3. Strange Attractors and the Origin of Chaos*

Yoshisuke Ueda

Kyoto University

Prologue

I am greatly honored to have been given this wonderful theme, "Strange Attractors and the Origin of Chaos" for my presentation today. First I would like to take this opportunity to offer a special thanks to each one of the people who planned and made this symposium possible.

At present, people say that the data I was collecting with my analog computer on the 27th of November, 1961, is the oldest example of chaos discovered in a second-order non-autonomous periodic system. Around the same time, it was Lorenz who made the discovery of chaos in a third-order autonomous system.

At that time I was simply frustrated with this seemingly mysterious phenomenon which I accidentally came upon during my experiments. For my part, it was nothing as glorious as an act of discovery—all I did for a long period of time was to keep on pursuing my stubborn desire to understand this unsettling phenomenon. "What are the possible steady states of a nonlinear system?"—this has always been my question. And my paradigm has always been the phenomena themselves, not papers with their abstractions, but something we can actually observe or quantify.

* This article was first presented at the international symposium entitled "The Impact of Chaos on Science and Society," where I was invited as a guest speaker. The symposium was organized by the United Nations University and the University of Tokyo, and held in Tokyo between 15-17 April, 1991.

In this report, I would like to reflect upon the circumstances of my research and the general conditions of Japanese science around 1978 before the study of chaos began. As I prepared this talk, I kept asking myself what propelled me to pursue my question so relentlessly, but I must confess that I don't know the answer. I have not, in my wildest dreams, imagined that I would be given an opportunity to speak on this very subject. It was so unexpected, my mind was reduced to total chaos!

As an academic term, I do not find the word "chaos" very appropriate. But what shall we call it then? My proposal has been "randomly transitional phenomena"; I will explain this below.

The characteristics of chaos in a physical system can be summarized as follows:

Random phenomena that occur in deterministic systems.
Random phenomena whose short-term behavior is predictable.
Random phenomena whose long-term behavior is unpredictable.
Although the phenomena are irregular and unpredictable, chaos does have a definite structure.

The original meaning of chaos, I feel, is a "total disorder and ultimate unpredictability." But as scientific terminology, the word "chaos" seems to overemphasize the unpredictability alone. This symposium provides an opportunity to clear this misunderstanding and to inform the non-specialist of the correct meaning of the word.

Even so I have to use the word "chaos" here, instead of "randomly transitional phenomena." It is a concise expression which has already filtered into people's minds, and therefore I have decided it is rather pointless to resist it.

The Oldest Chaos in a Non-Autonomous System—A Shattered Egg

The 27th of November, 1961 became a memorable day for me, although I did not have any joyous realization that something wonderful had happened, or any vivid memory of the events of that particular day. As I remember, I had just finished writing the narrative to accompany the data I was going to publish at the Special Committee on Nonlinear Theory of the Institute of

Electrical Communication Engineers, to be held on December 16th, and was carrying out some analog computer experiments with the help of Susumu Hiraoka, who was two years my junior, in order to test the applicability of the approximate computation I quoted in my paper. Had I not had the date on the printout of that old analog computer, which was destined for a wastebasket, I would never have been able to recall the date (Fig. 1).

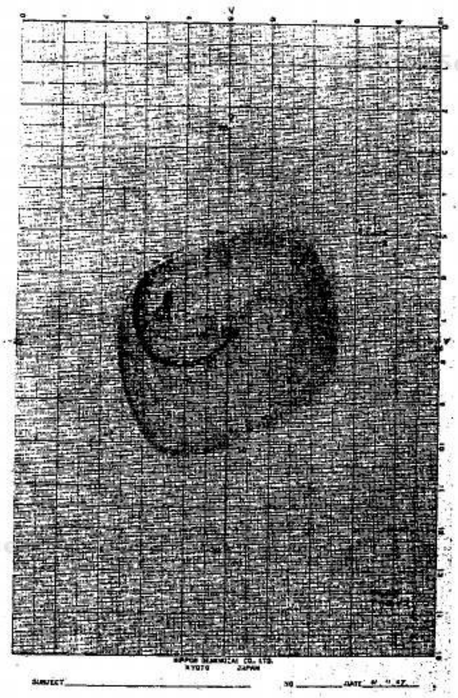

Fig. 1 Output of an analog simulation of the equation

$$\ddot{v} - \mu\left(1 - \gamma v^2\right)\dot{v} + v^3 = B\cos\nu t \qquad (1)$$

with $\mu = 0.2$, $\gamma = 8$ and $B = 0.35$ obtained on 27 November 1961 is shown. A continuous orbit is drawn lightly on the $v\dot{v}$ plane and points in the Poincaré section at phase zero are given by heavy dots; five dots near the top are fixed points for a sequence of values at $\nu = 1.01, 1.012, 1.014, 1.016$

and 1.018, the remaining points are on the chaotic attractor at $v = 1.02$. (Courtesy of Dr. H. B. Stewart, Brookhaven National Laboratory)

At that time, I was a third-year graduate student at Kyoto University, working on the phenomenon of frequency entrainment under the guidance of Professor Chihiro Hayashi. When a circuit (oscillator) which would, if left alone, keep on generating an electrical (self-sustained) oscillation with a certain frequency and amplitude, is driven externally with signals whose frequency is different from that of the self-oscillator, its self-oscillating frequency is drawn to and synchronized with that of the driving frequency. This phenomenon is called frequency entrainment. There are exceptions, of course—depending on the value of the driving frequency and amplitude, entrainment sometimes does not occur. Instead, an aperiodic "beat oscillation" with drifting frequencies would appear.

I was receiving direct guidance from Professor Hiroshi Shibayama of Osaka Technological University, who was visiting our laboratory as a guest scholar several times a week. Warm and gentle, he let me do whatever I wanted to do, while introducing me to the basics of the research. Even now I look up to him as my senior friend and receive all kinds of advice.

