: Dover, 1968 [1950]): 654-672, 654.
[61] Amy Dahan Dalmedico called Birkhoff "a true disciple" of Poincaré, although they probably never met. "Le difficile héritage," 24-27. For biographical information on Birkhoff and his work, see E. T. Whittaker, "George David Birkhoff," *Journal of the London Mathematical Society,* 20 (1945): 121-128; and the introductory essays in G. D. Birkhoff, *Papers,* 1.
[62] G. D. Birkhoff and D. C. Lewis, Jr., "Stability in Causal Systems," *Philosophy of Science,* 2 (1935): 304-333, 313; repr. *Papers,* 3: 575-604, 584. One should think here of Poincaré's philosophy of science often termed "conventionalism"; see D. J. Stump, *Conventionalism and Truth: Poincaré's Mediation Between Relativism and Absolutism in Science,* Ph.D. thesis (Nortwestern University, 1988).

Birkhoff always based his reflections on Poincaré's. Although Birkhoff was trained at Chicago, "Poincaré was Birkhoff's true teacher," once said Birkhoff's own student Marston Morse.[63] Shortly after Poincaré's untimely death in 1912, Birkhoff established his reputation by proving a conjecture known as "Poincaré's last geometric theorem."[64] According to Morse, this proof "was one of the most exciting mathematical events of the era and was widely acclaimed."[65] As Poincaré had already seen, this theorem had important consequences for dynamical theories.

Having read Poincaré's *Méthodes nouvelles de la mécanique céleste,* while at Princeton in 1912, Birkhoff started to work on the field he would call dynamical systems.[66] That year, he introduced the notions of "minimal" and "recurrent" motions.[67] His work on qualitative dynamics eventually culminated in his 1927 book, much of its content having been delivered on September 5-8, 1920 at the University of Chicago. According to Morse, "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

---

[63] O. Vleben, "George David Birkhoff (1884-1944)," *Yearbook of the American Philosophical Society* (1946): 279-285; repr. G. D. Birkhoff, *Papers,* 1: xv-xxiii.
[64] The theorem states that continuous, one-to-one, area-preserving maps from the annulus to itself that rotates the points on the boundaries in opposite directions have at least two fixed points. Henri Poincaré, "Sur un théorème de géométrie," *Rendiconti dei circolo matematico di Palermo,* 33 (1912): 375-407; repr. *Oeuvres,* 6: 499-538; and G. D. Birkhoff, "Proof of Poincaré's Geometric Theorem," *Transactions of the American Mathematical Society,* 14 (1913): 14-22; repr. *Papers,* 1: 673-681; French transl. *Bulletin de la Société mathématique de France,* 46 (1914): 1-12. See also G. D. Birkhoff, *Dynamical Systems,* 163-170.
[65] M. Morse, Preface to G. D. Birkhoff, *Dynamical Systems,* 2nd ed. (Providence: American Mathematical Society, 1966), iv.
[66] O. Vleben recalled Birkhoff's reading of Poincaré; see G. D. Birkhoff, *Papers,* 1: xv-xxiii.
[67] G. D. Birkhoff, "Quelques théorèmes."

Möser on the celebrated KAM theorem.[68] As we shall see, many scientists were inspired by it in less direct ways.

"The final aim of the theory of motion must be directed toward the qualitative determination of all possible types of motions and of the interrelation of these motions."[69] In Chapter 7 of his book, Birkhoff developed a "General Theory of Dynamical Systems," going further than Poincaré and Hadamard in the topological study of curves defined by differential equations.[70] In particular, he generalized Poincaré's limit cycles, by introducing several interesting concepts that prefigured different facets of the concept of *attractor*: non-wandering, minimal, alpha- and omega-limit sets, central and recurrent motions.[71] On the basis of these definitions, Birkhoff stated:

---

68 M. Morse, Preface, v; see Möser's Introduction in *ibid.* also. About the history of KAM theorem, see F. Diacu and P. Holmes, *Celestial Encounters,* chap. 5. It is also briefly discussed below.

69 G. D. Birkhoff, *Dynamical Systems,* 189. See also G. D. Birkhoff, "Recent Advances in Dynamics," *Science,* n.s., 51 (1920): 51-55; repr. *Papers,* 2: 106-110.

70 See A. Dahan Dalmedico, "Le difficile héritage," 25; G. D. Birkhoff, *Dynamical Systems,* 189-202.

71 In Birkhoff's 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 (*Dynamical Systems,* 192)." Now, finding the non-wandering 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 which come back arbitrary close to every point of the curve of motion. They are in the set of central motions but the reverse is not necessarily true. α- and ω-*limit points* are defined as the sets of limit points as time (t) tends to - or + ∞. Nonwandering sets are in general larger than limit sets. For these, and other, definitions, see G. D. Birkhoff, *Dynamical Systems,* 191-200. Some of these were picked up in A. A. Andronov and A. A. Witt, "Sur la théorie mathématique des auto-oscillations," *CRAS,* 190 (1930): 256-258. An attractor has been succinctly defined as "an *indecomposable, closed, invariant* set . . . which attracts all orbits starting at points in some neighborhood" by P. Holmes, "Poincaré, Celestial Mechanics, Dynamical-Systems Theory, and 'Chaos'," *Physics Reports,* 193 (1990): 137-163. For more on attractors, see below.

> a first problem concerning the properties of dynamical systems is the determination of the central motion. . . . [So,] the structure of the set of central motions is of vital theoretic importance."[72]

Generally speaking, central motions were those "which all other motions tend[ed] to approach."[73] Here is the seed of future focus on attractors.

The stability of the solution curves of dynamical systems was the central concern of Birkhoff in his book. He introduced a large array of notions of stability for dynamical systems and their periodic solutions, some of which already present in the literature, some of which new: complete or trigonometric stability, stability of the first order, permanent stability ("for which small displacements from equilibrium remain small over time"), semi-permanent stability, unilateral stability (due to Lyapunov), and stability in the sense of Poisson (due to Poincaré).[74]

Although stemming out of totally different worlds—clearly Harvard mathematics department and the Gorki Institute must have been worlds apart from one another—there are interesting comparison to be made between Birkhoff's and Andronov's approaches. While both dealt with general systems of (nonlinear) differential equations, using many of the same sources (Poincaré, Lyapunov), and while both emphasized stability as a way of probing these systems, they nonetheless ended up with almost opposite views on stability. For Andronov, the practice of mathematical modeling implied that only coarse systems were of interest. Birkhoff thought that one had to dictate, by convention or by a judicious choice of problems to be answered, the kind of stability that one wanted to look at.

---

72 G. D. Birkhoff, *Dynamical Systems,* 197 and 202.
73 G. D. Birkhoff and D. C. Lewis, "Stability," 309.
74 The stability of motions is dealt with mostly in chapters 4, 6, 8 and 9 of G. D. Birkhoff, *Dynamical Systems*. The quote is from p. 121.

> All that stability can mean is that, for the system under consideration, those motions whose curves lie in a certain selected part of phase space from and after a certain instant are *by definition* called stable, and other motions unstable.[75]

Interestingly, both Birkhoff and Andronov reflected on the philosophy of mathematical modeling of physical facts. Unlike future Bourbakists, Birkhoff thought that without a doubt mathematics was the language of nature, which itself should guide mathematicians' speculations.

> It will probably be the new mathematical discoveries which are suggested through physics that will always be most important, for, from the beginning, Nature has led the way and established the pattern which mathematics, the language of Nature, must follow.[76]

Like his master Poincaré, Birkhoff was a mathematical physicist; he worked on ergodic theory, wrote books on relativity, and remained ever skeptical of quantum mechanics. Furthermore Birkhoff believed that mathematics could offer guidance for other aspects of the human experience, like aesthetics or even ethics.[77] He issued repeated calls for a further collaboration between physicists and mathematicians:

> It is to be hoped that in the future more and more theoretical physicists will command a deep knowledge of mathematical principles; and also that mathematicians will no longer limit themselves so exclusively to the aesthetic development of mathematical abstractions.[78]

---

[75] G. D. Birkhoff and D. C. Lewis, "Stability," 332. My emphasis.
[76] G. D. Birkhoff, "The Mathematical Nature," 310; repr. 919. About Poincaré's recurrence theorem and Birkhoff's use of it, see A. Dahan-Dalmedico, "Le difficile héritage."
[77] First presented at the 1928 International Congress of Mathematician ("Quelques éléments mathématiques de l'art," *Atti del Congresso internazionale dei matematici, Bologna, 3-10 settembre 1928 (VI),* 1 [Bologna: Nicola Zanichelli, 1928]: 315-333; repr. *Collected Papers,* 3: 288-306), Birkhoff's theory of aesthetics inspired him many articles to be found in the 3rd volume of his *Collected Papers,* and a book *Aesthetic Measure* (Cambridge: Harvard University Press, 1933), which however I have never seen.
[78] G. D. Birkhoff, "The Mathematical Nature of Physical Theories," *American Scientist,* 31 (1943): 281-310, 286; repr. *Papers,* 2: 890-919, 895.

In many ways not too far from Thom's, Birkhoff's philosophy on the role of mathematics for building models oscillated between pure Platonism and a recognition that reality is never as simple as the mathematical model. For instance, Birkhoff, admittedly with a hint of irony, wrote that Poincaré's recurrence theorem entailed that:

> within a very large but finite period of time, our article will again appear on this same subject, in this same journal, read by the same individuals, as far as one may discern, and this will happen indefinitely often.[79]

But at the same time, Birkhoff and Lewis concluded their article on the stability of causal systems by expressing a modest goal for mathematical models of the universe:

> No matter how fascinating the purely mathematical study of causal systems may be, it would seem not to be desirable to take them too seriously from a realistic point of view as applicable to the actual universe. The real purpose of physical speculation is to enable us to calculate only within certain prescribed limits of error and for reasonable intervals of time the behavior of physical systems.[80]

Birkhoff however never went as far as Andronov in doubting the possibility of accurately modeling physical systems, and indeed the whole universe, with mathematical concepts. Inspired by the famous "problem of stability" of the Sun-Earth-Moon system, Birkhoff restricted the study of stability to that of orbits lying near a periodic (or central) motion. Concerned with radio systems, Andronov imagined a more general type of stability that applied not only to solutions of a system of differential equations, but to the system itself.

In summary, for contemporaries in the 1930s, it would have been almost impossible to juxtapose Andronov, Birkhoff, Cartwright, and van der Pol, as I just did, or to think of them as belonging to a single discipline. In 1952, Mary Cartwright described

[79] G. D. Birkhoff and D. C. Lewis, "Stability," 332.
[80] G. D. Birkhoff and D. C. Lewis, "Stability," 333.

the discipline she called "nonlinear vibration," as "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"[81] Summarizing his "interdisciplinary" career—before the word even existed—Balthasar Van der Pol, however, could not help voicing regrets at the lack of communication between disciplines:

> In my thirty years experience of research work I have been struck time and time again by the fact that the mathematician speaks a different language from that of the physicist. . . . This difference of languages seems to me to be one of the obstacles standing in the way of mutual understanding and appreciation.[82]

When one refrain from projecting Smale's later synthesis on the past, the remarkable fact in this prehistory of dynamical systems theory becomes, not that communication across disciplinary boundaries proved so difficult, but that it indeed sometimes took place. These few contacts however framed some common basis for the mathematical and philosophical study of stability. The lack of a stable community with clearly defined problems, tools, and social borders was one of the major reasons for the "long neglect" felt by many actors. Many people working on closely related phenomena hardly had a chance to communicate, and this created a sense of isolation. As we have seen, Lefschetz was the one who came the closest to succeeding in creating a dynamic research school, with publications, students, and a charismatic leader.

81 M. L. Cartwright, "Non-Linear Vibrations," 86.
82 B. van der Pol, "Mathematics and Radio," 1140.

4. AS IT GOES WEST, COARSENESS BECOMES STRUCTURAL STABILITY

a) Filling Wholes

Apparently, Solomon Lefschetz—who was born in Russia—was a close friend of Pontrjagin's.[83] They worked on closely related topics, and had high regards for each other's work. While working on the translation of Andronov and collaborators' book, Lefschetz's attention could not failed to be drawn to the concept elaborated in part by his friend.

However, Lefschetz judged that *Theory of Oscillations* needed to be adapted for an American audience. "It became evident quite early that considerable condensation, paring down and editing of the original was unavoidable if its value were not to be lost to the non-Russian reader." Lefschetz therefore eliminated from the text "many lengthy and purely theoretical discussions."[84] In particular, the philosophical introduction, described above, was condensed from 18 pages to merely 2, and much of its content was lost in the process. Without this, we may venture that many of Thom's and Ruelle's ideas might have appeared less novel at the time when they were formulated. In particular, the whole discussion about coarse systems was thrown away. To redress this, Lefschetz included an appendix in his translation, which more or less repeated the terms of the note published by Andronov and Pontrjagin in 1937. Like the original though, it contained no proof.

Lefschetz had a flair for names; he had coined the word "topology" as a "snappy title" for his 1930 monograph on a topic that had until then been called, following

[83] Albert W. Tucker to J. Douglas Brown (September 30, 1959). Princeton Arch.

