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The Chaos Revolution: a Personal View

by

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Abstract. Chaos theory began abruptly in 1889, in the prize work of Poincaré, published in 1890. After a tangled history, it arrived in my life in Berkeley in 1960. It was then my luck to be an observer of the chaos revolution from fairly close-up. In this paper I try to recollect the main events of this story.

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1. Introduction.

I maintain that the Chaos Revolution was one of the most important social transformations of all time. (Abraham, 1994) To others, it was a passing fad. Whatever the judgement on this issue, it has been a big factor in my life. And due perhaps to the occurrence of my name in an extraordinarily popular book by a journalist, I have been consulted by many scientists involved in the paradigm shift from order to chaos when it first appeared in their own fields. (Gleick, 1987) These first hand experiences are the grist of this chaos story.

2. The discovery of the homoclinic tangle by Poincare, 1889

This story has been told only recently. (Barrow-Green, 1997; Peterson, 1993) Dedekind (a professor at Berlin) claimed to have proved the stability of the solar system, then he died. Weierstrass (also professor at Berlin) tried to provide the proof, without success. His students Mittag-Leffler and Kovalevskaya (professors at Stockholm) persuaded King Oscar II of Sweden and Norway to offer a prize for the solution of this unsolved problem, the winner to be announced on his 60th birthday, January 12, 1889. Weierstrass, Mittag-Leffler, and Hermite (professor at Paris) were the judges. Poincaré (a student of Hermite) was judged the winner. But before the prize paper had been published, questions raised by Phragmén (professor at Stockholm) who was editing the prize paper for publication in July of 1889 led to the discovery by Poincaré of a mistake in his proof. Due to professional jealousies, ethics, and royal interest, there was a lot of pressure on Poincaré to repair the error. He succeeded, and the paper he originally submitted for the prize was hidden away, and replaced by a new one, in which the discovery of the homoclinic tangle first appeared. The discovery must have been around December 1, 1889. The replacement prize paper was submitted in January of 1890, and published later that year. See (Barrow-Green, 1997) for support of these dates, and (Abraham, 1992; Part III) for an extensive visual introduction to homoclinic tangles.

3. From Paris to Mexico City: the first 70 years of chaos theory

Chaos theory has been known variously as dynamical systems theory, the theory of nonlinear oscillations, the qualitative theory of systems of ordinary differential equations, and the mathematical theories of chaotic attractors and their bifurcations. Its peregrinations from the big bang of Poincaré in 1889 to me in 1960 involves the mathematical histories of France, Russia, the United States, and Mexico. The main sequence of events, may be pieced together from historical essays by Okan Gurel (Gurel, 1979; Introduction), Christian Mira (Abraham, Gardini, and Mira, 1997; Appendices 5, 6), (Mira, 1997), and (Hirsch, Marsden, and Shub, 1993; Chs. 1-10, 17).

The short version of the story, as I know it, is this. The new ideas of Poincaré, following his death in 1912, were continued by the young American mathematician, George David Birkhoff (professor at Harvard). This American line of heritage, however, soon died out. Meanwhile, perhaps inspired by Liapunov, the Russian contemporary of Poincaré and independent pioneer of the qualitative theory, the Moscow-Gorki school, beginning with Mandelstham around 1925 and his outstanding student Andronov, followed parallel lines, which continue to the present day. Concurrently, a European tradition evolved, including the engineers Duffing in Berlin and Van der Pol in Holland.

The revival of dynamical systems theory in the Americas was due to an intentional intervention by Solomon Lefshetz, the great pioneer of algebraic topology. Born in Russia and trained in electrical engineering, he switched to mathematics after an accident which claimed both of his forearms. Following his original work in algebraic topology during the 1930s and 40s, he returned to applied topics. Through his familiarity with the Russian language, he became aware of the work of the Moscow-Gorki school. In order to stimulate work in dynamics in the United States and Europe, he translated one of the main works of that school into English. (Andronov and Chaikin, 1949) Another factor might have been the publication of a text in English on the Russian developments. (Minorsky, 1947) At the same time, in an altruistic effort to promote mathematics in Latin America, Lefshetz began spending half of each year at the National Autonomous University of Mexico, in Mexico City. It was there that I first met him, in the summer of 1959.