The main purpose of my computer experiment was to simulate the non-autonomous nonlinear differential equation describing frequency entrainment, and to examine the range of the frequency and amplitude of the driving signals which cause synchronization, as well as the amplitude and the phase of its oscillation. Allow me to go into a little technical detail here. The approximate computation was done by rewriting the non-autonomous equation into an autonomous equation using the averaging method (approximation enters the process at this stage, with the result that chaos is suppressed). The aim was to approximate the steady state of the original system with an equilibrium point or limit cycle of the autonomous equation. In this method, the stable equilibrium point corresponds to synchronized frequency entrainment, and a stable limit cycle corresponds to asynchronous drift conditions. Actually there are two kinds of asynchronous oscillations—quasi-periodic oscillation (represented by a limit cycle in the averaged equation) and chaotic oscillation (represented by strange or chaotic attractors): but the common sense of the day failed to recognize the chaotic oscillation. In those days only the equilibrium point and limit cycle were known to exist as steady states of a (second order) autonomous system, so it was understandable for everyone to have possessed the preconceived notion that asynchronous condition meant quasi-periodicity. On that day, the 27th of November, when I changed the parameter (frequency of the driving input), and the condition shifted from frequency entrainment to asynchronization, the oscillation phenomena portrayed by my analog computer was chaotic indeed. It was nothing like the smooth oval closed curves in Fig. 1, but was more like a broken egg with jagged edges. My first concern was that my analog computer had gone bad. But I soon realized that that was not the case. It did not take long for me to recognize the mystery of it all—the fact that during asynchronous phase, the shattered egg appeared more frequently than the smooth closed curves, and that the order of the dots which drew the shattered egg was totally irregular and seemingly inexplicable. As I watched my professor preparing the report without a mention of this shattered egg phenomenon, but rather replacing it with the smooth closed curves of the quasi-periodic oscillation, I was quite impressed by his technique of report writing. But at the same time, I realized that one needs to be very careful in reading reports of this sort [1].

I am getting a little sidetracked here, but the analog computer had been developed and created as his research project by Minoru Abe who was three years my senior. My deep respect goes to him who so laboriously and meticulously handbuilt this practical computer with vacuum tubes. Figure 2 is an example of the waveform data made by the computer. It reminds me of the long hours patiently sitting in front of the analog computer, and of my wonder at its accuracy—a testament to its maker's unique skill. To obtain a sheet of data as shown in Fig. 2, it took the computer about 60 to 100 minutes. Most of them have been discarded, but we had accumulated at least 1000 sheets of data during my five years in graduate school. I would like to mention that I had not contributed very much in creating this computer beyond helping him build several operational amplifiers and learning to repair the chopper amplifier and the recorder. As I look back, I feel that after those long exhausting vigils in front of the analog computer, staring at its output, chaos had become a totally natural, everyday phenomenon in my mind. People call chaos a new phenomenon, but it has always been around. There's nothing new about it—only people did not notice it.

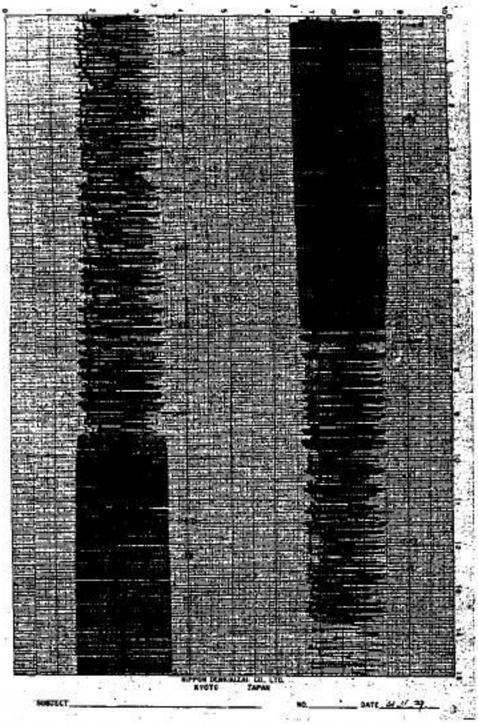

Fig. 2 Waveforms of an analog simulation of Eq. (1) with varying v are shown for $B = 0.3$. A pulse on the waveform which is generated by an auxiliary circuit independent of the main analog computation circuit for Eq. (1) shows a time mark at every period of the external periodic forcing; when this pulse sequence forms a straight line (after transient), we can infer that periodic motion appears with the system being entrained by the external periodic signal. These data progress from entrained state to asynchronized state and vice versa exhibiting a narrow hysteresis zone. (Courtesy of Dr. H. B. Stewart, Brookhaven National Laboratory)

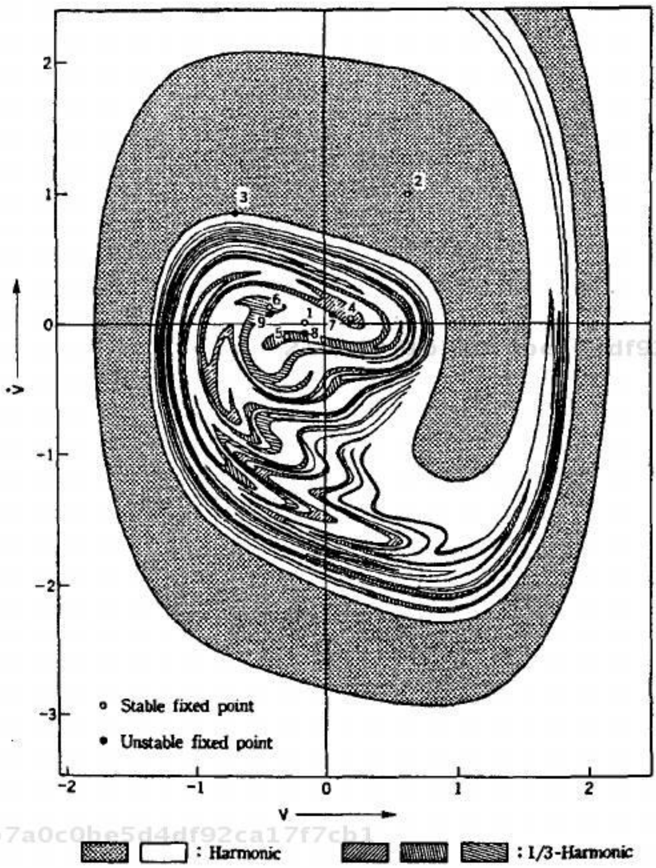

Fig. 3 Domains of attraction for harmonic and 1/3-harmonic responses. Points 1 and 2 show fixed points representing non-resonant and resonant harmonic oscillation, respectively, and point 3 is a saddle on the basin boundary. Points 4, 5 and 6 are completely stable 3-periodic points representing 1/3-harmonic oscillation. This attractor-basin phase-portrait is obtained by exhaustive checking of initial points on the $v\dot{v}$ plane by executing analog computer experiments for the Duffing equation