Poincaré, *analysis situs*.[85] Not to Lefschetz's liking, the term "coarseness" was replaced by "structural stability." He thereby shifted the attention on the property rather than the systems satisfying it. Partly as a consequence, he nearly divorced the mathematical meaning of coarseness (or structural stability) from others of Andronov's concerns. Even if, in two sentences, he drew attention to the physical significance of structural stability, Lefschetz in effect demoted Andronov's idea from a methodological guide for the study of nonlinear systems to a useful, intriguing, but marginal and rather technical mathematical concept.[86]

It is striking to note that the members of Lefschetz's group who worked on structural stability seemed to have come to Princeton with prior personal interest in it. Lefschetz himself scarcely studied it at first. The group as a whole did not devote much of its energy to it.[87] Mainly this neglect was due to the fact that, as Dahan Dalmedico emphasized, the focus of Lefschetz's school slowly evolved from an analytic study of a few cases of concrete nonlinear oscillators, catering to perceived needs of engineering science, toward a more global, and ambitious, program of classification of dynamical

[84] A. A. Andronov et al., *Theory of Oscillations,* vi.
[85] Press Release (October 6, 1972). Princeton Arch. AC#109 Box 39. Solomon Lefschetz, *Topology,* 2nd ed. (New York: Chelsea, 1956). The German word *Topologie* was the title of a book written in 1847 by one of Gauss's students, Johann Benedict Listing. Lefschetz also introduced the term "algebraic topology" instead of "combinatorial topology." A. W. Tucker, *History of Mathematics,* Course II-1962, NSF Institute, mimeographed lecture notes by A. K. Funderburg.
[86] "The physical necessity for this [structural stability] is fairly clear; in physical systems one never knows exactly what the functions *P* and *Q* are and so one will naturally exclude systems which are affected by ever so slight a modification of these functions." A. A. Andronov et al., *Theory of Oscillations,* 337-340.
[87] Cf. S. Lefschetz, "Nonlinear Differential Equations."

systems.[88] In fact, it was the attention placed on the notion of structural stability itself by people coming from outside which impelled its direction to this evolution.

Following the publication of *Theory of Oscillations,* Father Henry DeBaggis, a young professor at Notre Dame, joined the project in 1949 for two years, a reading of the appendix having "awakened" his interest in structural stability. In Lefschetz's plain words, "DeBaggis undertook to establish a complete theory and this objective was attained."[89] This work was facilitated by members of the Project. Without the assistance of Menachem Schiffer and D. C. Spencer, who joined the Project in 1949-50 and 1949-51, respectively "and that of Lefschetz it is safe to say that DeBaggis would never have succeeded in carrying his research successfully."[90]

In any case DeBaggis finally managed to provide all proofs omitted by Andronov and Pontrjagin. He showed that a necessary and sufficient condition for a system defined on a bounded region of the plane to be structurally stable was: the system (1) had at most a finite number of singular points which can only be nodes, foci or saddle points; (2) no separatrix joining saddle points; and (3) at most a finite number of limit cycles.[91]

In plain English, structurally stable systems were *simple.* Their trajectories tended towards equilibrium or periodic solutions; and these were finite in number. Given a

---

88 A. Dahan Dalmedico, "La renaissance des systèmes dynamiques."
89 S. Lefschetz, "Nonlinear Differential Equations," 15-16.
90 S. Lefschetz, "Nonlinear Differential Equations," 12.
91 To make this statement fully acceptable to a mathematician, additional technical conditions should have been imposed. See H. F. DeBaggis, "Dynamical Systems with Stable Structures," *Contributions to the Theory of Nonlinear Oscillations,* 2, ed. S. Lefschetz, Annals of Mathematics Series, 29 (Princeton: Princeton University Press): 37-59, esp. 48. Also S. Lefschetz, *Differential Equations: Geometric Theory* (New York: Interscience, 1957), 239-245; and A. Dahan Dalmedico, "La renaissance des systèmes dynamiques," 147-148.

system of differential equations, one was therefore justified in only looking for these simple solutions; provided, that is, that structurally stable systems were common enough to be of any use. And it was for this purpose only that Andronov's arguments in favor of the physical significance of coarseness now played any role.

"In the study of nonlinear problems it is difficult for the mathematician to find rich classifications of nonlinear systems which are sufficiently homogeneous in their properties to yield an interesting theory."[92] This had always been the central question for the study of nonlinear differential equations. There was no use in studying particular differential equations, except when one had a good reason to. A general method of solution seemed out of reach. And no class of equations seemed to emerge from the mathematical investigation alone. Structurally stable systems appeared to DeBaggis as a class that, resorting to Andronov's arguments, seemed rich enough to be relevant to the mathematical modeling of physical phenomena. Stability requirements "provide a clue to the restrictions a mathematician should place on his nonlinear problems."[93] In DeBaggis's work, this paragraph, certainly inspired by Andronov, played no further role than providing a justification for the mathematical study of structural stability.

b) A Density Theorem by Peixoto

Lefschetz apparently showed great interest for DeBaggis's work. In his 1957 book on the geometric study of differential equations, Lefschetz mentioned it in the introduction.[94]

---

[92] H. F. DeBaggis, "Dynamical Systems," 37.
[93] H. F. DeBaggis, "Dynamical Systems," 37.
[94] S. Lefschetz, *Differential Equations*. See A. Dahan Dalmedico, "La renaissance des systèmes dynamiques," 147.

The same year, he welcomed two mathematicians coming from Brazil, again with a specific interest for structural stability: they were Marilia and Mauricio Peixoto. "A man of rare enthusiasm, and most careful thinker," Lefschetz judged, "Mauricio soon made noteworthy contributions to this most delicate topic."[95] Following Andronov and Pontrjagin, DeBaggis had found necessary and sufficient conditions for a two-dimensional dynamical system to be structurally stable. Lefschetz had termed these "general systems." Tackling the problem by using a fully topological approach, Peixoto went a step further. He proved that "most" dynamical systems on the two-dimensional sphere were structurally stable. In technical terms, he showed the set of all structurally stable systems on two-dimensional manifolds was an open dense subset of the space containing all dynamical systems.[96] In other words, not only structurally stable systems were very common, but on top of this, any system could be approximated by one.[97]

This was a crucial step. Until then, the justification for studying structurally stable systems had been provided by philosophical arguments à la Andronov. It was assumed that structural stability could translate accurately more or less vague assumptions about the physical stability of systems under consideration. With Peixoto's density theorem, rigorous mathematical arguments grounded the belief that every dynamical system (in

[95] S. Lefschetz, " Nonlinear Differential Equations," 21.

[96] A subset A of Ω is said to be *open* if every point in A is surrounded by points belonging to A; it is said to be *dense* in Ω if every point in Ω either belongs to A, or lies arbitrarily close to A.

[97] M. M. Peixoto, "On Structural Stability," *Annals of Mathematics,* 69 (1959), 199-222; "Structural Stability on Two-Dimensional Manifolds," *Boletín de la Sociedad matemática mexicana,* 5 (1960) [Proceedings of the Symposium on Ordinary Differential Equations and their Applications, Universidad nacional autónoma de México, 7-13 September, 1959], 188-189; *Topology,* 1 (1962), 101-120.

two dimensions) could always be approximated by a structurally stable one. Structurally stable systems were thus the only ones susceptible of accurately representing reality, and this for mathematical reasons alone.

It is hard to believe that mathematics alone could dictate what kind of models are to be found in nature. The following example shows plainly that, even after Peixoto's theorem, metaphysical assumptions could not be totally removed from discussions about modeling. For a long time one of the most successful mathematical models, the harmonic oscillator turns out to be *not* structurally stable. It can indeed be approximated by a slightly anharmonic oscillator (with a very small friction term).[98] In the long run, their solutions are however very different since the anharmonic oscillator, no matter how small the friction term, will always tend towards rest. The harmonic oscillator, on the other hand, will oscillate forever. Does this mean that the harmonic oscillator is useless for the mathematical modeling of reality? No, it just means that it represents an imperfect idealization of reality, a fact that was long known. Moreover, when symmetry considerations impose that energy is conserved (think of quantum field theory), the harmonic oscillator might even be an exact representation of reality.

To express his density theorem, Peixoto introduced a new word into the theory of differential equations: structurally stable systems were "generic," he wrote in 1962 (with quotation marks).[99] It was through René Thom, who had picked it up in the mid-1950s for

[98] The anharmonic oscillator system can be written as follows: $dy/dt = \gamma y - x; \quad dx/dt = y$. The harmonic oscillator has the same form without the γ-term. In the C^1 topology used by Peixoto, the distance between the two systems tends to zero as γ does.
[99] M. Peixoto, "Structural Stability," *Topology,* 101.

his study of singularities, that Peixoto became familiar with the term.[100] The idea of using a similar concept in the theory of differential equations however went back to Poincaré (he studied the considerably different case of solutions that have probability 1 of happening, excluding exceptional trajectories from consideration), and had been picked up more or less rigorously by Hadamard, Birkhoff, Cartwright-Littlewood, and Eberhard Hopf.[101] Genericity was however a tricky concept to use, and often was the cause of much confusion.[102]

In his final report to the ONR, Solomon Lefschetz lauded Mauricio Peixoto's work in the following terms:

> Especially noteworthy is his introduction of a metric space *S* of differential equations . . . and showing that under a suitable definition one may consider the structural[ly stable] systems as dense in *S*. . . .The work of Mauricio Peixoto during his stay with the Project, his ebullient and enthusiastic attitude were so outstanding that when . . . RIAS was organized in the Fall of 1957, he was asked to join it for the following year (1958-59).[103]

---

[100] See R. Thom, "Les singularités des applications différentiables," *Séminaire Bourbaki,* 7, exposé #134 (May 1956), "Un lemme sur les applications différentiables," *Boletín de la Sociedad matemática mexicana,* 2nd ser., 1 (1956): 59-71, 59-60.
[101] On Poincaré's probabilist concepts, see A. Dahan Dalmedico, "Le difficile héritage," 17; J.-L. Chabert et A. Dahan Dalmedico, "Les idées nouvelles," 296-303; and M. W. Hirsch, "The Dynamical Systems Approach," 21. See G. D. Birkhoff, *Dynamical Systems,* 197; M. L. Cartwright and J. E. Littlewood, "On Non-Linear Differential Equations," 182n; E. Hopf, "A Mathematical Example Displaying Features of Turbulence," *Communications on Applied Mathematics,* 1 (1948): 303-322, 305.
[102] See M. W. Hirsch, "The Dynamical Systems Approach," 35-36. Also see A. Weil, "Correspondence," *American Journal of Mathematics,* 79 (1957): 951-952. Written in Italian, this anonymous letter, which shows using an argument of Thom's that abusive use of genericity could lead classical Italian algebraic geometers to erroneous results, was attributed to Weil by René Thom.
[103] S. Lefschetz, " Nonlinear Differential Equations," 21-22. For a more technical discussion of the work of DeBaggis and Peixoto about structural stability, see again A. Dahan Dalmedico, "La renaissance des systèmes dynamiques," 145-148.

Starting with Andronov and Pontrjagin's article, coarseness and later structural stability had been restricted to dynamical systems with two variables. Besides simplicity, a good reason for this limitation was that these systems represented second order differential equations: the most useful ones in dynamics. Nothing in the definition provided in 1937 forbade an extension to higher dimensions. By introducing such a definition, Peixoto thus opened up vast uncharted territories. Under Stephen Smale's lead, the program of classifying structurally stable systems in *n* dimensions would provide an important incentive for studying dynamical systems in the years to come. In the process, structural stability would, for the first time, reach a wide audience.

5. SMALE'S 'BAD' CONJECTURE AND THE HORSESHOE: 'AN ADMIRABLE BATTLE'

"Smale made a bad conjecture." Thus does James Gleick begin his description of Stephen Smale's work which would lead him to forge his famous horseshoe.[104] 'Bad' is here a bad choice of word. Smale's conjecture was indeed shown (by himself!) to be faulty. But in a way it was as successful a conjecture as can be. It was the logical follow-up of decades of research on structural stability, and furthermore it led to an unprecedent boom in the study of dynamical systems. One of Smale's students, Bob Williams, described the benefits of his mentor's audacity: "he's brave enough to make the conjectures, so we got to play with them."[105]

[104] J. Gleick, *Chaos,* 45.
[105] R. F. Williams in *From Topology to Computation,* 179.

In charge of presenting Smale's work to the 1966 International Congress of Mathematicians at Moscow, René Thom emphasized his special ability of suggesting fruitful directions for mathematical research:

> If Smale's works perhaps do not possess the formal perfection of definitive work, it is because Smale is a pioneer who take risks with a tranquil courage; in a completely unexplored domain, in a mathematical jungle of inextricable wealth, he is the first to have shown the way and placed the first milestones.[106]

Late in the summer of 1958, at Princeton, Peixoto met Smale. And the latter, a topologist, showed some interest for Peixoto's work on structural stability. "I was delighted to see this interest," remembered Peixoto; "at that time, hardly anybody besides Lefschetz cared about structural stability."[107] Peixoto was hitting on a problem. Having generalized the definition of structural stability to higher dimensions, he was looking for an equivalent to DeBaggis's theorem in *n* dimensions. In particular, condition (2) above (see p. 276), which stated that no separatrix ran between two saddle points (the "no saddle-connection condition"), was not obvious to transpose. Smale, using a notion introduced by Thom, found a solution to this problem.

a) The Topologists' Hand

Born in 1932 in Flint, Michigan, Steve Smale received his Ph.D. in 1956 in "a new branch of mathematics called topology" with Raoul Bott at the University of Michigan.[108] That

---

[106] R. Thom, "Sur les travaux de Stephen Smale," *Proceedings of the International Congress of Mathematicians (Moscow, 1966):* 25-28, 28.
[107] M. M. Peixoto, "Some Recollections of the Early Work of Steve Smale," in *From Topology to Computation: Proceedings of the Smalefest,* ed. M. W. Hirsch et al. (New York: Springer, 1993): 73-75, 73.
[108] S. Smale, "Chaos: Finding a Horseshoe on the Beaches of Rio," 2. Written for a meeting in Rio de Janeiro, in March 1996, celebrating the 45th anniversary of the National Research Council of Brazil (CNPq), this article was posted on the Web by

summer, Smale went to a topology symposium at Mexico City, where he met Thom and two graduate students from the University of Chicago Morris W. Hirsch and Elon Lima.[109] These encounters decisively shaped Smale's later involvement with dynamical systems.