Among his graduate students in Mexico were several excellent topologists, including Mauricio Peixoto. The 1958 work of Peixoto on structurally stable systems in the plane, published in 1959, triggered the revival of the Poincaré tradition in the United States. Stephen Smale met Peixoto in 1958, and learned of this work. (Hirsch, Marsden, and Shub, 1993; Ch. 2) A meeting in Mexico City in the summer of 1959 brought together Lefshetz, Peixoto, Smale, and others, who would figure prominently in chaos theory. Ironically, I was there at the time, working on my thesis in general relativity theory. I saw the participants from the wings, but did not attend the talks.

4. The golden years of global analysis: Berkeley, 1960-1968

With my thesis finished in early 1960, I looked for a job. My thesis advisor, Nathaniel Coburn, was very helpful, but by the late Spring, I had only one offer. I prepared to move to Milwaukee. But at the last minute, I received an another offer, for a special position with reduced teaching, from the University of California at Berkeley. I accepted at once and moved to Berkeley, not knowing yet that a substantial number of leading mathematicians were moving there at the same time. When I arrived, I began to meet these people — Chern, Spanier, Hochschild, Smale, Hirsch — without fully realizing the level on which they were working. In October, 1960, I met Smale in the daily math tea, and innocently asked him what he was doing. This began a working partnership and friendship which spanned several years. Our subject was then called global analysis, and included dynamical systems theory, the calculus of variations, manifolds of mappings, nonlinear functional analysis, partial differential equations, and so on. We would speak regularly — over a span of eight years, moving separately around Berkeley, Columbia, and Princeton — on all areas of global analysis, and especially about homoclinic tangles. While I obtained results on manifolds of mappings and transversality theory, Smale obtained a number of results on dynamical systems theory which dominated the advance of the subject at the time, and were summarized in his fantastic survey paper of 1967. Among these results my favorite was his spectacular work on the horseshoe map, in which the homoclinic tangle of Poincaré was untangled for once and for all. Here are some highlights of the eight golden years.

- 1960-61. In Berkeley, Smale and I reread Birkhoff's collected works, particularly a paper written jointly with Paul Smith in 1928, and had many discussions on homoclinic tangles of diffeomorphisms of the plane. We were also very interested in Hamiltonian dynamical systems. René Thom was in Berkeley at this time, and I attended his course on singularity theory. At the end of this year we had a Summer conference on dynamics in Berkeley. Shlomo Sternberg and Sol Schwartzman were among the active participants. Then we went to Urbino, where Smale presented his work on global stable manifolds, and we went on to Bonn, where I presented my version of transversality theory for the first time.

• 1961-62. Smale transferred to Columbia University, where ironically Paul Smith was the chair, while I stayed on for a second year in Berkeley. During this time I frequently spent the lunch hour with Moe Hirsch and Ed Spanier at the swimming pool on the Berkeley campus, and we enjoyed long and fruitful discussions on global analysis, transversality theory, and the like. Among other things, these swims resulted in some of the main ideas presented in my 1967 book, *Transversal Mappings and Flows*, and Morris Hirsch's 1976 book, *Differential Topology*. These happy times are still vivid in my memory.

• 1962-63. I followed Smale to Columbia, where we resumed working together. He was interested in quantum mechanics and variational calculus. I gave lectures on transversality theory in his course, especially during his absence occasioned by the Bay of Pigs crisis, as recounted in his book, *The Dynamics of Time*. My notes for these lectures were published as a pamphlet in 1963, entitled *Lectures of Smale on Differential Topology*, even though they covered primarily my own lectures. One goal of my lectures was to firmly establish Birkhoff's signature of a tangle. During this year we met Bob Williams, Mike Shub, and Charlie Pugh, who eventually joined our group. I returned to Berkeley for the summer to find Smale and Hirsch very occupied with an antiwar movement, the Vietnam Day Committee.

• 1963-64. My second year at Columbia, Smale had gone back to Berkeley. I continued to work on features of homoclinic tangles, such as the signature of Birkhoff. (This work was not published until 1985.) In the Spring, I accepted an offer from Princeton.