$$\ddot{v} + k\dot{v} + v^3 = B\cos t \tag{2}$$

with $k = 0.1$, $B = 0.15$.

(Reproduced with the courtesy of McGraw-Hill Book Company and Nippon Printing and Publishing Company [2, 3, 17])

When I think of those long hours in front of the analog computer, I also think of my classmate, Toshiaki Murakami. He had completed his master's degree and came to work in April of 1961. During our graduate school days, we used to compete for the use of the analog computer. He and his instructor, Yoshikazu Nishikawa, four years ahead of us, used to keep long vigils, too, painstakingly examining point by point the domain of the initial conditions of the Duffing equation. Their main results appear in Fig. 10.6 in [2], and Fig. 4 of [3] in the bibliography, reproduced here as Fig. 3. In Figure 3 of [3], reproduced below as Fig. 4, we can clearly see the curve (alpha-branch) which swirls down from the saddle point 3 on the basin boundary to point 1. At the time, the pertinence of the figure was controversial among the specialists. Dr. Hayashi himself voiced his doubts and did not include it in his book published by McGraw-Hill in 1964. I will discuss this point later in "From the Harmonic Balance Method to the Mapping Method."

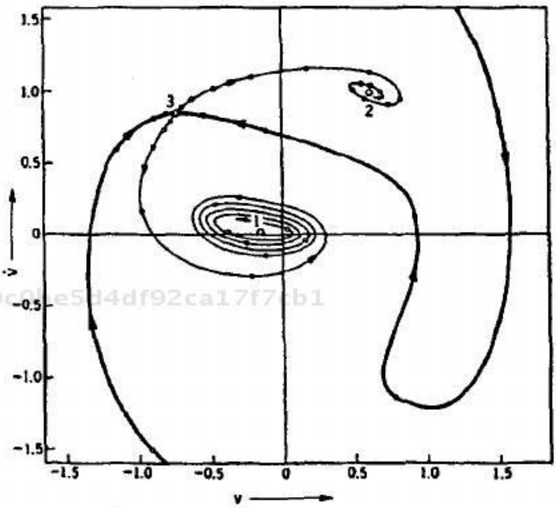

Fig. 4 The loci of some image points on the invariant curves of the directly unstable fixed point 3, for the same equation and parameter values as Fig. 3.
(Reproduced with the courtesy of Nippon Printing and Publishing Company [3, 17])

In the research data [1] published in December of 1961, the saddle-mode point corresponding to the boundary of the frequency entrainment domain was plotted on the limit cycle, but a careful examination proved that it was actually not on the limit cycle. This error was corrected immediately after the publication.

The fact that these data survived was by itself a miracle. It symbolizes my packrat tendency—a typical trait for those who grew up during the war. Naturally I cannot verify the dates for those which were not dated at the time, but the following account can be corroborated from other data such as those of the nonlinear research group, to within a few months. I will touch upon the original data at the time of the experiments later in "The Original Data that were Preserved."

Thus ended 1961. In 1962, the nonlinear oscillation group of Hayashi students was studying frequency entrainment phenomenon of the self-oscillatory system with driving periodic signals (forced self-oscillatory system) and the oscillatory phenomenon of a system which holds a steady equilibrium if no external periodic signals are added (forced oscillatory system). The former research centered around Prof. Shibayama, focusing mainly on the analyses of Van der Pol equations with added forced terms, or of a mixture of Van der Pol and Duffing equations, while the latter, the study on the Duffing equation, centered around Prof. Nishikawa.

Encounter with the Japanese Attractor

One day in the fall or winter of 1962, Prof. Chihiro Hayashi was waiting for me in the laboratory room. At that time he was working on the manuscript for his forthcoming book [2] which was eventually published in 1964 by McGraw-Hill. The laboratory room was probably more convenient for his work: he left his office vacant and occupied the laboratory room. "I want you to do this computation in a hurry," he told me. It was to solve third order simultaneous equations with four unknowns and draw the amplitude characteristic curve of the periodic solution of the Duffing equation, taking into consideration the components up to the third harmonic component. Let me explain the circumstances of Prof. Hayashi's request. When he sent chapter 6 of his book to McGraw-Hill, it was returned with an objection from a reviewer. The contents of chapter 6 were exactly the same as chapter 3 of his earlier book (1953) [4]. The reviewer, as I recall, was Prof. Higgins of the University of Wisconsin. In the book, Prof. Hayashi showed the amplitude characteristic curve for the periodic solution represented by cosine components alone, of the Duffing equation with zero as a damping term, (Fig. 5), but not the curve with non-zero damping coefficient. There was a reason for this. When the damping coefficient is not zero, sine components become necessary in the periodic solution, in addition to the cosine components, thus doubling our workload. It was particularly difficult on the hand-cranked calculator. I was somehow trusted by Prof. Hayashi as someone having fast and accurate computation and careful drawing skills. So on that occasion, I felt quite proud of myself and accepted the challenge, knowing it was a difficult job. Even if I did not want to do it, I would not have been able to refuse him anyway. I was a little amused to find this most exalted professor high up in the clouds a bit flustered, especially with the objection written in English no less!

