4. My Encounter with Chaos*

Yoshisuke Ueda

Kyoto University

By the Time It's Popular, It's Too Late

It has been several years since many people began to give me credit for discovering Chaos, claiming that the output data I got from my old analog computer on Nov. 27, 1961 was in fact one of the earliest examples of chaos, and that the theory of the randomly transitional phenomenon I had proposed was in fact Chaos theory. But while I was toiling alone in my laboratory, I was never trying to pursue such a grandiose dream as making a revolutionary, new discovery, nor did I ever anticipate writing a memoir about it. I was simply trying to find an answer to a persistent question, faithfully trying to follow the lead of my own perception of a problem. Was it really such a great feat? I still am not sure. In any case, when I asked myself this question in 1979, everything about it was already a thing of the past. It is true that some friends and acquaintances had warned me earlier: "You're still messing around with van der Pol and Duffing? It's about time you quit!" For myself, however, persistence seemed to have paid off. My advice, then, to the young readers of this memoir is to faithfully pursue your own questions, and build solid work day by day, without giving in to peer pressure. Many scientists all over the world flock to popular research topics

* This Memoir was translated by Mrs. Masako Ohnuki and Dr. H. Bruce Stewart from the article in Japanese published in the *Transactions of the Institute of Electronics, Information and Communication Engineers of Japan,* Vol. 77, May 1994, with kind permission of the Institute.

of the day, but one should realize that by the time they become popular, it may already be too late for newcomers to begin working on them.

The Oldest Attractor was a Result of Perseverance, Not of Wisdom

When I entered graduate school, Professor Hiroshi Shibayama (presently an Emeritus Professor at Osaka Institute of Technology) encouraged me to work on synchronization phenomena. Synchronization occurs when periodic (external) signals with a frequency different from that of self-oscillation are injected into an oscillatory circuit. The self-oscillatory frequency can be entrained by the external frequency, and becomes synchronized with it. If synchronization is achieved, periodic oscillation appears; if it fails, a beat oscillation is sustained.

Prof. Shibayama was a research fellow at the time who came to our laboratory several times a week and helped me with my project. He was a very kind man who worked meticulously, although he did not force others to follow suit. I tried to follow his example and work as carefully as possible. I am grateful, as I realize now what he had given me was of lasting value. He was working on synchronization based on Van der Pol's equation, to which a term for a forced sinusoidal (external) frequency was added:

$$\frac{d^2x}{dt^2} - \mu(1-\beta x-\gamma x^2)\frac{dx}{dt} + x = B\cos\nu t \qquad (1)$$

Deriving an averaged equation that approximates the equation (1), its point of equilibrium and limit cycle were obtained numerically using a Tiger Calculator (the commercial name for the most popular mechanical desk-top hand calculator of the day). In this approximation, the point of equilibrium represents the periodic oscillation, while the limit cycle represents the best oscillation. Our main job was to compare the approximate results with those of the original equation obtained through an analog computer.

In the analog computer experiments, the periodic oscillation shifts to a beat oscillation when the amplitude B and frequency ν are moved outside of the boundary of frequency entrainment. The boundary of frequency entrainment was drawn on the (B, ν) plane for each of the numerous (B, ν)

values in this manner. Today it is well known that the limit of entrainment is represented by a saddle-node bifurcation (fold bifurcation) if the external amplitude B is small, and by a Hopf bifurcation if it is large. In those days, however, these phenomena had to be carefully tracked down with Tiger Calculators and low-speed analog computers. The analog computer experiments were extremely cumbersome and took an infernally long time—almost impossible to imagine today when computer technology is so highly advanced. Our work in those days proceeded as follows: first, you would switch on the analog computer and wait for 20 to 30 minutes until the vacuum tubes heated up and a stationary state was achieved. Next, you would carefully perform a drift adjustment on each of the operational amplifiers. Since the multiplier was operated by a servo mechanism, there would be a time delay if you quickened the response speed. Consequently, the period could not be any shorter than 6.28 seconds even in the case of nearly pure-sinusoidal waves with very little higher harmonic components. During the calculations, the chopper amplifier occasionally balked and had to be coaxed back to show normal wave patterns by cleaning the contact points. Then, at the crucial moment, the thread holding the recorder pen would break! If you sent it out for repair, it would take a few days, so you had to fix it on the spot yourself. If you were unlucky, it took half a day just to repair it. If you were using a recorder with a fountain pen, it was absolutely necessary to wash the tip of the pen after each experiment. And in order to prevent others from using your clean pen, you needed to hide it.