The next fall at Chicago, where Smale got his first teaching position, Thom lectured on transversality theory, which generalized the notion of secant for manifolds and topological spaces. Three years later, Smale would use the notion of transversal intersection in order to solve the problem that stopped Peixoto.[110] Moe Hirsch became Smale's first, though "informal," student and, later, his colleague at Berkeley in the

---

Smale himself. I do not know if it has been published. The information for this section was provided by this article and S. Smale, "On How I Got Started in Dynamical Systems (1959-1962)," (partly based on a talk given at a Berkeley seminar circa 1976), in *Mathematics of Time* (New York: Springer, 1980) [hereafter *MT*]: 147-151; "The Story of the Higher Dimensional Poincaré Conjecture (What Actually Happened on the Beaches of Rio)," *The Mathematical Intelligencer,* 12(2) (1990): 44-51; both repr. in *From Topology to Computation,* ed. M. W. Hirsch et al.: 22-26 and 27-40; and Smale's interview in *More Mathematical People: Contemporary Conversations,* ed. D. J. Albers, G. L. Alexanderson, and C. Reid (Boston: Harcourt Brace Jovanovich, 1990): 305-323. See also J. Palis, "On the Contribution of Smale to Dynamical Systems," in *From Topology to Computation,* 165-178.

109 S. Smale, "On How I Got Started," 147.

110 Cf. J. Palis, "On the Contribution of Smale," 166. In technical terms, the stable and unstable manifolds at limit sets (fixed points or limit cycles) are defined as the sets of points that tend towards the limit sets as t goes to, respectively, $+\infty$ or $-\infty$; Smale's condition was that the stable and unstable manifolds intersect transversally. For definitions, see S. Smale, "Morse Inequalities for a Dynamical Systems," *Bulletin of the American Mathematical Society,* 66 (1960): 43-49, 46-47; and Earl A. Coddington and Norman Levinson, *Theory of Ordinary Differential Equations* (New York: McGraw Hill, 1955), 330-333. About Thom's lecturing at Chicago, see S. Smale, "How I Got Started," 148, and "The Story of the Poincaré Conjecture," 29. References for transversality: see R. H. Abraham, "Transversality in Manifolds of Mappings," *Bulletin of the American Mathematical Society,* 69 (1963): 470-474; R. H. Abraham and J. Robbin, *Transveral Mappings and Flow* (New York: Benjamin, 1967); and the excellent book by V. Guillemin and A. Pollack, *Differential Topology* (Englewood Cliffs: Prentice Hall, 1974), Chap. 2.

second half of the 1960s at the time when they developed dynamical systems theory with a whole new generation of students. Regarding their exceptional complementarity, John R. Stallings once wrote: "Smale is the Mad Genius and Hirsch is the Hard Worker."[111] Finally Elon Lima, a student from Brazil, was responsible for introducing Mauricio Peixoto to Steve Smale, when the latter moved to the Institute for Advanced Study at Princeton with a two-year NSF postdoctoral fellowship late in the summer of 1958.

Why was Smale interested at all in Peixoto's work on structural stability? His domain of expertise then hardly overlapped with the field of mathematics he came to. In fact, Smale recounted retrospectively, he at once saw that topology could prove a first class tool for this study. "I was immediately enthusiastic," he wrote, "not only about what he [Peixoto] was doing but with the possibility that, *using my topology background,* I could extend his work to *n* dimensions." [112] The involvement of renown topologists confirmed his feeling. "I believe that it was the topologist's, Pontryagin and Lefschetz, hand in the subject that contributed to the fact that I was ready to listen to Mauricio."[113]

In any case, as a offshoot of the contact he had with Peixoto, Smale wrote two papers in 1959, in which he made his famous and bold conjecture. Smale suggested that the equivalent in more than two dimensions of Lefschetz's general systems—those used by DeBaggis—was a necessary and sufficient conditions for structural stability. Thom

---

[111] Quoted in S. Smale, "The Story of the Poincaré Conjecture," 34. Hirsch is called Smale's "informal" student by J. Palis, "On the Contribution of Smale," 175.
[112] S. Smale, "On How I Got Started," 148. My emphasis.
[113] S. Smale, "Chaos," 13. There is a bit of black humor in this statement since in 1907 Lefschetz lost his two hands in a factory accident, a tragic accident that determined his decision of becoming a mathematician instead of an engineer. See A. Dahan Dalmedico, "La renaissance des systèmes dynamiques," 133-134.

later called this class of dynamical systems "Morse-Smale systems."[114] Smale's conjecture had two parts:

> (A) It seems at least plausible that [Morse-Smale systems] form an open dense set in the space . . . of all vector fields. . . .
>
> (B) It seems likely that the conditions [for a system to be Morse-Smale] are necessary and sufficient conditions for [the system] to be structurally stable in the sense of Andronov and Pontrjagin.[115]

We therefore see that Smale's celebrated conjecture actually was a pair of conjectures. Proposition (A) generalized Peixoto's density theorem, while (B) extended DeBaggis's theorem, to higher dimensions. Together they implied that most dynamical systems were structurally stable. To sense how bold it was to suggest this, let us note, as Peixoto did in 1962, that it was not known, at the time, whether on any n-dimensional manifold, structurally stable systems even existed.[116]

Armed with Morse-Smale systems, "Smale began an admirable battle to have a global description (if only conjecturally) of 'most' of the world of dynamics, still hoping that the stable systems formed an open dense subset of it."[117] About fifteen years later, Smale acknowledged that he "was extremely naive about ordinary differential equations at that time and was also extremely presumptuous." His "overenthusiasm" had led him to suggest that Morse-Smale "systems were almost all (an open dense set) of ordinary

[114] See S. Smale, "How I Got Started,"148. See Jacob Palis, "On Morse-Smale Dynamical Systems," *Topology,* 8 (1969): 385-405. Morse-Smale systems therefore consisted of the hyperbolic dynamical systems which had only a finite number of fixed points and closed orbits as their limit sets (in this condition, limit sets were later replaced by nonwandering sets; see J. Palis, "On the Contribution of Smale," 167), and whose stable and unstable manifolds at the limit sets intersected transversally.
[115] S. Smale, "Morse Inequalities," 43.
[116] M. M. Peixoto, "Structural Stability," *Topology,* 101.
[117] J. Palis, "On the Contribution of Smale," 170.

differential equations!"[118] In 1996—ten years after the publication and popular success of Gleick's book—Smale stated this conjecture as "chaos does not exist!"[119] Smale had however been warned not to be so bold.

> Peixoto told me that he had met Pontryagin, who said that he didn't believe in structural stability in dimensions greater than two, but that only increased the challenge. . . . If I had been at all familiar with the literature (Poincaré, Birkhoff, Cartwright-Littlewood), I would have seen how crazy this idea was.[120]

b) 'My Best-Known Work Was Done on the Beaches of Rio'[121]

In September 1959, Steve Smale presented his conjecture at the Symposium on Ordinary Differential Equations and their Applications in Mexico City. At this international conference—one of the first convened by Lefschetz's group after the end of the Project—many specialists who had at one time or another come to Princeton as part of the Project attended. But newcomers and outsiders were also present: René Thom, Georges Reeb, and, of course, Steve Smale.[122] Considering the exposure it received, it is therefore somewhat surprising that Smale's false conjecture was greeted with a certain success in the mathematical community he was addressing.

In December 1959, invited by Peixoto and Lima, Smale left for the Instituto de matemática pura e aplicada (IMPA) in Rio de Janeiro, Brazil. Shortly after his arrival, he received a letter from MIT mathematician Norman Levinson. As Smale recalled,

---

[118] S. Smale, "On How I Got Started," 148.
[119] S. Smale, "Chaos," 4.
[120] S. Smale, "On How I Got Started," 148.
[121] Smale to Connick, Vice-Chancellor of Academic Affairs at the University of California, Berkeley, unpublished, but quoted in Daniel S. Greenberg, "The Smale case: NSF and Berkeley Pass through a Case of Jitters," *Science,* 154 (October 7, 1966): 130-133; and S. Smale, "the Story of the Poincaré Conjecture," 39.

Levinson "had coauthored the main graduate textbook in ordinary differential equations [which Smale had cited]. He was a scientist to be taken seriously."[123] According to Smale, Levinson's unpublished letter informed him that one could not expect Morse-Smale systems to occur so generally, and that one of his own papers already contained a counterexample for conjecture (B) above.

(i) Ancestors of the Horseshoe

Like Cartwright and Littlewood, Levinson participated in the effort spurred by World War II by studying the van der Pol and Liénard equations. Building on their work, Levinson studied in 1948 the solutions of the Liénard equation with forcing:

$$y'' + p(y)y' + y = c \sin t;$$

where y' and y'' represented first and second derivatives with respect to t. Among the solutions of the forced Liénard equation, Levinson showed that a family F exhibited a "remarkably singular structure."[124] Levinson emphasized that, contrary to relaxation

---

[122] Proceedings were published in the *Boletín de la Sociedad matemática mexicana,* 5 (1960).

[123] S. Smale, "Chaos," 4. See also S. Smale, "On How I Got Started," 149. Smale is referring to E. A. Coddington and N. Levinson, *Theory of Ordinary Differential Equations.* Note furthermore that Earl Coddington acted as deputy-director of Lefschetz's Project from September 1957 to September 1958, thus just barely overlapping with Smale at Princeton. On Coddington's role in the Project, see S. Lefschetz, "Nonlinear Differential Equations," 12.

[124] Norman Levinson, "A second Order Differential Equation with Singular Solutions," *Annals of Mathematics,* 50 (1949): 127-153, 153. For his previous work in the field, see N. Levinson, and O. Smith, "A General Equation for Relaxation Oscillations," *Duke Mathematical Journal,* 9 (1942): 382-403; and N. Levinson, "Transformation Theory of Non-Linear Differential Equations of the Second Order," *Annals of Mathematics,* 45 (1944): 723-737, where he suggested that a "bad" curve of Birkhoff's type could possibly emerge from the forced van der Pol equation (p. 736).

oscillations studied by van der Pol and his followers, "most of the solutions of F are certainly *not* periodic."

As Levinson noted, similar behavior, which they qualified as "very bizarre," had already been observed by Cartwright and Littlewood just a few years before.[125] They had exhibited an infinite set of "non-periodic trajectories, of the type described as 'discontinuous recurrent [motion]'."[126] This was how Birkhoff had described recurrent motions that seemed not to be of such a trivial type as steady or periodic motions. In phase space, such discontinuous recurrent motion defined, in the notation adopted by both Cartwright-Littlewood and Levinson, a set K_0, which was connected, of measure zero (zero area), and which separated the plane in two open subsets, a bounded and an unbounded one. Moreover all motions in a neighborhood tended towards K_0 as t went to infinity. But since it had different rotation numbers for limit points of interior or exterior points, K_0 could not be a simple Jordan curve. "In fact," Ralph Abraham noted much later, such curves "are fractals."[127] In the terminology of Ruelle and Takens, they were *strange attractors* (Chapter VII).

When Cartwright and Littlewood hit upon such strange sets, which they dared not call "curves," they looked for comfort in the literature. As they wrote in their article, "our

---

[125] M. L. Cartwright and J. E. Littlewood, "On Non-Linear Differential Equations," 182.
[126] M. L. Cartwright and J. E. Littlewood, "On Non-Linear Differential Equations," 183; for a description of discontinuous recurrent motions, see G. D. Birkhoff, "Surface Transformations and Their Dynamical Applications," *Acta Mathematica,* 43 (1922): 1-119, chap. 5; repr. *Papers,* 2: 111-229. See also M. Morse, "Recurrent Geodesics on a Surface of negative curvature," *Transactions of the American Mathematical Society,* 22 (1921): 84-100.
[127] R. H. Abraham, "In Pursuit of Birkhoff's Chaotic Attractor," in *Singularities and Dynamical Systems,* ed. S. N. Pnevmatikos (Amsterdam: North-Holland, 1985): 303-312, 303.

faith in our results was one time sustained only by the experimental evidence" provided by van der Pol and van der Mark.[128] Hearing through headphones the noise produced by the frequencies corresponding, or so they wanted to believe, to periodic solutions of the van der Pol equation, they made the following observation:

> Often an irregular noise is heard in the telephone receivers before the frequency jumps to the next lower value. However, this is a subsidiary phenomenon, *the main effect being the regular frequency multiplication.*[129]

The irregular noise heard by the Dutch experimenters might have been the mark of a continuous spectrum, which were later interpreted as revealing the presence of strange attractors. Let me remark that more than sixty years later, Ruelle suggested that chaotic attractors should be easy to observe experimentally in oscillating electric circuits.