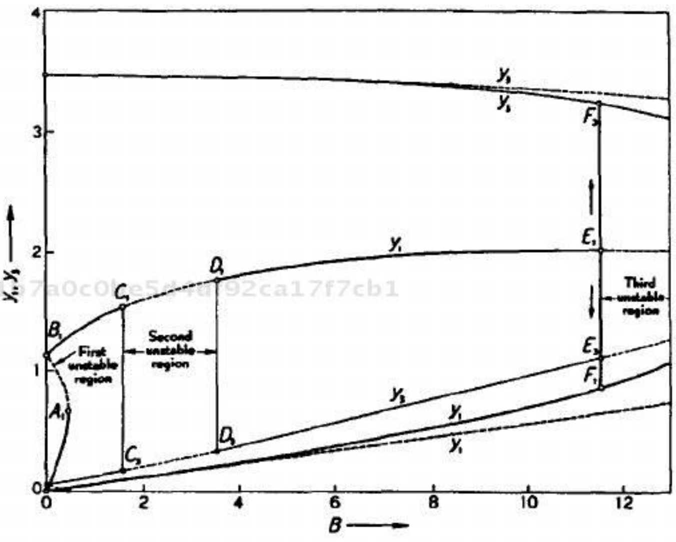

Fig. 5 Amplitude characteristic of the periodic solution

$$v = y_1 \cos t + y_3 \cos 3t \qquad (3)$$

of the Eq. (2) for $k = 0$, obtained by using harmonic balance method. (Reproduced with the courtesy of Nippon Printing and Publishing company [4])

As it was impossible to solve the third order simultaneous equations with four unknowns by sheer brute force, I eliminated two variables from the four expressions to lead to high order simultaneous equations with two unknowns, gave the amplitude of the periodic solution ahead of time, with a bit of a "manipulation," returned to the computation of the amplitude of the external force, and finally, by hand calculations, solved approximately fifty cases of third order equations with the Newton-Raphson method, and drew the curves shown in Fig. 6 (Fig. 6.1 in the reference [2]). I fondly remember this crash project completed within a few weeks. The damping coefficient was set at 0.2.

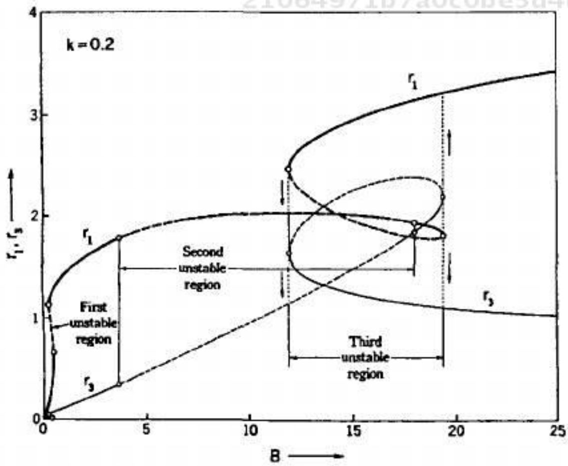

Fig. 6 Amplitude characteristic of the periodic solution

$$v = x_1 \sin t + y_1 \cos t + x_3 \sin 3t + y_3 \cos 3t$$

with $r_1^2 = x_1^2 + y_1^2, r_3^2 = x_3^2 + y_3^2$, of the Eq. (2) for $k = 0.2$.
(Reproduced with the courtesy of McGraw-Hill Book Company [2])

The amplitude characteristic curves drawn with only two frequency components contained in the periodic solution, are nothing but an approximation. The standard procedure, therefore, was to verify the results with the analog computer. I struggled again for several days in a row, in front of the computer. When the amplitude of the external force increased, the high frequency components in the periodic solution also increased, accelerating the response time, thus making it extremely difficult for the servo multiplier—that represented the nonlinear term—to follow up. Consequently, one had to extend the computer's time scale so as to slow down the response time. I repeated the experiment with one cycle of the external force 2π corresponding to 31.4...seconds. During the process, I encountered enough chaotic oscillations (the source of the Japanese attractor) to make me sick. But Prof. Hayashi told me, "Oh, it's probably taking time to settle down to the subharmonic oscillations. Even in an actual series resonance circuit, such a transient state lingers for a long time." When I look back, though, I seemed to have sensed at that time that chaos was not a phenomenon unique to forced self-oscillatory systems in which quasi-periodic oscillation appeared.

Only a fragmental record of the original data of my work with the analog computer and manual calculation remains now. I may have been worried at that time that, had Prof. Hayashi seen this data, he would have told me to repeat the analog computer experiments until the transient state settled to a more acceptable result. Sensing that no matter how long I continued the simulation, I would never be able to come up with the data he wanted, short of making up some false data, I must have suggested the larger damping coefficient, 0.4, and done away with the problematical data, or burnt them in 1978 when we moved. These were the circumstances of obtaining the amplitude characteristic curve for my doctoral dissertation (Figs. 7 and 8). By the summer of 1963, the amplitude characteristic curves for the unsymmetrical periodic solution had already been obtained as well, but since they did not appear in the McGraw-Hill book, the manuscript editing must have been completed by that time. The Figures 7 and 8 were included in reference [5], but the description of chaos does not appear anywhere. Based on this data, Prof. Hayashi wrote his paper on higher-harmonic oscillation, which he reported during the International Congress at Marseilles [6]. I finished my doctorate in the spring of 1964, or more accurately, completed the units requirement, left school, and was hired as a research assistant in the Dept. of Electrical Engineering. During that year, Prof. Hayashi's book, *Nonlinear Oscillations in Physical Systems,* came out [2]. Students are often critical of their teachers. I was thinking "the book may be nothing but a databook of harmonic balance methods." Now the table is turned. As I write this, I chide myself who has not yet published a book.