• 1964-65. My first year at Princeton, I was assigned a course called Honors Calculus. The 15 students were extremely good, most of them followed my lectures for four years, then went on to graduate school and became professors. Harold Abelson, Michael Buchner, Len Fellman, Carl Morgenstern, and Lee Rudolph were in this group. At this time, Smale was in Geneva, writing his paper on the horseshoe map. (Smale, 1965)

• 1965-66. My second year at Princeton. I was asked by Arthur Wightman of the physics department to offer a course in mechanics, including the new results of Kolmogorov, Arnold, and Moser. The course began in February, and attracted an excellent audience of visitors and graduate students, including Jerrold Marsden. By the end of July, Marsden's lecture notes had become the manuscript of a book, *Foundations of Mechanics*. At the same time, Joel Robbin's notes of my lectures for another graduate course became a book, *Transversal Mappings and Flows*. In addition, Thom was sending me draft chapters for his book on structural stability and morphogenesis. Through Wightman, I arranged to have Thom's book published by Bill Benjamin.

• 1966-67. Most of this year was spent on sabbatical in Paris, where I renewed my friendship with Thom, and met David Ruelle and Harold Levine. Smale's very influential article on his program for dynamics appeared in the *Bulletin of the American Mathematical Society*, in which he introduced basic sets and strange attractors.

• 1967-68. My last year in Princeton. After a party with some of my great undergraduate students, I was passing outside my ground floor office on the way home when I heard the phone ringing. I ran in to find Steve Smale calling from Berkeley, with a hypothesis on generic properties of dynamical systems. A counterexample came immediately to mind, which we presented in a joint paper in the climactic event of the golden years, the Summer Institute on Global Analysis of the American Mathematical Society, held in Berkeley in 1968. (Chern and Smale, 1970) Following this event, I moved to the University of California at Santa Cruz.

• 1971. A conference in Brazil brought many of these people together during the twilight of our golden age. At this time I noted the beginning of a backlash movement against Thom and catastrophe theory. (Peixoto, 1973)

• 1990. The cast of characters met in Berkeley for a reunion in honor of Smale's 60th birthday. (Hirsch, 1993)

• 1998. Another reunion, this time in Cincinnati, in honor of Bob Williams' 70th birthday.

5. The chaos revolution: 1968-1998

An amazing aspect of our work during the early 1960s was our total ignorance of the experimental work on chaotic attractors of Yoshisuke Ueda in Kyoto, from 1961, Edward Lorenz in Cambridge, from 1963, and Christian Mira in Toulouse, from 1965. Ueda, in particular, had painstakingly drawn the homoclinic tangle which formed the skeleton of his chaotic attractor.

I do not remember how or when this exciting news penetrated our circle. For myself, I believe that during my Princeton course on mechanics in the Spring of 1966, I began to hear about chaos in the solar system, and later, about the Lorenz attractor. But certainly by the 1968 Berkeley Summer Conference of the American Mathematical Society, we all knew that our particular line of work had come to a halt, for our favorite hypotheses were not satisfied by the chaotic attractors recently discovered by the experimentalists. At this time, many of us turned to applications, in an effort to reground and to reorient our work. Personally, I was excited by the work of René Thom on catastrophe theory. In this context, Thom had introduced the basic concepts of a radically new direction in applied mathematics, with his ideas of attractors and basins, and catastrophic bifurcations. I went to Paris (that is, to IHES, the Institut des Hautes Études Scientifiques in Bures-sur-Yvette) to study with him in 1967, and again in 1972.

It was during these years, after I had moved from Princeton to Santa Cruz in 1968, that the chaos revolution began in earnest. The crux, I believe, was the work of Takens and Ruelle on turbulence. Fluid dynamical turbulence, before chaos theory, was an embarrassment to theoretical physics, so it was downloaded to the engineers. Floris Takens had finished his thesis on dynamical systems theory (Berkeley style) in Holland in 1968, and went to IHES for a year. There he met the resident theoretical physicist, David Ruelle, a uniquely capable mathematician. Together they made a new model for the onset of turbulence in fluids through a sequence of bifurcations. Their paper was rejected by various journals as heretical, but finally, in 1971, it appeared in a journal of which Ruelle himself was an editor. And that, I believe, was the turning point: chaos theory triggered the chaos revolution, a sequence of paradigm shifts in the various branches of science. First physics, then astronomy, chemistry, biology, medical physiology, and then economics, and the social sciences.

In 1972, Thom had introduced me to the work of Hans Jenny of Basel, showing fluid dynamical forms created by vibrations in transparent liquids and powders. I hastened to Basel to meet Jenny, who showed me films and photos of his results. In 1974 I returned to UCSC from Amsterdam, India, and the casinos of Nevada, to initiate a program of research on vibrations, chaos, and spatial bifurcations in transparent fluids. In the student machine shop, I built a device, modeled on that of Jenny, the Jenny Macroscope. Along with ideas of Thom and Zeeman, it contributed greatly to my marriage of ideas from chaos theory and from Hindu cosmology. A number of students worked with me on this project, Paul Kramerson in particular. At this time, through Terence McKenna, I met Erich Jantsch, who encouraged me to record my ideas in articles for his books.