> It should be possible to visualize the transition to continuous spectrum. . . . Alternatively, if frequencies are in the audible range, the transition to continuous spectrum should correspond to a change in the musical nature of the corresponding sound. These experiments . . . have not yet been performed as far as I know. Since they are easy, I strongly suggest that they should be attempted.[130]

In their search for previous observation of strange sets, Cartwright and Littlewood also found comfort in Birkhoff's work. In 1932, in the words of Abraham, "Birkhoff published a remarkable paper on *remarkable curves.*"[131] Such curves arose in the study of mappings from an annulus to itself considered in Poincaré's last geometric theorem. Birkhoff acknowledged that he was at first surprised by the existence of such curves,

---

[128] M. L. Cartwright and J. E. Littlewood, "On Non-Linear Differential Equations," 182n.
[129] B. van der Pol and J. van der Mark, "Frequency Demultiplication," *Nature,* 120 (1927): 363-364. My emphasis.
[130] D. Ruelle, "Sensitive Dependence on Initial Conditions and Turbulent Behavior of Dynamical Systems," *Annals of the New York Academy of Sciences,* 316 (1978): 408-416; repr. *TSAC,* 175-184, 182. Ruelle's claim was not false since, van der Pol and van der Mark's experimental observations did not correspond exactly to what he suggested.
[131] R. H. Abraham, "In Pursuit," 303.

having for a while exploited them for a "*reductio ad absurdum*" that failed.[132] In 1935, Marie Carpentier was able to construct explicitly such a strange curve.[133] Cartwright and Littlewood suggested that their K_0 was an example of "bad" topological behavior comparable to Birkhoff's remarkable curves. But Birkhoff's and Carpentier's curves were invariant sets of an analytic mapping from the plane to the plane, *not* dynamical recurrent sets.

Using the technique of the Poincaré map, Levinson (and Cartwright and Littlewood before him) defined a transformation associated with the differential equations as such: given (y, v), where $v = y'$, and $y(t)$ is a solution of the forced Liénard equation Levinson was considering, he defined a transformation T from the plane to the plane by $T(y,v) = (y_1, v_1)$, where $y_1 = y(t+2\pi)$ and $v_1 = y'(t+2\pi)$.[134] By looking at the invariant set of T, Levinson showed what Cartwright and Littlewood had only hinted at, namely that Birkhoff's "curves" (with quotation marks) "could arise from the transformation associated with a differential equation [which previously] was not known."[135] This was the transformation that Smale would have to translate into his own geometrical way of thinking before he would come up with the horseshoe.

---

132 G. D. Birkhoff, "Sur quelques courbes fermées remarquables," *Bulletin de la Société mathematique de France,* 60 (1932): 1-26; repr. *Collected Papers,* 2: 418-443, 443.
133 M. Carpentier, "Sur les courbes fermées analogues aux courbes de Birkhoff," *Journal de mathématiques pures et appliquées,* 9th ser., 14 (1935): 1-48.
134 Poincaré's "first return map," which allowed to replace the study of orbits of a differential equation by that of discrete recurrences $x_{n+1} = f(x_n)$ defined on a lower dimensional surface, is discussed in J.-L. Chabert and A. Dahan Dalmedico, "Les idées nouvelles," 292-295.
135 N. Levinson, "A Second Order Differential Equation," 129.

A striking aspect of Levinson's, as well as Cartwright and Littlewood's, analyses consisted in the realization that the behavior they observed was robust. The strange "curves" arose, not for exceptional, isolated values of the parameters in the equations, but for some continuum; they could not be perturbed away. And this is exactly what Smale reported that Levinson had written him. A counterexample to one of Smale's conjectures (conjecture (A)) could be extracted from these papers.

(ii) Smale's Geometric Translation of Levinson: The Horseshoe

With modesty, Steve Smale often emphasized that his horseshoe merely was "a natural consequence of a geometrical way of looking at the equations of Cartwright-Littlewood and Levinson", "an abstract geometrization of what [they] had found more analytically before."[136] To destroy conjecture (A), Smale had to come up with a system (and an open set of systems around it) whose limit set contained either an infinite number of the trivial limit sets he had admitted (fixed points or periodic cycles) or a nontrivial set that was neither a point nor a cycle.

"Still partly with disbelief, I spent a lot of time studying [Levinson's) paper," Smale recalled.[137] He had a hard time trying to "translate Levinson's analytic argument into my own geometric way of thinking." Eventually, he became convinced that Levinson was correct and that his conjecture was wrong. "I had guessed wrongly. But while

---

[136] S. Smale, "Chaos," 5; "On How I Got Started," 149. Already in 1964, Smale acknowledged Levinson's letter: see S. Smale, "Diffeomorphisms with Many Periodic Points," *Differential and Combinatorial Topology: A Symposium in Honor of Marston Morse,* ed. S. S. Cairns (Princeton: Princeton University Press, 1965): 63-80, 64.
[137] S. Smale, "On How I Got Started," 149.

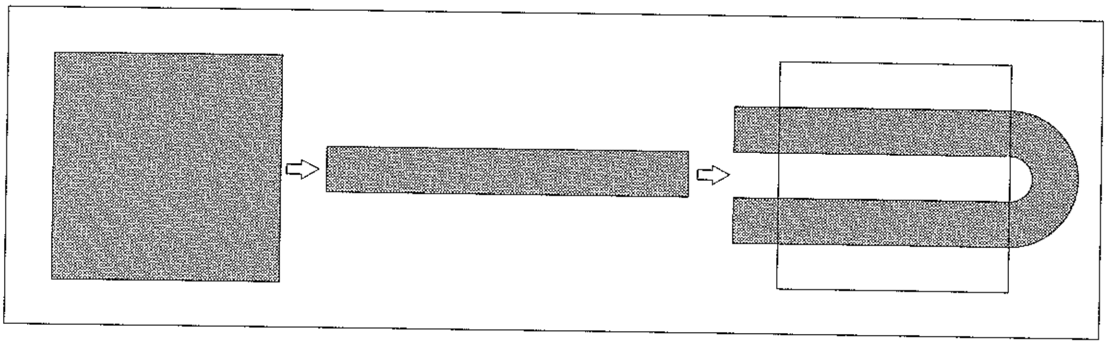

Figure 10: Smale's Horseshoe. By repeating this transformation an infinite number of times, one get a recurrent set whose section has the structure of a Cantor set.

learning that, I discovered the horseshoe!"[138] Since the horseshoe has very often been described, I refrain to do this once again.[139] Let me just say that this nonlinear mapping of the plane to the plane possessed an infinite number of periodic points, as well as nonperiodic sequences (Fig. 10).

But I do want to emphasize a few consequences of Smale's work with the horseshoe. With this counterexample, Smale showed not only that there existed diffeomorphisms with an infinite number of periodic points, but also that slight perturbations of the horseshoe exhibited the same property. For him, this showed that dynamical systems could not be generically so simple as he expected: there existed other types of limit sets, besides points and cycles. This infinite number of periodic points formed a structure similar to that of a Cantor set. This was quite complicated behavior. But was this mapping really dynamics?

---

[138] S. Smale, "Chaos," 5.

[139] The original description is to be found, but in an abstract form very hard to recognize, in S. Smale, "Diffeomorphisms with Many Periodic Points," 74. For an excellent discussion of the horseshoe which is quite readable, see F. Diacu and P. Holmes, *Celestial*

In Smale's earlier work, dynamical systems remained equivalent to differential equations. If, since Poincaré, it had been possible to associate a mapping with them, the inverse process—that is, associate a differential equation to a mapping—was not obvious. Smale focused on Levinson's mapping, thus making an important conceptual shift from differential equations to mappings, homeomorphisms and diffeomorphisms, without extracting them from any system of differential equations.[140]

In fact, Smale subsumed the study of ordinary differential equations $dx/dt = f(x, dx/dt)$, or as Birkhoff had said, dynamical systems, under the study of diffeomorphisms. Smale regrouped this program under the heading of "dynamical systems," which thereby acquired a new, wider meaning. In 1962, Smale justified his reasons for doing this as such:

> It appears that usually a qualitative problem in differential equations has an analogue in the conjugacy problem [for diffeomorphisms]. This analogue is a little simpler than the original, and if solved, its solution seems to give a way of doing the original problem.[141]

He announced that he would provide details on how to do this in a following publication, "Stable manifolds for differential equations and diffeomorphisms," which was never published. Smale however supplied some clues in his famous paper "Differentiable Dynamical Systems" published in the *Bulletin of the American Mathematical Society* in 1967. An important point however remained: granted that they might be more complicated than previously conjectured by Smale, were structurally

Encounters, chap. 2; and I. Ekeland, *Mathematics and the Unexpected* (Chicago: University of Chicago Press, 1988), 70-73.

[140] Homeomorphisms are continuous mappings; diffeomorphisms are differentiable homeomorphisms with a differentiable inverse.

[141] S. Smale, "Dynamical Systems and the Topological Conjugacy Problem," 490.

stable systems dense? Could any differential system be approximated by a structurally stable one?

c) 'An Unfinished Painting with Several Superposed Sketches'

Mathematically speaking, but also on a political level, Steve Smale was a busy man in the 1960s. This might explain why, his enthusiasm for the topic notwithstanding, he worked on dynamical systems only intermittently in the few years following his stay at Rio. Nonetheless, starting from his interest in structural stability, Smale would, before the end of the decade, synthesize in an unexpected manner several older traditions, destroy hopes for genericity of structural stability, set new terms for a rich research program in dynamical systems, and establish his own school at Berkeley in later part of the 1960s. This school produced most of the workers that would shape the field in the following decades.

(i) Poincaré Again: The Homoclinic Tangle

Still at Rio, having worked out the horseshoe, Smale found in Poincaré's work an analogue of this complicated dynamic situation. "I learned about homoclinic points and Poincaré's work," he recalled in 1996, "from browsing in Birkhoff's collected works which I found in IMPA's library." 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.[142]

It was not quite true that these ideas had been totally forgotten. At about the same time, renewed interest in the theory of integrable Hamiltonian systems, such as those involved in celestial mechanics, started to be felt. Starting from a famous talk of Andrei Nikolaevich Kolmogorov (1903-1987) at the Amsterdam International Congress of Mathematicians in September 1954, KAM (Kolmogorov-Arnol'd-Möser) theory was developed, which as we have seen exploited Birkhoff's work.[143] The question Kolmogorov asked was what happened to the solutions of a conservative system when the equations were slightly perturbed. This should remind us Andronov's concerns with coarse systems.

In the course of the following decade, work inspired by Kolmogorov's theorem turned Hamiltonian dynamics, from the "hopelessly obsolete, outmoded and purely

---

142 S. Smale, "Chaos," 12. For a survey of the results of Poincaré and his followers concerning homoclinic points, see Karl Gustav Anderson, "Poincaré's Discovery of Homoclinic Points," *Archives for History of Exact Science,* 48 (1994): 133-147; June Barrow-Green, *Poincaré and the Three-Body Problem* (Providence: American Mathematical Society, 1997); and F. Diacu and P. Holmes, *Celestial Encounters.*
143 Again, for an easy-going introduction, and an unsophisticated historical discussion of, KAM-Theory, I refer the reader to F. Diacu and P. Holmes, *Celestial Encounters,* ch. 5. See also the original articles: A. N. Kolmogorov, "On Conservation of Conditionally-Periodic Motions for a Small Change in Hamilton's Function" (in Russian), *Doklady Akademi Nauk SSSR,* 98 (1954): 525; transl. in *Selected Works of A. N. Kolmogorov,* ed. V. M. Tikhomirov, 1 (Dordrecht: Kluwer, 1991): 349-354 and *Chaos II,* 107-112; V. I. Arnol'd, "Small Denominators II: Proof of a Theorem of A. N. Kolmogorov on the Preservation of Conditionally-Periodic Motions under a Small Perturbation of the Hamiltonian," *Russian Mathematical Surveys,* 18(5) (1963): 13-40; Jürgen Möser, "On Invariant Curves of Area-Preserving Mappings of an Annulus," *Nachrichten der Akademie der Wissenschaften in Göttingen,* 1 (1962): 1-20. A. N. Kolmogorov's 1954 address was published (in Russian) as "Théorie générale des systèmes dynamiques et mécanique classique," *Proceedings of the International Congress of Mathematicians,*

formal branch of analytical mechanics" it once was, into "a fashionable branch of mathematics."[144] In due time, this work "appeared as the most important breakthrough in the subject [celestial mechanics] since the fundamental difficulties were first recognized by Poincaré in 1892."[145] Among the scientists whose numerical work was triggered by KAM, we may count some who would extract from simple systems extremely complicated behavior that would play crucial roles in the emergence of chaos, in particular astronomer Michel Hénon, and then physicist Joseph Ford.[146]

It was true, however, that a trench had slowly been dug between the study of conservative and dissipative systems. Smale's introduction of homoclinic points in the study of general (dissipative) systems helped a rapprochement between these fields of study, but did not close the gap. Especially with the concept of attractor soon becaming central to the study of dynamical systems, much emphasis remained placed on dissipative systems.