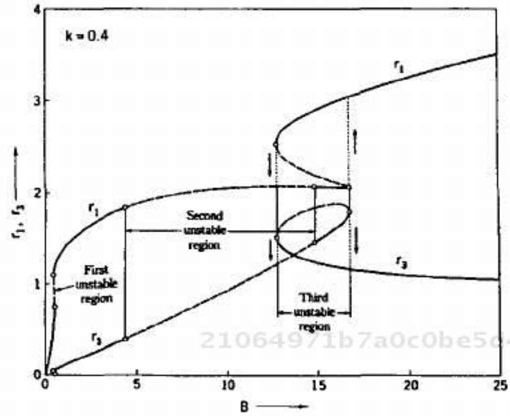

Fig. 7 Amplitude characteristic of the periodic solution

$$v = x_1 \sin t + y_1 \cos t + x_3 \sin 3t + y_3 \cos 3t$$

with $r_1^2 = x_1^2 + y_1^2, r_3^2 = x_3^2 + y_3^2$, of the Eq. (2) for $k = 0.4$.
(Reproduced with the courtesy of Nippon Printing and Publishing Company [6, 17])

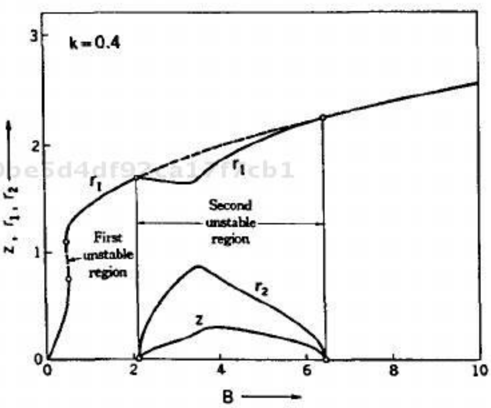

Fig. 8 Amplitude characteristic of the periodic solution

$$v = z + x_1 \sin t + y_1 \cos t + x_2 \sin 2t + y_2 \cos 2t$$

with $r_1^2 = x_1^2 + y_1^2, r_2^2 = x_2^2 + y_2^2$, of the Eq. (2) for $k = 0.4$.
(Reproduced with the courtesy of Nippon Printing and Publishing Company [6, 17])

The Hayashi Laboratory at the Time of the "McGraw-Hill Book"

The above was the condition of the Hayashi Laboratory up until the publication of Prof. Hayashi's "McGraw-Hill Book." The laboratory was overflowing with chaotic data produced by the analog simulations, and yet they were overlooked as a quasi-periodic oscillation or transient state. I would like to touch upon the goings-on in the laboratory around that time. Prof. Shibayama, having finished his dissertation, had stopped coming around. Prof. Nishikawa had left nonlinear oscillations and changed his field to control engineering, a few years earlier. Prof. Abe was focusing his attention mainly on the application of analog techniques to control systems, and was concerned with nonlinear oscillations only indirectly. This made me the senior researcher on nonlinear oscillation in the laboratory.

Even after the publication of his book, Prof. Hayashi used to ask me to do all kinds of work for him. He was especially strict with me, always demanding "Don't give this work to anyone, be sure to do it yourself!" Even if he did not say that, there were several other senior researchers in the laboratory who used most of the students' help, leaving no one to help me.

Let me mention a group that started from then on. "From then on" because it continues even now. It includes Nobuo Sannomiya, three years my junior, and Masami Kuramitsu, four years my junior. Since 1963, we three took turns at holding seminars a couple of hours every week, reading Smirnov's *A Course of Higher Mathematics* (the original was in Russian). Starting in 1963, it took five years to finish studying the Japanese version from Vols. 5 through 12. It was truly helpful. Without the background thus developed by this seminar, I, who had been trained with the engineering school curriculum, would never have been able to understand papers such as Birkhoff's. Prof. Hayashi did not seem to like the idea of our round-robin seminar, and told us often to work on calculations if we had time to read books. But we kept ignoring his admonitions. He also had an extreme suspicion of and dislike for the digital computer KDC-I which finally became available around that time. But I used it to compare results such as the periodic solution of the Duffing equation with the analog data. The KDC-I was a machine built with transistors, and took about 60 seconds to integrate the Duffing equation along the time axis from $t=0$ to 2π, using the Runge-Kutta-Gill method that set the size of integration step at $2\pi/60$. It wouldn't even make a toy today, but at that time I was deeply impressed by the fact that such a calculation—impossible to do by hand—was finally possible.

From the Harmonic Balance Method to the Mapping Method

Dr. Hayashi's "McGraw-Hill Book" was highly regarded, and he was invited to be a guest professor at Columbia University and the Massachusetts Institute of Technology from the fall of 1965 through the summer of 1966. I was his trusted student, and was even given the honor of handling his paychecks during his absence. As he was always present in the laboratory before and after his stay in the United States, I naturally cherished a year's freedom from his watchful eyes. That fall, during Prof. Hayashi's absence, Hiroshi Kawakami from Tokushima (five years my junior) was in the second year of his masters program, studying analog techniques and their applications to control systems. But he came to me for help in studying nonlinear oscillation which he apparently found interesting. It was impossible to change his research project in his second year of graduate school especially in such a small laboratory, but I drew him into the calculation and analog simulation of the amplitude characteristics of nonlinear oscillation with two saturable cores. Soon after, Prof. Hayashi returned. By then Kawakami was already in his doctorate program and his determination to pursue nonlinear oscillation research was already established. He was trying to select a thesis for his research. In our laboratory, as I have already mentioned, mapping or the stroboscopic method had been used for analog computer experiments, and technical terms such as completely stable fixed point were familiar to us. The knowledge came mostly from W. S. Loud's Papers [7]. Prof. Hayashi was friendly with Prof. Loud, regularly corresponding with him. Around that time we got a copy of N. Levinson's paper [8], although I am not sure when and how it fell into our hands. Probably Kawakami, being a diligent student, copied it from somewhere. When I was studying the paper, I came upon the Figs. 2 and 3 on p. 735. The moment I understood the meaning of these figures, I thought "This is it!" (Fig. 9). It solved a long-standing mystery for me. Figure 4 was clearly an error, and the key to the correct answer was in these figures. I thought it through for several days, and figured it out also in numerical terms. This was my first encounter with the concept of heteroclinic point. Of course I did not know the term then. After several days, I was confident. I drew a rough sketch and presented it to Prof. Hayashi (Fig. 10). He did not agree with me immediately, but eventually did, and we decided to investigate all the cases in which subharmonic oscillation of order 1/3 appeared. I recommended the subharmonic oscillation of order 7/3 which I had seen during my earlier analog simulation of higher harmonic oscillation. It used to appear where the external force was large and things got complicated, and I used to feel relieved. We immediately asked Kawakami to draw the invariant curves of the directly unstable fixed point in this case. At the same time we asked Prof. Abe to create an automatic mapping device. Figure 7(b) in the reference [9] shows our results. Figures 11, 12 and 13 were completed right after the abstracts for the 4th Conference on Nonlinear Oscillations held at Prague in 1967 had been mailed, and therefore became its appendix. Since these figures have never been published either in the proceedings of the Conference or in Kawakami's doctorate dissertation, except in the NLP research data [10], I would like to include them here. Please refer to references [10] and [11].