In those days the University used to cut off direct current at 8 p.m. sharp. Since a part of our analog computer was powered by DC, we had to rush to the substation around 7:50 to plead for a time extension. The old man at the substation got to know us rather well, and did not seem to mind our frequent requests..... So this was the way we worked.

By the time I entered the Ph.D. program, I was working on a forced self-oscillatory system containing a nonlinear restoring term—in other words, a Van der Pol/Duffing mixed-type equation:

$$\frac{d^2x}{dt^2} - \mu(1-\gamma x^2)\frac{dx}{dt} + x^3 = B\cos\nu t \qquad (2)$$

I used this equation because when one chose an intermediate value of external amplitude B, that was neither too large nor too small, both the periodic and beat oscillation would coexist, thus making the boundary of entrainment extremely complex, causing hysteresis and jump phenomena. The parameter range within which such phenomena could occur was too narrow in the case of equation (1) for analog computer experiments. But with equation (2), analog computer experiments were possible because the response curve had a greater incline, thus widening the parameter range. Thus it was through these analog computer experiments, based on equation (2), when I was studying the hysteresis between periodic and beat oscillations, that I happened to come across, what is reputedly the earliest observation of chaos (in a non-autonomous system, to be exact).

The beat oscillation in equation (1) is called "almost periodic oscillation." Stroboscopic sampling (observation of a representative point once during each cycle of the external signals) filled out a smooth closed curve very similar to the approximating limit cycle. In contrast, when the beat oscillation based on equation (2) was viewed by stroboscopic sampling, a jagged shape resembling a broken egg shell appeared. The first time I saw it, I attributed it to malfunction of my analog computer. But within several days, I had no doubt that the computer was normal and that whenever the external parameters B and ν were outside of the boundary of entrainment, this annoying and perplexing shape would appear. After spending several weeks staring day and night at the output data from my analog computer, I felt I could hear the phenomenon itself (or at least the fact revealed through these computer experiments) insisting that "the result can't be any other way but this!" In this way came the answer to my long-held question. "What are the steady states of a nonlinear system?" It just came to me—I did not plan it. I just got the answer by staring at the data for a long time.

After the hysteresis between the periodic and beat oscillation based on equation (2) was largely understood, I became concerned about further bifurcations that occur only in the system based on equation (2), but not on (1). (These bifurcation occur if the angular frequency ν of the external force is smaller than the self-oscillatory angular frequency.) In this case, once again, the parameter range based on equation (2) would become too narrow for analog computer simulation. The equation I found (or rather, created) was a Rayleigh/Duffing mixed-type equation:

$$\frac{d^2x}{dt^2} - \mu\left[1 - \gamma^2\left(\frac{dx}{dt}\right)^2\right]\frac{dx}{dt} + x^3 = B\cos\nu t \qquad (3)$$

Using this equation, I repeated the averaging and the analog computer experiments. It proved to be an extremely cumbersome problem. The phenomena were too complex to produce a satisfactory approximation using a single frequency component. Moreover, it was difficult to distinguish between transient and steady states because the effect of damping was almost negligible. This made it hard to draw the limit cycle of the averaged equation using the isocline method. And the results of analog computer experiments were not reliable. What more or less saved us was the new digital computer KDC-1, which had just come out.

When the external frequency was greater than that of the self-oscillatory frequency, a phenomenon similar to that of equation (2) was observed in the system based on equation (3). The illustration in the Transactions of the Institute of Electronics and Communication Engineers of Japan (53-A, p.155, Fig.6) describes the two types of beat oscillation that occur in a system based on equation (3): the almost periodic oscillation, and the strange oscillation (chaos).

Although this is out of sequence, I must mention the following Duffing equation, which was thrust upon us during our work on equations (2) and (3):

$$\frac{d^2x}{dt^2} + k\frac{dx}{dt} + x^3 = B\cos t \tag{4}$$

Equation (4) was the trademark of Prof. Chihiro Hayashi's laboratory. I cannot pinpoint the exact date of my encounter with chaos within this system: it occurred sometime during the latter semester of my fourth year in graduate school (between the fall of 1962 and the spring of 1963). Prof. Hayashi was at the time writing a book to be published by McGraw-Hill. Rather than work in his office, he was sitting in the laboratory, frequently calling on his staff and students for assistance. Each chapter was sent to the publisher as it was completed. Chapter 6, entitled "Higher Harmonic Oscillation," was returned to him with a reviewer's criticism. In order to counter this, it was necessary to draw the amplitude characteristic curve using the harmonic balance method, and then confirm the result with the analog computer without delay. This urgent task fell on my lap. It was a tough job, especially under pressure of time, but somehow I completed it for the deadline. During the course of this work, I again encountered a whole slew of chaos. These studies were the origin of the strange attractor which

Prof. D. Ruelle of the Institut des Hautes Etudes Scientifiques later called "The Japanese Attractor."