Considering Smale's interest in purely topological matters, on the one hand, and his pursuit of complicated structures in dynamical systems, on the other, it is hardly surprising that he could not help be attracted towards some of Poincaré's old results in the

Amsterdam, 1954, 1 (Amsterdam: North-Holland, 1957): 315-333; transl. in *Selected Works,* 1: 355-374.

144 V. I. Arnol'd, "On A. N. Kolmogorov," *Golden Years of Moscow Mathematics,* ed. S. Zdravkovska and P. L. Duren (Providence: AMS, 1993): 129-153, 132.

145 H. K. Moffatt, "KAM-Theory," *Bulletin of the London Mathematical Society,* 22 (1990): 71-73.

146 M. Hénon and C. Heiles, "The Applicability of the Third Integral Of the Motion: Some Numerical Experiments," *Astronomical Journal,* 69 (1964): 73-79. J. Ford and G. H. Lunford, "Stochastic Behavior of Resonant Nearly Linear Oscillator Systems as the Nonlinear Coupling Approaches Zero," *Physical Review,* A1 (1970): 59-70; "On the Stability of Periodic Orbits for Nonlinear Oscillators in Region Exhibiting Stochastic Behavior," *Journal of Mathematical Physics,* 13 (1972): 700-703.

later field. Indeed, working on the structural stability of gradient systems—those later used preferably by Thom—Smale "noticed how dynamics led to a new way . . . to attack the Poincaré conjecture, . . . and before long all my work focused on that problem."[147] Smale had been introduced to this problem as early as 1955. Not without difficulty, he proved the Poincaré conjecture in dimensions greater than five, which in 1966 "probably warrant[ed] his presence" at the Moscow Congress where he was awarded the Fields Medal.[148] It is to be noted that the proof of higher dimensional Poincaré conjecture became for a while a matter of priority quarrels between him and others, including British topologist E. C. Zeeman who would play a first rank role in the later development of qualitative dynamics.[149]

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.[150] This example together with the horseshoe therefore comforted Smale in giving away all hope for the genericity of Morse-Smale systems. In his 1962 address at the International Congress of Mathematicians in Stockholm, Smale emphasized homoclinic points, rather than the horseshoe, both of which he then saw as intimately linked. Homoclinic points provided him an example for his theorem G, which stated that there existed structurally stable systems with an infinite number of periodic points (and minimal set homeomorphic to a Cantor set). On this occasion, Smale quoted (in French) Poincaré's dramatic description of homoclinic points:

---

[147] S. Smale, "The Story of the Poincaré Conjecture," 32. Smale wrote a paper "On Gradient Dynamical Systems," *Annals of Mathematics,* 74 (1961): 199-206.
[148] R. Thom, "Sur les travaux de Stephen Smale," 25.
[149] See S. Smale, "The Story of the Poincaré Conjecture."

> 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.[151]

By then, Smale had encountered still another example of complex dynamics. Witnesses are unanimous in saying that it was Thom who, in 1960, started asking whether the following diffeomorphism from the torus to itself was structurally stable: $x \to 2x+y \pmod{2\pi}$; and $y = x+y \pmod{2\pi}$.[152] This map possessed a dense set of periodic orbits. Moreover a dynamical system could be constructed starting from it, which shared all these properties.[153] The flow obtained by this system was similar to the geodesics on negative curvature surfaces, studied by Hadamard, and taken up by Birkhoff, Morse, and E. Hopf.[154] But was it stable?

(ii) A Russian Encounter

The horseshoe was first presented in internal seminars at the University of California, Berkeley, where Smale had accepted a job for July 1960. He also spoke at a conference on ordinary differential equations in Colorado Springs during the summer of 1961 (before

---

150 M. W. Hirsch, "The Dynamical Systems Approach," 36-38.
151 Henri Poincaré, *Méthodes nouvelles de la mécanique céleste,* 3 (Paris: Gauthier-Villars, 1899), 389. Quoted and transl. in June Barrow-Green, *Poincaré and the Three-Body Problem,* 162. Only the last two sentences were quoted by S. Smale, "Dynamical Systems and the Topological Conjugacy Problem," 494.
152 M. W. Hirsch, "The Dynamical Systems Approach," 38. J. Palis, "On the Contribution of Smale," 170-171. This map, known as "Arnol'd's cat" is discussed in V. I. Arnol'd and A. Avez, *Problèmes ergodiques de la mécanique classique* (Paris: Gauthier-Villars, 1967), 7-8 and 45-58, and I. Ekeland, *Mathematics and the Unexpected,* 50-59.
153 V. I. Arnol'd and A. Avez, *Problèmes ergodiques,* 48-50.
154 See J.-L. Chabert, "Hadamard," 329-330.

he moved to Columbia were he taught until 1964), and then in September 1961, at a meeting on nonlinear oscillations in Kiev, Ukraine, where decisive encounters took place.

In Kiev, Smale presented the horseshoe as "the first structurally stable dynamical system with an infinite number of periodic solutions."[155] He also asked whether the flow obtained from Thom's automorphism of the torus was structurally stable, conjecturing it was. In USSR, Smale met Kolmogorov and, "in his words, an extraordinarily gifted group of mathematicians: Anosov, Arnol'd, Novikov and Sinai."[156] The following year, at the Stockholm Congress, Smale was now convinced that this flow provided him with a first example of dynamical system in the form of a differential equation which was structurally stable, but not Morse-Smale. He then learned that Arnol'd and Sinai had proved the conjecture and published their result.[157] Moreover, Anosov had introduced a vast class of structurally stable flow with complicated dynamics later known as Anosov flows.[158] The first part of Smale's conjecture was definitely crushed by the horseshoe, homoclinic points, and Thom's torus map.

---

[155] S. Smale, "On How I Got Started," 150. Horseshoe published in: S. Smale, "A structurally stable differential homeomorphism with an infinite number of periodic solutions," *Report on the Symposium on non-linear Oscillations* (Kiev Mathematical Institute, 1961): 365-366 (which I have never found); "Structurally Stable Systems are not Dense," *American Journal of Mathematics,* 88 (1966), 491-496; "Differential Dynamical Systems," *Bulletin of the American Mathematical Society,* 73 (1967), 747-817; repr., with additional notes and references, in *MT,* 1-82

[156] J. Palis, "On the Contribution of Smale," 170.

[157] V. I. Arnol'd and Ya. G. Sinai, "Small Perturbations of the Automorphisms of the Torus," *Soviet Mathematics,* 3 (1962): 783-787; 4: 560.

[158] See V. I. Arnol'd and A. Avez, *Problèmes ergodiques,* ch. 3. D. V. Anosov, "Roughness [i.e. Coarseness] of Geodesic Flows on Compact Riemannian Manifolds of Negative Curvature," *Soviet Mathematics,* 3 (1962): 1068-1070.

In the summer of 1961, his proof of the Poincaré conjecture behind him, Smale recalls he made "a very clean break with topology."[159] He wrote: "I announced to my friends that I had become so enthusiastic about dynamical systems that I was giving up topology." The reason he gave was that "no problem in topology was as important and exciting as the topological conjugacy problem for diffeomorphisms." This "problem represented the essence of dynamical systems, I felt."[160] His address at the 1962 Stockholm Congress was the result. It deserves that we examine it here.

(iii) What That Allowed in Mathematics?

"There is a paper of Smale that I particularly like to discuss," Smale's student Jacob Palis wrote about his teacher's 1962 address.

> When I first read it about 3 years later, I was initially amazed and taken by it. It looked to me like an unfinished painting with several superposed sketches: Was that allowed in Mathematics? It was certainly very inspiring then as well as three (and many more) years later. *Some results were announced in the paper, but mostly it made transparent his struggle at the time to reach the right concept of hyperbolic systems.* [161]

It was in this paper that Smale made clear that he was now enlarging the concept of dynamical systems to include differential equations as well as mappings (homeomorphisms and diffeomorphisms). He noted that both these problem could be seen as special cases in the study of "a non-compact Lie Group G acting differentiably on a manifold, corresponding to $G=\mathbf{R}$ and $G=\mathbf{Z}$."[162] Structural stability could be equally well

[159] S. Smale in *More Mathematical People,* 311.
[160] S. Smale, "On How I Got Started," 150.
[161] J. Palis, "On the Contribution of Smale," 171. Original emphasis.
[162] S. Smale, "Dynamical Systems and the Topological Conjugacy Problem," 490.

defined for both cases; Smale named the problem of defining an open dense set of diffeomorphisms the "conjugacy problem."

In the rest of the article, Smale toyed with several axioms trying to define an interesting class of diffeomorphisms forming an open dense set. Having to acknowledge nontrivial minimal sets, he nonetheless clung to his original hope that the class of diffeomorphisms defined by his axioms would coincide with the sets of structurally stable diffeomorphisms, with however a hint of uncertainty. "Although very possibly, in the final picture, [the above class] will not be the structurally stable diffeomorphisms, it seems that to date it is the best guess for such."[163]

Smale's address therefore contained at least as many unanswered questions and open problems as theorems. The last paragraph shows his enthusiasm:

> Certainly the main problems stated here are very difficult. On the other hand, it seems quite possible to us that this field may develop rapidly and already as indicated here, there have been some initial steps in this direction.[164]

But even before the end of the year, while in Lausanne, Smale was already drifting away: "I had begun to start thinking about the calculus of variations and infinite-dimensional manifolds, and this preoccupation took me away from dynamical systems for the next three years."[165] Until 1965 therefore. Until after his move back to Berkeley in 1964.

As an introduction to the course on differential topology he gave at the University of Columbia in 1962/63, Smale remarked:

---

[163] S. Smale, "Dynamical Systems and the Topological Conjugacy Problem," 492.
[164] S. Smale, "Dynamical Systems and the Topological Conjugacy Problem," 495.
[165] S. Smale, "On How I Got Started," 151.

> Recent events in differential topology indicate a change of direction is taking place, away from manifolds and toward differentiable mappings and analysis. In this course the new direction will be followed with global calculus of variations as the main goal.[166]

d) Steve Smale's Research School of Dynamical Systems

(i) 'The Heady Wonderful Years of the Mid-Sixties'[167]

In 1964, Steve Smale moved back to the University of California, Berkeley. During the 1960s, the mathematics department at Berkeley was at the center of political agitation whose main objective was to oppose the war in Vietnam.[168] Smale, who in the past had belonged to the Communist Party, got directly involved in the protests. Together with Jerry Rubin, he organized national "Days of Protest" against the Vietnam War.[169]

On the last day of the Moscow congress, where in 1966 he had received his Fields Medal, Smale held a press conference at the request of the North Vietnamese Press. He denounced the American military intervention in Vietnam as "horrible." Simultaneously, he made the parallel with the Soviet invasion of Hungary ten years earlier. "Never could I see justifications for Military Intervention, 10 years ago in Hungary or now in the much more dangerous and brutal American Intervention in Vietnam." On the same occasion, Smale protested against renewed McCarthyism as witnessed by the actions of the House

---

[166] R. H. Abraham, *Lectures of Smale on Differential Topology,* Columbia University 1962/63. Jussieu Lib.
[167] N. Koppel, "Dynamical Systems and the Geometry of Singularly Perturbed Differential Equations," *From Topology to Computation,* ed. M. W. Hirsch et al., 545.
[168] See W. J. Broad, "Unabom Case Is Linked to Antiwar Tumult on U.S. Campuses in 1960's," *The New York Times* (June 1, 1996), 8.
[169] See S. Smale's interview in *More Mathematical People,* ed. D. J. Albers, et al., 305-309; and S. Smale, "The Story of the Poincaré Conjecture," 38-40.

Un-American Activities Committee (HUAC), and the internal situation in the Soviet Union, where "even the most basic means of protest are lacking."[170]

After he had left for Europe, but before his press conference, Smale was subpoenaed by the HUAC. Being already abroad, he did not learn of this before he went to Moscow. All of these events were largely reflected in the press, including the *San Francisco Examiner* and *The New York Times*. The political implications of the case became obvious when in 1966 Representative Richard Roudebush began to attack Smale on the House floor and elsewhere. "The Congressman looked into Smale's background and he's about as pink as they come," Roudebush was quoted as saying.[171]

All this created an unhealthy political situation for the National Science Foundation, which had been supporting, not only Smale's personal research, but also of his whole group at Berkeley. NSF support was withdrawn during the following year, for official reasons that had to do with Smale's management of his funds and technicalities. The charges were nicely encapsulated by the following statement by President Johnson's Science Advisor, Donald Hornig:

> The blithe spirit leads mathematicians to seriously propose that the common man who pays taxes ought to feel that mathematical creation should be supported with public funds on the beaches of Rio de Janeiro or in the Aegean Islands.[172]

---

170 The text of Smale's statement at the Moscow press conference was published in: C. Morrey, "The Case of Stephen Smale," *Notices of the American Mathematical Society,* 14 (October 1967): 778-782, 778. About this episode, see Smale's account "On the Steps of Moscow University," *Mathematical Intelligencer,* 6(2) (1984): 21-27; repr. *From Topology to Computation,* ed. M. W. Hirsch et al.: 41.

171 C. Morrey, "The Case of Stephen Smale," 780.

172 D. Hornig, "A Point of View," *Science,* 161 (1968): 248; quoted in S. Smale, "The Story of the Poincaré Conjecture," 39.