The first results we obtained from Prof. Abe's automatic mapping device are shown in Fig. 6 in reference [12] (reproduced here as Fig. 14), in which the error of Fig. 4 has been corrected. The device itself is summarized in the appendix of reference [13].

In modern terms, the automatic mapping device is a device which uses the analog computer to create Poincaré maps. In other words, the device plots on the recording paper a sampling of the analog signal at the same instant during each cycle of the external force. Although Kawakami and myself helped Prof. Abe build the trial device, it would have been extremely difficult without Prof. Abe's remarkable expertise. Thanks to the device, drawing invariant curves, or outstructures in more modern terms, became much easier. It goes without saying that as a result, the application of discrete dynamical systems theory to nonlinear oscillation was speeded up considerably. Through this experience I learned the importance of examining the original texts as well as making the tools and devices on our own, which are necessary in achieving one's research goals.

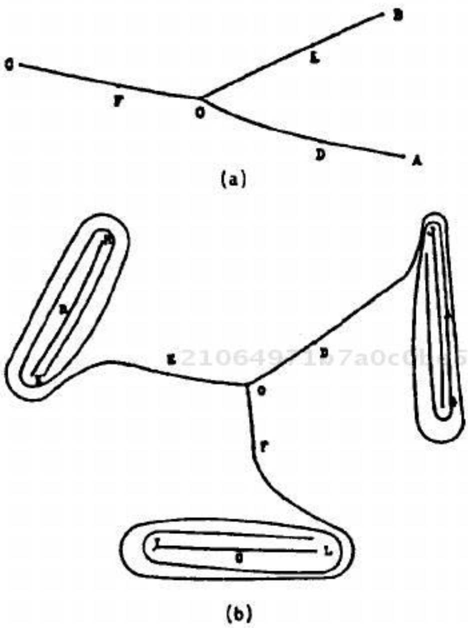

Fig. 9 Examples of maximum finite invariant domains.

(a)
O: completely stable fixed point
A, B, C: completely stable 3-periodic points
D, E, F: directly unstable 3-periodic points

(b)
O, D, E, F: the same as in (a)
A, B, C: inversely unstable 3-periodic points
G, H, I, J, K, L: completely stable 6-periodic points

(Reproduced with the courtesy of Annals of Mathematics [8])

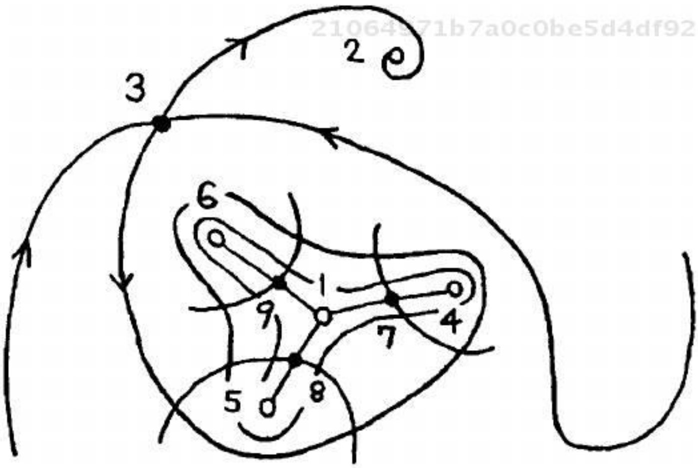

Fig. 10 Corrected schematic diagram of the invariant curves corresponding to Fig. 4.

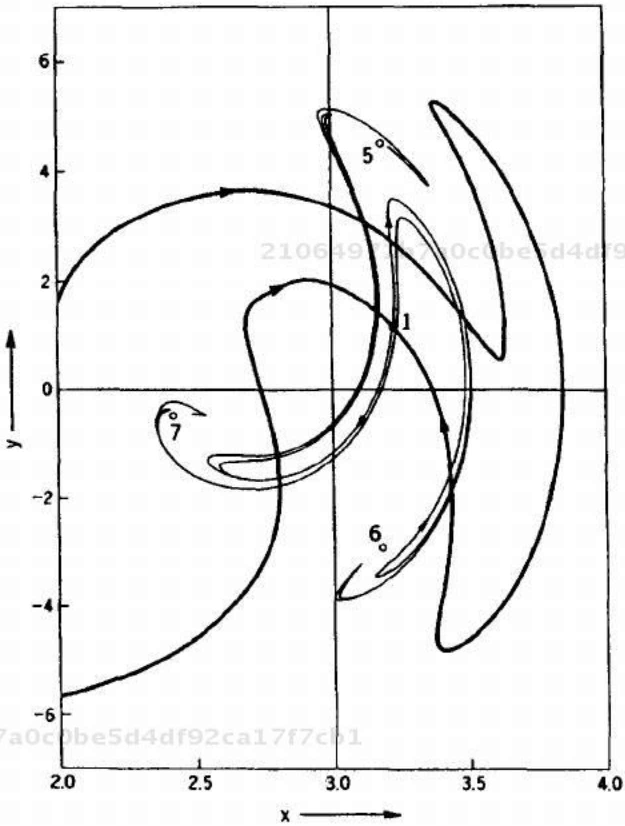

Fig. 11 Fixed points and invariant curves of the mapping T for equation

$$\dot{x} = y, \qquad \dot{y} = -ky - x^3 + B\cos t \tag{7}$$

with $k = 0.2$ and $B = 10.0$.
(Unpublished supplements to Ref. [12])