The experience with this Duffing equation made me confident that chaos was not just a freak phenomenon unique to a forced self-oscillatory system (a system that sustains itself oscillation after the periodic external force has been removed), but one that also occurs in forced oscillatory systems—in other words, rather than being a variation of an almost periodic oscillation, it was a phenomenon in itself.

Professor's Principles and Mutual Trust

In writing this memoir, I cannot but touch on Prof. Hayashi's work habits, and the standards he maintained in his work. He was thorough, meticulous and strict. I had a deep respect for his care and perfectionism in experiment. He also had an extraordinary passion for the aesthetics of the diagrams in his papers. It was I who took the direct onslaught of this obsession. First I resisted it, but after a long exposure to this habit, it has become my second nature. Early on, partly being proud of Prof. Hayashi's trust in my work, I even took on drawing diagrams for other people's papers and books totally unrelated to my own work. As I got older, however, I realized it was the most inefficient use of my precious time. Even so, I did not protest and kept drawing diagrams for Prof. Hayashi albeit begrudgingly until his retirement. I did lose a lot of my time for his principles, but what I gained from him amply compensates it.

I absorbed Prof. Hayashi's principles by osmosis so to speak—so much so that I believed him without questioning when he told me about his experimental observation of a sustained transient condition where subharmonic oscillation with higher harmonic components appears, as the voltage is increased. He was telling me, in essence, "chaos was observed in an actual circuit," although he himself never accepted the concept of chaos.

Publication of My First "Chaos" Paper

The fact that work long pursued by a single researcher has gained recognition only after twenty-some years might seem to imply a lack of awareness or interest by other scientists. During my long pursuit of the subject, I never hid my data from others or kept them secret. On the contrary, I kept reporting my results and observations in papers as well as at scientific meetings. In discussing the subject with others, however, I often found myself retreating after failing to convince my powerful opponents. I was even subjected to reprimands. Eventually I resigned myself to believing that meetings weren't important as long as I understood what I believed to be true, and that no one would lend an ear to a powerless research fellow's opinions until the right time arrived. This was my feeling in 1972 when I submitted my second "chaos" paper, proposing my understanding of randomly transitional phenomena (chaos) to the Transactions of the IECE whose motto was to accept papers both worthy and seemingly unworthy (the Trans. of IECE, 56-A, pp.218-225, 1973). My only ally in this chaos research was Norio Akamatsu (presently a Professor of Engineering, School of Engineering, Tokushima University) who was a graduate student at the time.

The first person who gave recognition to my chaos research was Hiromu Momota (associate professor, Kyoto University, School of Engineering at the time: presently a professor at the Nuclear Fusion Science Laboratory). It was in the early morning hours (3 a.m.) of March 3, 1974, the first day of entrance examination in the midst of the student unrest. Ten or so of us faculty were captured by the students during the meeting of associate professors and lecturers, and were forced to spend the night locked up in a classroom. Since we had nothing to do, we decided to talk about the work each of us was doing at the time. Impressed by my work on chaos, Momota encouraged me. I was delighted. He later introduced me to the Plasma Laboratory at Nagoya University. My liaison with the Laboratory eventually opened the way to our cooperative work in computer use for experiments. Although Kyoto University also had computers, we could not use too much computer time there, since our funding was scarce. At Nagoya, I could use the computers to calculate the power spectra of chaos. This work was published in the Transactions of the Electrical Engineers of Japan (98-A, pp. 167-173, 1978).

Since Chaos Became Fashionable

So far I have written of the dark hours before chaos was universally recognized. After that, however, I have had many happy experiences. Thanks to the popularity of chaos, I can now write a memoir like this.

When I was asked to recount my experience, I wasn't sure if I had anything left to say, since I had poured everything into my invited speech at the 1991 International Symposium, "The Impact of Chaos on Science and Society," organized by the United Nations University together with the Tokyo University; the Proceedings of the Symposium will be published soon. I decided to write this piece, however, at the urging of the former chairman of the Non-Linear Problem Study Group of this organization, Prof. Masami Kuramitsu, who reminded me that it was the duty of us old timers to show the reality of scientific research to young scholars who are eager to do good work, but who are unsure how to proceed. I have tried not to duplicate what has already been published.

The reference material for this memoir is included in my book entitled *The Road to Chaos* (published in 1992 by Aerial Press, P.O. Box 1360, Santa Cruz, CA 95061).

In closing, I would like to express my deep gratitude to the editorial members of this journal for their effort in planning and publishing this memoir.