Eventually, after an extensive investigation by Daniel Greenberg, editor of *Science,* it appeared that all charges brought by NSF against Smale were unfounded, and that at the time of its refusal, NSF possessed evidence to that effect. NSF held a policy of "not knowingly giv[ing] nor continu[ing] a grant in support of research for one who is an avowed Communist or anyone established as being a Communist." Since this was not Smale's case, the official conclusion was that "the known facts regarding Professor Smale's activities do not constitute a basis for action."[173] Protests, the exchange of numerous letters, and campaigns in the press succeeded in having the NSF backtrack and renew, although cutting it by half, the grant of the global dynamics group at Berkeley.

Whether Smale then considered his political activities as directly bearing on his mathematical work, I cannot say. In the early 1970s, however, partly as a consequence of his contact with the IHÉS mathematicians, Smale began to concern himself with applications of his purely mathematical work. In this context, he thought, social and political questions had to be raised. In a talk delivered at the International Union of Pure and Applied Physics Conference on Statistical Mechanics, held in Chicago, in March 1971, Smale tackled these issues:

> These days especially, provocative questions confront a *socially conscious scientist* when he begins contemplate where applications of his work might lead. As one whose main work has been in pure mathematics, and who is beginning to concern himself with areas of applied mathematics such as electric circuit theory, *I*

---

[173] Enclosures of a letter from Leland J. Haworth to Charles Morrey (July 24, 1967), repr. in C. Morrey, "The Case of Stephen Smale," 783-784. The story of the NSF episode is chronicled in the following articles: Dan Greenberg, "Smale and BSF: A New Dispute Erupts," *Science,* 157 (1967): 1285; "Handler Statements on Smale Case," *Science,* 157 (1967): 1411; "The Smale Case: Tracing the Path that Led to NSF's Decision," *Science,* 157 (1967): 1536-1539; "Smale: NSF Shifts Position," *Science* 158 (1967): 98; "Smale: NSF's Records Do Not Support the Charges," *Science,* 158 (1967): 618-619.

wonder to what extent I should explicitly direct my work towards socially positive goals. [174]

Apparently Smale then considered directing his political activities away from anti-war protests and towards a reflection on the "many issues on the relations of the profession of a scientist to the social crises of this time." At Chicago, he did not pursue such discussion, but stated that he felt "that mathematicians and scientists in general must face these questions in a much more serious way than they have done to date (myself included)."[175] For reasons opposite to Lefschetz's, Smale certainly considered his work on global analysis as potentially reaching towards "socially positive goals," as he said.

The mathematics department at Berkeley had also been infused early in the 1950s by a noteworthy student coming out from Lefschetz's Project on nonlinear analysis. "Certain it is that upon pursuing his work [Stephen] Diliberto formed a real group of young and capable disciples at Berkeley."[176] Did Diliberto leave a lasting legacy that could still be felt in mid-1960s when Smale got there? Nothing is less certain, as Smale never acknowledged any debt to him.

However, even more than his predecessor, Steve Smale certainly did build a very strong school while at Berkeley. Thom testified to this: "he created a whole school of Dynamicists (Abraham, Pugh, Shub, Bowen, Franks, Robbins, Newhouse), a very brilliant one which completed his results on the characterization problem for structurally

---

[174] S. Smale, "Personal Perspective on Mathematics and Mechanics," *Statistical Mechanics: New Concepts, new problems, New Applications,* ed. S. A. Rice et al. (Chicago: University of Chicago Press, 1972): 3-12; repr. *MT,* 95-105, 95. My emphasis.
[175] S. Smale, "Personal Perspective," 95. Note that, on November 22, 1978, Smale gave a talk at the IHÉS on "Game Theory and the Dynamics of Disarmament (resolving the Prisoner's Dilemma)." *Rapport scientifique 1978 - Séminaires et conférences,* 1. Arch. IHÉS.

stable diffeomorphisms."[177] At the *Smalefest,* a celebration for his sixtieth birthday organized by his ex-students, many, like Sheldon Newhouse, recalled with nostalgia the "atmosphere that existed in the sixties, with all the excitement and developments happening virtually every other week."[178] A partial list of his students is impressive for anyone acquainted with later development of dynamical theories: Nancy Koppel (Ph.D. 1967), Michael Shub (1967), Jacob Palis (1968), Sheldon Newhouse (1969), Rufus Bowen (1970), John Guckenheimer (1970).[179] In addition, Moe Hirsch, Oscar Lanford, and Charles Pugh were also present. A remarkable crew indeed.

(ii) Structurally Stable Systems Are Not Dense, So What Is Next?

Since Smale's conjecture had two parts, it took two step to show it false: (1) show that there were structurally stable systems with an infinite number of periodic solutions: Smale went as far with the horseshoe; 2) show that structurally stable systems were not generic. A counterexample showing the falsity of the second part of his conjecture, which Smale seems to have started to suspect as early as 1962, was published in 1966, in an article simply called "Structurally Stable Systems Are Not Dense."

There, Smale constructed an open set U of vector fields on a four-dimensional manifold such that no vector field in U was structurally stable. Smale's hope for replacing Andronov's philosophical basis for restricting the study of differential equations by a formal mathematical one was challenged.

---

176 S. Lefschetz, "Nonlinear Differential Equations," 17.
177 *Note sur l'oeuvre de Stephen Smale,* par Thom (n.d., prob. 1972). Arch. IHÉS.
178 *From Topology to Computation,* 183. See also J. Palis, "On the Contribution of Smale," esp; 177-178; and N. Koppel, "Dynamical Systems," 545.

> *The example in this paper surely reduces the importance of the notion of structural stability,*" he had to admit. "One might be further discourages from studying the global qualitative theory of higher dimensional, ordinary differential equations.[180]

In Smale's view, pure mathematical rigor could not justify anymore too strong an emphasis on structural stability. But this hardly meant that his dream of finding rigorous mathematical bases for focusing on a well-defined class of dynamical systems as the only ones susceptible of being of interest for the modeling of nature had to be forsaken. Smale still believed that the study of the qualitative theory of differential equations could be "constructive." He announced he was preparing a paper "in this direction based on an axiom, which we consider to be of central importance, axiom *A*."[181]

A major synthesis of dynamical systems theory, containing a wealth of new results and promising research avenues, this paper set the ground for later studies in the field, including catastrophe and chaos theories. Smale's seminars out of which it grew were a major factor in binding the Berkeley school together. Presenting in detail the recently exhibited examples of dynamical systems with complex behavior, Smale struggled for unity. The goal still was to find "a class of diffeomorphisms which include all of the previous examples in a transparent way *and will at least have the possibility* of including an open dense subset of" all diffeomorphisms.[182]

---

[179] *From Topology to Computation,* 184. See the list of Smale's graduate student in *Ibid.,* 59-63.

[180] S. Smale, "Structurally Stable Systems Are Not Dense," *American Journal of Mathematics,* 88 (1966): 491-496, 494. My emphasis.

[181] All quotes above come from S. Smale, "Structurally Stable Systems," 494.

[182] S. Smale, "Differentiable Dynamical Systems," *Bulletin of the American Mathematical Society,* 73 (1967): 747-817; *MT,* 1-82. Quote on 776. My emphasis.

A few years later Smale had to acknowledge that he had posed the problem in "too simple, too rough and too centralized" a way. "I believe now that the main problems of dynamical systems can't be unified so elegantly."[183] The "Dark Realm of dynamics," which did not fall into the domain defined by Smale's axioms, was in the next decade shown to be much "bigger."[184] Nonetheless Smale's "Differential Dynamical Systems" remained the standard reference on the topic for the many years to come and provided the founding stone for much of the later development of theories of chaos.

Given its formidable ambition of classifying, on a purely mathematical basis, dynamical systems general enough to be useful in the modeling of natural phenomena, it is striking to note how abstract, if not abstruse, Smale's program remained in those days. "I cannot recall in my four years at Berkeley having seen many actual differential equations," Nancy Koppel remembered.[185] Speaking of those years, Jacob Palis wrote:

> I like to remember the many discussions we had about dynamics and mathematics, in general, almost never about details and almost always about ideas, directions, theories and this is a concrete way.[186]

Apparently still under Bourbaki's spell, Smale thought that mathematical purity and abstractness could not be divorced from each other.

As a consequence of his search for unity, Smale defined dynamical system in the broadest way he thought possible. His stated purpose was to deal with the action of a Lie group G on a manifold M, the action being defined as "a homomorphism $G \rightarrow \mathrm{Diff}(M)$

---

[183] S. Smale, "Stability and Genericity in Dynamical Systems," *Séminaire Bourbaki,* 22 (February 1970), exposé #374; repr. *MT,* 90-94. Quote on 91.
[184] J. Palis, "On the Contribution of Smale," 178.
[185] N. Koppel, "Dynamical Systems and the Geometry of Singularly Perturbed Differential Equations," in *From Topology to Computation,* ed. M. W. Hirsch et al.: 545-556, 545.

such that the induced map $G \times M \rightarrow M$ is differentiable."[187] This hardly was a language that most physicists, not too speak of specialists of other disciplines, were ready to hear.

In the early 1970s, however, as a result of his extensive contacts with the IHÉS, Smale began to talk to economists, physicists, and biologists. His approach, he acknowledged at the 1971 Statistical Physics Conference,

> may indeed be a little hard to accept for the applied mathematician trained in traditional methods. However, the approach here tends to make applied mathematics and also ordinary differential equations accessible and attractive to the modern mathematician, on who has been brought up in the purist, Bourbaki style of education.[188]

The problems of applied mathematics had to be transformed in order to cater to the expectations of Bourbakist mathematicians, rather than asking the latter to adapt their tools to the problems! For several decades, Smale thought, mathematics had developed as "very separated from applications." Acknowledging "a certain disdain for applied mathematics by many leading mathematicians," he saw "some healthy sides to this separation." But now, as a socially conscious scientist, Smale believed it "worthwhile to change this course of events," by bringing some "modernizations" to fields of applied mathematics. Although exuding condescension, this call of Smale's would be widely heard. It is true that, in this endeavor, he was greatly helped by a group of mathematicians and physicists who orbited IHÉS.

[186] J. Palis, "On the Contribution of Smale," 177.
[187] S. Smale, "Differentiable Dynamical Systems," 747.
[188] S. Smale, "Personal Perspectives," 100.

6. HISTORIOGRAPHY OF CHAOS: A QUESTION OF TIMING

"Chaos was discovered by Poincaré."[189] This statement is among the most common clichés about chaos.

Chaos has had a short history, and the history of historical accounts of it is even shorter. Some historiographical questions have nevertheless surfaced insistently. Among them, the most pressing remains the conundrum of having to explain why such a burst of activity could happen, on the basis of old mathematical ideas, very often going back to Henri Poincaré's work often seen in a new light. 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 "revolution" of the last decades?[190]

Thus a theme focusing on the "nontreatment" of chaos emerged in the literature.[191] As was immediately recognized, simply to state this scarcely solved the problem. One had to explain it. Several reasons emerged: the physicists' interest in other theories than classical mechanics (relativity and quantum theories); social emphasis on stability in the scientists' training and research practices, as well as on theories which enhanced human control over nature rather then our understanding of its processes, be it for reasons that had to do with technological, philosophical, or even specifically masculine *a priori* dogmas about the nature of science.

---

[189] F. Diacu and P. Holmes, *Celestial Encounters,* 78.
[190] The most important advocate for this revolutionary view was of course J. Gleick's book, *Chaos.*
[191] Present in scientists' account, this theme was best articulated by Stephen Kellert, *In the Wake of Chaos,* 119-128.

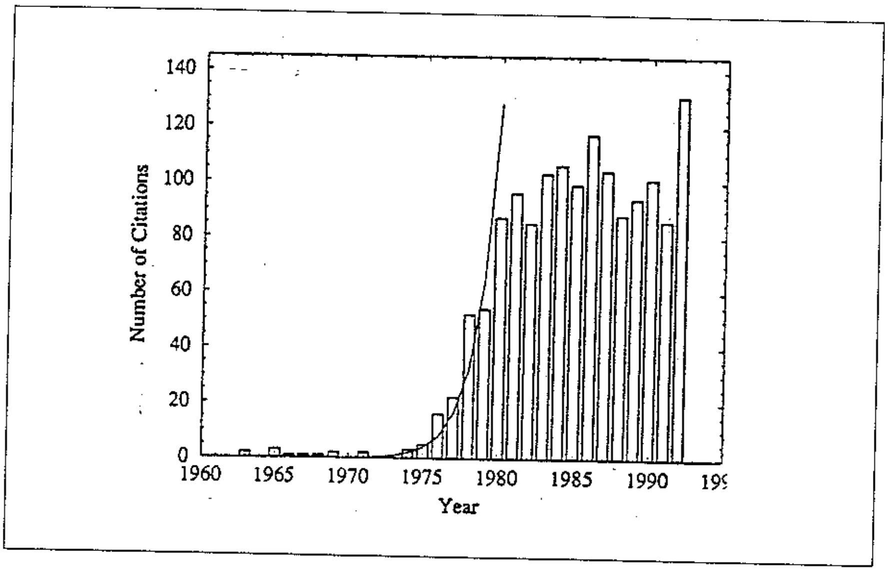

Figure 11: Citations to Edward Lorenz, "Deterministic Nonperiodic Flow," *Journal of the Atmospheric Sciences,* 20 (1963): 130-141. Repr. with permission from J. B. Elsner and J. C. Honoré, "Ignoring Chaos," *Bulletin of the American Meteorological Society,* 75 (1994):1846. Copyright © The American Meteorological Society.