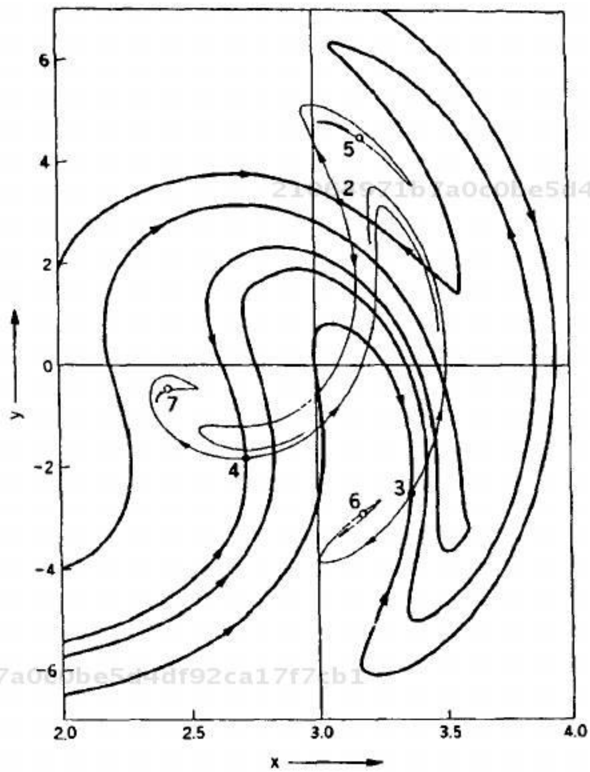

Fig. 12 Fixed points and invariant curves of the mapping T^3 for Eq. (7) with $k = 0.2$ and $B = 10.0$. (Unpublished supplements to Ref. [12])

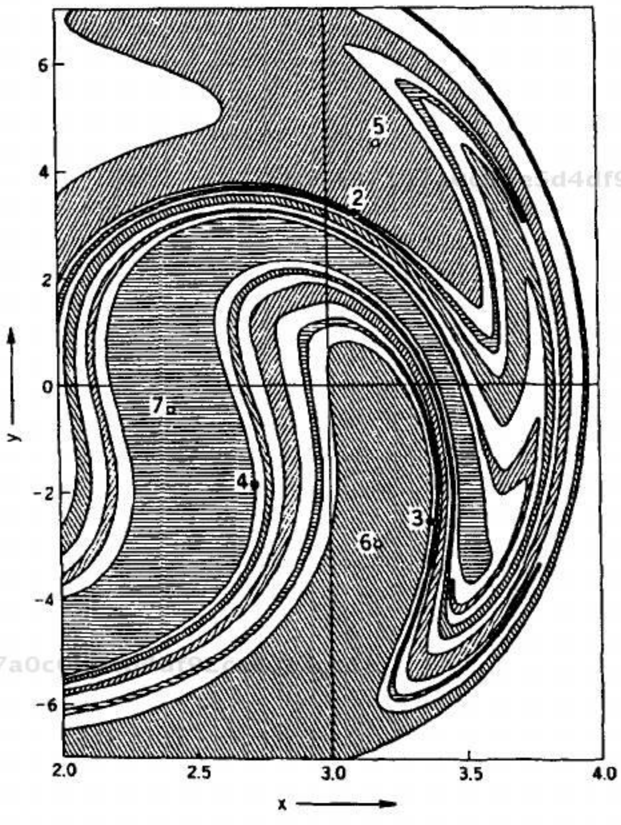

Fig. 13 Domains of attraction of the completly stable fixed points of the mapping T^3 for Eq. (7) with $k = 0.2$, $B = 10.0$. (Unpublished supplements to Ref. [12])

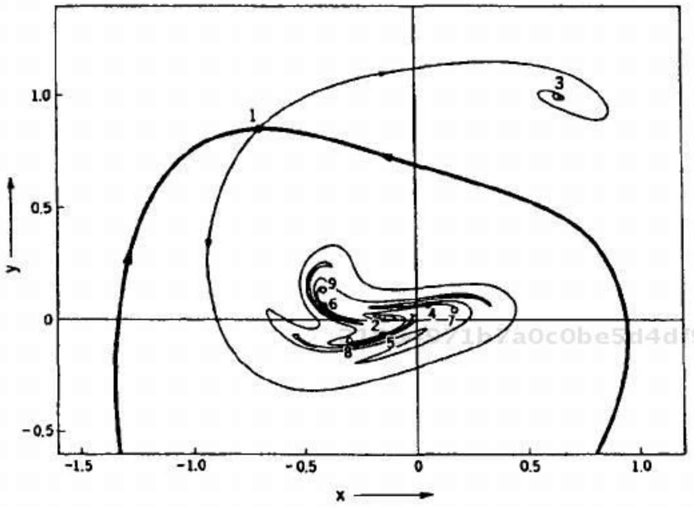

Fig. 14 Computed fixed points and invariant curves corresponding to Figs. 3, 4 and 10. Note: Numbering of the points is different, but the equation and the parameter values are the same. (Reproduced with the courtesy of Nippon Printing and Publishing Company [17])

In the spring of 1966, Norio Akamatsu (seven years my junior) came into the masters program. Another Tokushima native, he chose nonlinear oscillation as the subject of his research from the very start and came directly under my guidance. I asked him to begin with the application of the mapping method to the research in synchronization phenomenon, for which the harmonic balance method had been the standard method in the past. A part of his master's thesis is described in the latter half of reference [9]. These two students from Tokushima were both extremely sharp, and contributed greatly to nonlinear oscillation research. I owe them a great deal. I did not force them to do anything under Prof. Hayashi's authority, but instead built a cooperative atmosphere so as to let their unique abilities grow to the fullest. I pride myself for helping to build the golden era of the Chihiro Hayashi Laboratory during the latter half of the 1960s. In fact, I tried to point out to them the particulars of bibliographic research and interpretations, while gathering data with them, examining their parameter settings, and at the same time acted as their protecting wall, or more accurately, I completed or did over their results which did not meet Prof. Hayashi's approval. Akamatsu was the last graduate student to complete his doctorate program in the Hayashi Laboratory. Kawakami left us in 1970, and Akamatsu left in 1971, leaving Masami Kuramitsu, Kenji Ohshima and myself, who were studying nonlinear oscillations in a postdoctorate capacity. Because of our research themes, we three maintained a parallel relationship. Kuramitsu was working on a nonlinear system with many degrees of freedom, while Ohshima was undertaking experimental research on actual electrical circuits.