In 1975, just as Li and Yorke were bringing the word *chaos* into the picture, I turned completely towards experimental and applied chaos theory. The trigger was the arrival on our campus of the newly developed computer graphic device, the Tektronix 4006 "green screen." This new direction got a boost from Richard Palais, who wrote a BASIC program for dynamical systems research, ORBIT, during his brief stay in Santa Cruz. Our program, the Visual Math Project, was supported by state and federal funds until 1982. It was aimed primarily at supporting the lower division math courses with interactive computer graphics, for which we created extensive software in C. But by 1983, the Visual Math Project had morphed into a thriving graduate program in applied and computational mathematics (read chaos theory) at UCSC. This program came to an end with my early retirement from UCSC in 1994.

During the same period, the 1970s and 80s, an autonomous group of graduate students in the physics department, later known as the Santa Cruz Chaos Cabal, began publishing significant experimental results. A meeting at the New York Academy of Sciences in 1979 brought many of the chaos pioneers together for the first time. An historic summer school on chaos theory in Les Houches, in 1981, brought chaos theory to the attention of the international physics community. And during these years, I created, with Chris Shaw, the lengthy picture books of dynamical systems theory, *Dynamics, the Geometry of Behavior.* As my usual publisher, Addison-Wesley, refused to publish these books in four colors as I wanted, Aerial Press sprang into existence to self-publish them. Time proved Addison-Wesley correct, as Aerial income was rather less than out-go.

It was in medical physiology, from 1978 to 1986, that I made my first serious efforts to make a difference with chaos theory. Walter Freeman (neurophysiology) and Gene Yates (endocrinology) were among my early collaborators. But it was the enormous popularity of Gleick's book, *Chaos*, from 1987 to about 1990, which brought me into contact with scientists from many fields, most notably, Richard Goodwin and his group in dynamical economics, Ervin Laszlo and his General Evolution Research Group (GERG), and William Irwin Thompson and his Lindisfarne Association. At a conference on Goodwin-style economics in 1991, I met Christian Mira and Laura Gardini, and heard for the first time of the method of critical curves, which became the subject of our joint book in 1997.

I might end here with a list of some of the paradigm shifters whose stories I knew first hand, as a result of this brief wave of popularity, or otherwise. I am not sure of all the dates.

- Mauricio Peixoto, structural stability theory, 1960
- Yoshisuke Ueda, electrical engineering, the first sighting, 1961
- Christian Mira, control theory, 1964
- Rene Thom, math, catastrophe theory, 1966
- David Ruelle and Floris Takens, physics, turbulence, 1968
- Otto Rossler, chemistry, taffy pulling, 1974
- Erich Jantsch, systems theory, 1975
- Christopher Zeeman, math, catastrophe theory, 1977
- SC Chaos Cabal, physics, attractor reconstruction, 1978

- Eugene Yates, endocrinology, 1979
- Walter Freeman, neurophysiology, olfactory bulb, 1979
- Alan Garfinkel, medical physiology, heart muscle, 1979
- Hermann Haken, physics, synergetics 1980
- Eugene Yates, medical physiology, 1983
- Benoit Mandelbrot, math, fractal geometry, 1985
- Michael Mackey and xx Glass, medical physiology, 1985
- Heinz-Otto Peitgen, math, fractal geometry, 1985
- Ervin Laszlo, social dynamics, 1985
- Fred Abraham, Psychology, 1988
- Kate Hayles, literature, 1990
- William Irwin Thompson, history, 1990
- Herbert Shaw, Earth Sciences, 1991
- Richard Goodwin, economics, 1991
- Robert Langs, Psychiatry, 1993

6. Conclusion

Triggered by a mathematical discovery, the Chaos Revolution is a bifurcation event in the history of the sciences, comprised of sequential paradigm shifts in the various sciences. Perhaps it is also a major transformation in world cultural history: time will tell. Meanwhile, we are struck with the personal observation of the similarity in the sociological and psychological experiences of th various pioneers who have suffered from the novelty or their ideas, and the bravery of their convictions. We are deeply in their debt.

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