(i) Traditions, Synthesis, and Topology

The above has shown that, contrary to what is often claimed, it rather seems that the quest for stability was, at least in cases of Smale (and in Thom's also, as we shall see in Chapter VI), the dominant motivation for studying those systems which, perhaps were *not* stable in the sense of Poisson (sensitive dependence on initial conditions), but still were (structurally) stable in a topological sense. The search for stability far from hindering the emergence of chaos thus crucially contributed to it.

Moreover, it is obvious from the above that Poincaré's ideas and techniques were never lost to the scientific community as a whole. Several decades of work on these ideas allowed mathematicians and physicists to go much farther than Poincaré might have thought possible. As I mentioned above, the major weakness in the social makeup of the communities that exploited these ideas lay in their dispersion. Conditions for uniting the several strands of research that were more or less directly inspired by Poincaré's qualitative theories did not exist before Smale's synthesis.

But, as Edward Lorenz noted, "one may argue that an early outburst [of research on chaos] was not *caused* by a prevailing lack of interest; it *was* the lack of interest" (Fig. 11).[192] With this striking graph, the chaos craze is made especially clear. After ten years of neglect, the number of citation grew exponentially, at a rapid pace, for about 7 years, from 1974 to 1980. It then stabilized at around 100 citations per year over the following decade. Of course, the surge of interest for Lorenz's paper, which I call the chaos craze, is not the cause, but the effect of the emergence of chaos. This lack of interest and the subsequent craze for catastrophe and chaos theories led some scientists to ponder on the roles that fashion might play in orienting their research.[193] If we take this view seriously, the question of the "nontreatment of chaos" would then amount to ask, for example: Why were miniskirt *not* in fashion before the sixties?! This would be a clearly ridiculous way of putting the nonetheless interesting historical question of determining what social changes in Western societies could be seen as being reflected the miniskirt fashion.

This analogy, no matter how stretched it might appear, offers us a way into our historiographical conundrum. Instead of looking for reason why scientists did not exploit Poincaré's ideas and techniques before—they clearly did, as the above chapter plainly

[192] E. N. Lorenz, *The Essence of Chaos,* 125. His emphasis.
[193] See, e.g., David Ruelle, *Chance and Chaos,* 71.

shows—we might try to isolate positive reasons that triggered the fashion. This is exactly what I am attempting to work out throughout this dissertation.

When in 1996 Smale recalled the context in which he was able to find his famous horseshoe, himself offered such a reason: "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)."[194] These three traditions were: the Gorki school starting with Andronov and Pontrjagin and taken up by Lefschetz after World War II; the tradition coming out of studies of the van der Pol equation *via* Cartwright and Levinson; and the somewhat forgotten tradition of Poincaré and Birkhoff's work on dynamical systems and homoclinic points. Smale seemed to imply that by chance he could see a way to synthesize these three traditions.

Without minimizing Smale's achievements, we may wonder whether these traditions only seemed separate from his own viewpoint, which, after all, was one of an outsider. It seems true that Birkhoff's heritage was not emphasized by Lefschetz's school.[195] However, the work of Cartwright, who was invited at Princeton, and that of Levinson, were well known to members of the Project.[196] Poincaré's work was important for the Gorki school, and for Cartwright, among others; but the different concepts of Poincaré's that were exploited came from diverse, more or less connected parts his opus, which was so vast that knowledge of one part hardly precluded total ignorance of many other parts. Furthermore, Lyapunov's notions, important for LaSalle and Lefschetz, lost

[194] S. Smale, "Chaos," 12. My emphasis.
[195] See A. Dahan Dalmedico, "La renaissance des systèmes dynamiques."

their prominence with Smale's and Thom's schools. Smale's three traditions appeared to have intersected more than he thought before he took up the problem.

A major reason for Smale's success was that only with his work do we see abstract topology fully coming into play in the study of dynamical systems. With brio, he used highly abstract tools and techniques that had been developed only recently, and were extremely well codified, thanks to a large extent to the dominant Bourbakist attitude of mathematicians of all countries. These techniques, matched with some mathematicians' powerful ability for geometric reasoning, offered powerful new ways of perceiving older results. As Smale explained in retrospect:

> The more traditional way of dealing with dynamics was with the use of mathematical, e.g. algebraic, expressions. But a description given by formulae would be cumbersome [in the case of the horseshoe]. It would unlikely lead to insights or to a perceptive analysis, since that form of a description wouldn't communicate as efficiently the information in the figure. My background as a topologist, trained to bend objects as squares, helped to make it possible to see the horseshoe.[197]

(ii) The Impact of the Computer: Lorenz's Butterfly and Similar Cases

Remarkably, at about the same time as Smale was designing the horseshoe, a few scientists began to encounter other examples of nonperiodic solutions of systems of differential equations, which might have shed light on the above remaining question, but did not. The people working around Smale, Thom, and Ruelle would not become aware of them, then especially focusing on Lorenz's attractor, for a good fifteen years. Jacob Palis later recalled,

[196] J. LaSalle, "Relaxation Oscillations," *Quarterly of Applied Mathematics,* 7 (1949): 1-19; M. L. Cartwright, "Forced Oscillations in Nonlinear Systems," *Contributions to the Theory of Nonlinear Oscillations,* 1, ed. S. Lefschetz: 149-242.

> It is curious to remark that at that very moment, a remarkable work that would remain unknown to us for the next ten years was being developed: *Lorenz was proposing the impossibility of the second half of the dream [i.e. that stable systems form a dense subset of all systems] through his still experimental robust (open) strange attractors (unstable).*[198]

Rediscovered in the mid-1970s by an applied mathematician from the University of Maryland, James Yorke, in the article that made the word "chaos" famous, the Lorenz equation was obtained from an extreme simplification of a system arising in hydrodynamics:[199]

$$
\begin{aligned}
X' &= -\sigma X + \sigma Y, \\
Y' &= -XZ + rX - Y, \\
Z' &= XY - bZ.
\end{aligned}
$$

In the early 1960s, MIT meteorologist Edward Lorenz first arrived at this set of equations, from which he extracted the butterfly attractor simply by numerically plotting the trajectories of its solutions. As an undergraduate at Harvard, Lorenz had briefly had G. D. Birkhoff as a professor. But since, he had become involved in a vast program, initiated after World War II by John von Neumann (1903-1957) and Jule Charney (1917-1981) among others, to use computers for weather prediction more efficiently.[200]

---

[197] S. Smale, "Chaos," 6.

[198] J. Palis, "On the Contribution of Smale," 171. His emphasis.

[199] E. N. Lorenz, "Deterministic Nonperiodic Flow," *Journal of the Atmospheric Sciences,* 20 (1963): 130-141; Repr. *Univ. Chaos,* 367-378; *Chaos II,* 244-255. T.-Y. Li and J. A. Yorke, "Period Three Implies Chaos," *American Mathematical Monthly,* 82 (1975): 985-992; repr. in Hao B.-L., *Chaos,* 1st ed. (1984): 244-251. The story of how Yorke came to be interested in Lorenz's paper, and introduced Smale's group to it, is told in E. N. Lorenz, *The Essence of Chaos* (Seattle: The University of Washington Press, 1993), 145. See in Chapter VI, however, for a discussion of the word "chaos" questioning the received idea that Li and Yorke were the first to use it.

[200] I thank Amy Dahan Dalmedico for having provided me with details about the story of this program. A forthcoming publication should gather her findings.

It is useful to contrast the reasons that pushed Smale and Lorenz to come up with systems that exhibited nonperiodic solutions. For Smale, as we have seen, the main drive was to explore the structure of limit sets of differential equations. Having realized that they might be complicated, he isolated the horseshoe as a simple example.

In his later recollections, Lorenz explained why, as opposed to Smale, he had for a long time explicitly sought a system of differential equations with nonperiodic solutions. He reckoned that, in the postwar years, two distinct approaches for weather prediction could be isolated. "Dynamic meteorology" was concerned with applying the laws of physics—dynamics and thermodynamics—to the circulation of the atmosphere. This was a formidably complex problem, but the hope was that, soon, computers could help integrating systems of equations with several thousands, or even millions, of variables. On the other hand, there was also an old tradition of doing "synoptic meteorology." This approach differed from the dynamicists' in that it focused on characteristic structures of the atmosphere: high- and low-pressure systems, fronts, hurricanes, etc. Based on craft knowledge and more qualitative, the synopticians' approach was less obvious to put on a computer.[201]

A way into this problem, called "statistical weather forecasting," was provided by Thomas Malone who directed a fairly successful project at MIT. As Lorenz described it in 1993,

> the type of statistical forecasting that had received most attention was 'linear' forecasting, where, for example, tomorrow's temperature at New York might be predicted to be a constant *a*, plus another constant *b* times today's temperature at Chicago, plus another constant *c* times yesterday's relative humidity at St. Louis,

[201] E. N. Lorenz, *Essence of Chaos,* 82-83.

> plus similar terms. There were long-established procedures for estimating the optimum values of the constants *a*, *b*, *c*, etc.[202]

In 1955 Lorenz was appointed to fill in for Malone. As he recalled, statistical forecasting "was regarded by many dynamical meteorologists, particularly those who were championing numerical weather prediction, as a pedestrian approach that yielded no new understanding."[203] For practical purpose, however, the important question was whether, given sufficient computing power, linear forecasting methods could be as accurate as those of numerical dynamical meteorology.

Lorenz soon became convinced, he said, that this method was not very good. Facing opposition from the statistical forecasting community at a meeting in Madison, Wisconsin, in 1956, he proposed:

> to test the hypothesis by selecting a system of equations that was decidedly not of the linear type. I would use a computer to generate an extended numerical solution, and then, treating the solution as a collection of real weather data, I would use standard procedure to determine a set of optimum linear prediction formulas.[204]

On a Royal-McBee LGP-30, Lorenz first studied simplified meteorological equations, settling on a system of twelve equations. He first found solutions that settled down to a steady state, therefore useless as a simulation of the weather. Tinkering with parameters, he found oscillating solutions, only to realize that, using standard methods, one could obtain *perfect* linear prediction of them. "It was [around 1959] that I recognized that for my test *I would need a set of equations whose solutions were not periodic.*"[205]

---

[202] E. N. Lorenz, *Essence of Chaos,* 130-131.
[203] E. N. Lorenz, *Essence of Chaos,* 131.
[204] E. N. Lorenz, *Essence of Chaos,* 131-132.
[205] E. N. Lorenz, *Essence of Chaos,* 133. My emphasis.

As Lorenz wrote in 1963, it was "not obvious that such solutions can exist at all. . . . [N]onperiodic flow has sometimes been regarded as the [mere] result of nonperiodic or random forcing."[206] Had he been aware of Smale's conjectures and able to see what they meant, he might have thought it impossible to exhibit such nonperiodic behavior. A crucial difference between Lorenz and Smale's approaches however lay in the fact that while the latter was looking for *generic* complex behavior, the former only needed *one* system with nonperiodic solutions.

Not aware either of Poincaré's work, nor of Cartwright, Littlewood and Levinson's, already known to exhibit nonperiodic solutions for simpler systems, he pursued the numerical exploration of his system, and was finally able to get the long-sought nonperiodic behavior. "When I applied the standard procedure to the new 'data,' the resulting linear forecasts were far from perfect, and I felt that my suspicions had been confirmed."[207]

More importantly for the history of chaos, with this system, Lorenz observed that nonperiodic flows exhibited an "instability with respect to modifications of small amplitude."[208] In this oft-told episode, Lorenz decided to repeat an experiment with rounded-off values as its initial conditions, and found that the new output quickly diverged significantly from the original experiment.[209]

> It implies that two states differing by imperceptible amounts may eventually evolve into two considerably different states. If, then, there is any error whatever in observing the present state—and in any real system such errors seem

[206] E. N. Lorenz, "Nonperiodic Deterministic Flow," 131.
[207] E. N. Lorenz, *Essence of Chaos,* 134.
[208] E. N. Lorenz, "Nonperiodic Deterministic Flow," 132.
[209] See, e.g., E. N. Lorenz, *Essence of Chaos,* 134-136, and J. Gleick, *Chaos,* chap. 1, 11-31.

> inevitable—an acceptable prediction of an instantaneous state in the distant future may well be impossible.[210]

Lorenz's emphasis on this kind of instability may easily be understood in view of his interest in the possibility of modeling the weather on computers. He thus immediately raised a fundamental, though hypothetical, consequence of his study:

> When our results concerning the instability of nonperiodic flow are applied to the atmosphere, which is ostensibly nonperiodic, they indicate that *prediction of the sufficiently distant future is impossible by any method, unless the present conditions are known exactly.* In view of the inevitable inaccuracy and incompleteness of weather observation, precise very-long-range forecasting would seem to be non-existent.[211]

Lorenz briefly presented this result at a 1961 conference in Tokyo.[212] In his famous "Nonperiodic Deterministic Flow" paper published in the *Journal of the Atmospheric Sciences* in 1963, he studied the consequences of this very instability. This article however used another set of equations: the celebrated Lorenz system presented above. They came from a further idealization of Barry Saltzman's already idealized system of seven equations, intended to model convective fluid motion driven by heating from below.[213] In his 1963 paper, Lorenz suggested that within the limit of accuracy the solutions converged towards a pair of intersecting surfaces, indefinitely spiraling around

---

[210] E. N. Lorenz, "Nonperiodic Deterministic Flow," 133.

[211] E. N. Lorenz, "Nonperiodic Deterministic Flow," 141. This and the preceding quotes is a quite lucid, early definition of sensitive dependence on initial conditions. Compare with Poincaré and Duhem. This issue was addressed more thoroughly by Lorenz in the famous talk he gave on December 29, 1972, at an AAAS meeting: "Predictability: Does the Flap of a Butterfly's Wings in Brazil Set off A Tornado in Texas?," first published as an appendix of the *Essence of Chaos,* 181-184.

[212] E. N. Lorenz, "The Statistical Prediction of Solutions of Dynamic Equations," *Proceedings of the International Symposium on Numerical Weather Prediction, Tokyo,* 629-635.

[213] B. Saltzman, "Finite Amplitude Free Convection as an Initiate Value Problem, I," *Journal of the Atmospheric Sciences,* 19 (1962): 329-341.

two centers without ever intersecting themselves. He described this set later to be called a strange attractor as an "infinite complex of surfaces."[214]

Trying to apply linear forecasting methods, Lorenz also constructed discrete iteration laws for the maximum reached by one of the coordinates $M_{n+1} = f(M_n)$, clearly showing that—like Smale's horseshoe—the discrete problem exhibited an infinite number of periodic, as well as nonperiodic, solutions. It was this part of the paper that drew applied mathematician Jim Yorke's attention.

In 1974, Li and Yorke presented an article at the last of the great symposia on dynamical systems, inspired by Lefschetz's spirit two years after his death. "Our initial investigations were stimulated by a fascinating series of Lorenz."[215] They stated their purpose as "to get a new glimmer of understanding of the possible analytic regularities in the chaotic, turbulent, unstable, irregular processes of such greater complexity that surround us."[216] Lorenz's system provided Li and Yorke with an example of a simple system with complicated behavior, which they naturally qualified as "chaotic" in the usual sense of the. They explained:

---

[214] E. N. Lorenz, "Nonperiodic Deterministic Flow," 140. Note that, besides Birkhoff and Poincaré, Lorenz used: V. V. Nemystkii and V. V. Stepanov, *Qualitative Theory of Differential Equations* (Princeton: Princeton University Press, 1960 [1949]), translated under the auspices of Lefschetz.

[215] T.-Y. Li and J. A. Yorke, "The 'Simplest' Dynamical System," *Dynamical Systems: International Symposium on Dynamical Systems, Brown University, 1974,* ed. L. Cesari, J. K. Hale, and J. P. LaSalle (New York: Academic Press, 1976): 203-206, 203. Other articles by Lorenz on this topic to which Li and Yorke referred: "The Problem of Deducing the Climate from the Governing Equations," *Tellus,* 16 (1964): 1-11; "The Mechanics of Vacillation," *Journal of the Atmospheric Sciences,* 20 (1963): 448-464; "The Predictability of Hydrodynamic Flow," *Transactions of the New York Academy of Sciences,* 2nd ser., 25 (1963): 409-432.

[216] T.-Y. Li and J. A. Yorke, "The 'Simplest' Dynamical System," 203.

> chaotic oscillations of complicated phenomena may sometimes be understood in terms of the simple model, even if that model is not sufficiently sophisticated to allow accurate numerical prediction.[217]

Only when around 1975 mathematicians from Smale's school took up the study of the Lorenz system, would the convergence between Lorenz's numerical investigation and Smale's topological methods be realized.[218] This convergence first sparkled the chaos burst. But, as we shall see in the next chapters, much had happened in between.

As computer simulations became more and more accessible, other examples of nonperiodic solutions of differential equations surfaced quite naturally, but people had trouble finding the right conceptual setting to interpret them. Let me mention just a few examples. In 1958, five year before the publication of Lorenz's article, Japanese

---

[217] T.-Y. Li and J. A. Yorke, "Period Three Implies Chaos," 985. The terms "chaos" and "chaotic regime," with due acknowledgment to Yorke and Li, as well as the reference to Lorenz, were immediately taken up by ecologist Robert May in his famous articles: "Biological Populations with Nonoverlapping Generations, Stable Points, Stable Cycles, and Chaos," *Science,* 186 (1974): 645-647; and "Simple Mathematical Models with Complicated Dynamics," *Nature,* 261 (1976): 459-467; repr. *Univ. Chaos,* 85-73; *Chaos II,* 151-159. On the first use of "chaos" in this context, see however Chapter VI below.

[218] The first works, after Li and Yorke's, done by physicists and mathematicians on the Lorenz system are: J. Guckenheimer, "A Strange, Strange Attractor," *The Hopf Bifurcation and Its Applications,* ed. J. E. Marsden and M. McCracken (New York: Springer, 1976): 368-381; D. Ruelle, "The Lorenz Attractor and the Problem of Turbulence," *Turbulence and Navier-Stokes Equations,* ed. Roger Tenam (Berlin: Springer, 1977): 146-158 [which widely circulated in preprint form]; D. Ruelle, "Statistical Mechanics and Dynamical Systems," and O. E. Lanford, III, "An Introduction to the Lorenz System," both in *1976 Duke University Turbulence Conference* (Durham: Duke University, 1977); and R. F. Williams, "The Structure of Lorenz Attractors," and O. E. Lanford, "Computer pictures of the Lorenz Attractors," both in *Turbulence Seminar: Berkeley 1976/77,* ed. P. Bernard and T. Ratiu, Lecture Notes in Mathematics, 615 (Berlin: Springer, 1977): 94-112 and 113-116.

geophysicist Tsuneji Rikitake studied coupled disk dynamos leading to the following system of equations, which strikingly resembled Lorenz's:[219]

$$\frac{dx}{dt} = yz - \mu x;$$
$$\frac{dy}{dt} = 1 - xz;$$
$$\frac{dz}{dt} = xy - ax - \mu z.$$

Explicitly, Rikitake drew attention to the fact that this system of equations could exhibit non-steady, i.e. nonperiodic, numerical solutions. The process that led Rikitake to this result recalls Lorenz's, since he suggested that oscillations of disk dynamos could provide a possible analogy with the magnetic field of the earth, and its reversal mechanism.[220] Here again a simple idealized system was used to gain insight on a complex situation, and indicated that nonperiodicity need not come from the many degrees of freedom in the system.

Similarly, Yoshisuke Ueda, a Japanese third-year graduate student in electrical engineering at Kyoto University, used an analog computer to plot solutions of a mixture of van der Pol's and Duffing's equations. He was of course searching smooth oval closed curves, and instead, on November 27, 1961, found "a broken egg with jagged edges."

---

[219] T. Rikitake, "Oscillations of a System of Disk Dynamos," *Proceedings of the Cambridge Philosophical Society,* 54 (1958): 89-105; and also T. Rikitake, *Electromagnetism and the Earth Interior* (Amsterdam: Elsevier, 1966), 61-64. The following equation is the consequence of equations (5-14) and (5-15), 62-63. For an accessible discussion of it, see P. Bergé et al., *Des Rythmes au Chaos* (Paris: Odile Jacob, 1994), ch. 3.
[220] This suggestion came from E. C. Bullard, "The Stability of a Homopolar Dynamo." *Proceedings of the Cambridge Philosophical Society,* 51 (1955): 744-760.

This started a fruitful career in nonlinear dynamics for Ueda, which culminated in the finding of a strange attractor, christened the "Japanese attractor" by David Ruelle.[221]

The striking commonality across all these examples of strange attractors *avant la lettre*—and several other ones could be provided—is that they all came up in the course of numerical studies of simplified models for physical situations. Rather, what seems now to be the most remarkable aspect of the work of Smale, Thom and other topologists, contrary to the above, resides in the fact that they discovered all their examples *without* the help of the computer. The issue was almost never even raised in their circles. Whether they indirectly reacted to the expanding availability of computers is another, but crucial, question. Already in 1946, John von Neumann had declared:

> Our present analytical methods seem unsuitable for the solution of the important problems arising in connection with nonlinear partial differential equations and, in fact, with virtually all types of nonlinear problems in pure mathematics. . . . . [R]eally, efficient high-speed computing devices may . . . provide us with those heuristic hints which are needed in all parts of mathematics for genuine progress.[222]

With more and more urgency, it became obvious to everyone, in the last third of the century, that the computer offered a way to solve as accurately as needed any

---

[221] Y. Ueda, "Strange Attractors and the Origin of Chaos," *Nonlinear Science Today,* 2(2) (1992); repr. in Y. Ueda, *The Road to Chaos,* ed. R. H. Abraham and H. B. Stewart (Santa Cruz: Aerial Press, 1992): 185-216; quote on p. 189. Ueda simulated the following equation: $v'' - \mu(1-\gamma v^2)v' + v^3 = B\cos\nu t$. To obtain Duffing's equation, replace the nonlinear coefficient of v' by a constant. See D. Ruelle, "Les attracteurs étranges," *La Recherche,* 11 (1980): 132-144.

[222] John von Neumann, in a talk at McGill University in 1946; quoted in H. R. Pagels, *The Dreams of Reason: The Computer and the Rise of the Sciences of Complexity* (New York: Bantam, 1988), 85. Already in 1908, Poincaré saw that the goal of computing with more or less rapidly converging series the solutions of differential equations, while enough for the engineer, was only a first step for a mathematician who was more interested in finding general formal procedures. See "L'avenir des mathématiques," *Atti*

nonlinear differential equation; the only question remaining was whether this solution meant anything at all. It is there that the mathematical work done on dynamical systems, using the latest of topological knowledge, became essential.[223]

Of course, the rise of a topological point of view for the study of differential equation does not answer the question of why examples of chaotic behavior, like the one so nicely exhibited by Lorenz, started to proliferate in distant domains at about the same time. This question of timing has to be solved by looking at the evolving conditions of scientific research in those years, especially with regard to the changing practices of mathematical modeling.

As a lesson of the debate between Smale and NSF, in 1967, the *Washington Star* concluded: "If only conformists need apply [to NSF], 1984 is here already. What can Stephen Smale do that a computer can't do a lot faster? And besides, a computer wouldn't talk back."[224] Nowhere had the question been so plainly put: if a computer could provide the solution to any differential equation, of what use were applied mathematicians anymore?

7. **CONCLUSION**

The above has shown that a geometrical point of view inspired by recent advances in topology, together with growing awareness that the advent of the computer changed the

---

del IV Congresso internazionale dei matematici, Roma 1908, 1 (Rome: Accademia dei Lincei, 1909): 167-182, esp. 172-174.

223 This point is raised by J. Möser, "Is the Solar System Stable," *Mathematical Intelligencer,* 1(1979): 65-71. I deal with this question in more detail in Chapter VII below when I talk about Ruelle.

modeling practices of scientists, were at the roots of much innovation. Moe Hirsch later clearly expressed the implications this had on the mathematician's trade:

> Contrary to some philosophers of science, accurate description of reality is not the only role for mathematics in the natural sciences. . . . Instead of precise equations, a robust *class* of equations is resorted to, no one of which is accurate, but which are plausible as a class. In this way, mathematics offers insights to the natural sciences that probably cannot be obtained in any other way.[225]

The computer made it hard for scientists to avoid facing complex, chaotic behaviors in simple dynamical systems. Topology was needed in order to show that these could not be talked away as simple artifacts of numerical analysis.

The methodological consequences of the topologization of dynamics were important, even at a philosophical level. In 1966, René Thom suggested to replace the deterministic a priori of mathematical modelers by an hypothesis of structural stability. Clearly identifying, as a simple matter of fact, sensitive dependence to initial condition as a limit to classic views about determinism, he noticed that in many examples of mundane natural phenomena (a flat disk spiraling in a free fall through air, for example), "minute variations in initial conditions may lead to very great variations in subsequent development." For Thom, the postulate that such systems were determined was "properly speaking a metaphysical position, impossible to verify experimentally." He therefore suggested to replace "the unverifiable hypothesis of determinism by the empirically

---

224 In the November 9, 1967, issue of *Washington Star;* quoted by C. Morrey, "The Case of Stephen Smale: Conclusion," *Notices of the American Mathematical Society,* 15 (January 1968): 49-52, 52.

225 Comment made by M. Hirsch in *From Topology to Computation,* ed. M. W. Hirsch et al., 604. See also Morris W. Hirsch, "The Dynamical Systems Approach to Differential Equations," *Bulletin of the American Mathematical Society,* 11 (1984): 1-64, 11-14.

verifiable property of 'structural stability'."[226] In plain words, Thom expressed the belief that topology could replace metaphysics.

The process by which topologists became interested in applying their results to the modeling of natural phenomena was a crucial one taking place in 1966-1972. I have argued that the rise of what we might call "applied topology" becomes one of the most important historical phenomenon at the roots of the emergence of catastrophe and chaos theories. There, Thom's role, as well as that of his institution, the IHÉS, was central. This is what I examine in the next chapter.

---

[226] R. Thom, "Une théorie dynamique de la morphogénèse," *Towards a Theoretical Biology, I: Prologomena,* ed. C. H. Waddington (Edinburgh: University of Edingburgh Press, 1968): 152-166, 155; repr. *MMM,* 13-38, 16.