The True Value of an Advisor: A Scion of Chaos

I cannot forget to mention my mentor, Dr. Michiyoshi Kuwahara, to whom my work is deeply indebted. Dr. Kuwahara was my senior fellow in the Hayashi Laboratory, and was the very first graduate student who completed his program in our Lab. He understood my position well, and had paid me a visit once a month for the past thirty some years, always ready to give me good advice and suggestions. To this day, I still listen to his wisdom. He does not mince his words—a frankness I always like and admire. Sometimes his opinion struck close to home and was quite painful to my ear. But when I received a lashing from Prof. Hayashi, Dr. Kuwahara used to listen to me patiently and comfort me. I remember those occasions very vividly indeed.

I received much valuable advice from Dr. Kuwahara. Among his innumerable suggestions, I am most thankful for his insistence concerning the art of paper-writing. "Write papers and send them to appropriate academic journals." he advised. "Oral reports (nonlinear research group data, etc.) do not help you. Send them off yourself, so that it will be credited to you by the referees." I did not receive this kind of advice from Prof. Hayashi. Most of the data in the nonlinear research group reports were published by Prof. Hayashi at the international meetings, so I had to be careful not to duplicate his material. Around the time when the university was in turmoil with student protest, I sent a paper of my own for the first time, to the *Journal of the Institute of Electronics and Communication Engineers,* which had referees. The democratic atmosphere that prevailed on campus because of the protest might have prompted me to take this action. The paper passed the review smoothly and was accepted. Figure 6 in the paper [14], demonstrates quasi-periodic oscillation and chaos in the Rayleigh-Duffing mixed type equation (Fig. 15). Though it hadn't taken a clear shape yet, the concept of chaos was already established in my mind by that time.

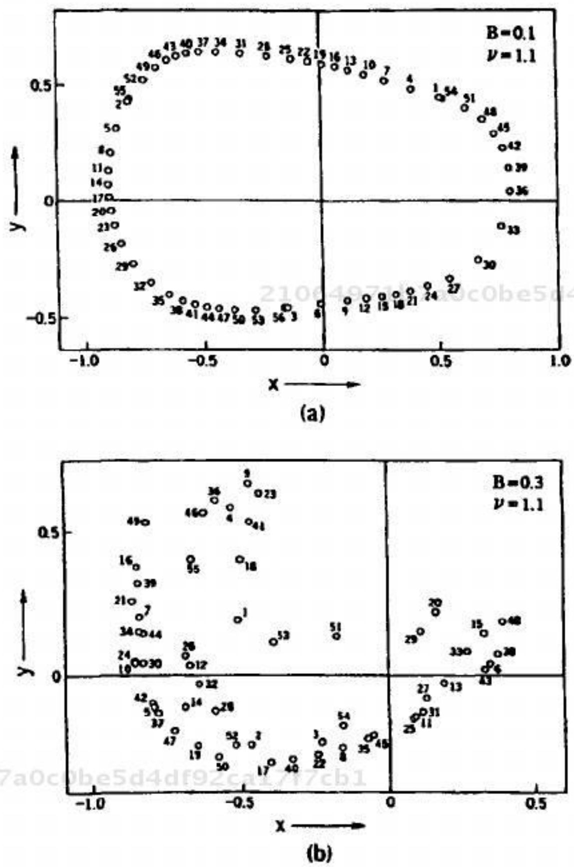

Fig. 15 Point sequences representing beat oscillation for equation

$$\dot{x} = y, \qquad \dot{y} = \mu(1-\gamma y^2)y - x^3 + B\cos\nu t \tag{8}$$

with $\mu = 0.2, \gamma = 4.0$, showing the difference between almost periodic oscillation and chaotic oscillation.
(a) $B = 0.1$, $\nu = 1.1$: invariant closed curve representing almost periodic oscillation.
(b) $B = 0.3$, $\nu = 1.1$: chaotic attractor.
(Reproduced with the courtesy of the Institute of Electronics and Communication Engineers of Japan [14])

This fact can be supported by the following circumstance. A seminar called "Ordinary differential equation and nonlinear dynamics" was held at the Research Institute for Mathematical Sciences of the Kyoto University from the 17th through the 19th of December, 1970, under the leadership of the research director, Prof. Minoru Urabe. At the seminar, I volunteered to make an oral report entitled "A steady solution of the nonlinear ordinary differential equations of the second order." The record of the following comments I made at the end of my research report still remains. ". . . However, according to my observation of the phenomenon with the use of a computer, each of the minimal sets which make up the set of central points are all unstable, and the steady state seems to move randomly around the vicinity of the minimal sets, influenced by small fluctuations in the oscillatory system or external disturbances." [15] These minimal sets are of course the unstable periodic motions in the attractor; the above description led to my proposed name "randomly transitional phenomena." The reason I made the report in Prof. Urabe's seminar was because I hoped for the mathematicians to hear and possibly support my ultimate interpretation of the random oscillations. I was hoping especially because in those days Prof. Hayashi did not welcome my mention of "set of central points," or "minimal set," etc. during our seminars. Everytime my interpretation of the random oscillations was mentioned, he kept pressing me to examine the errors further. Despite my ardent hope, however, my gamble backfired that day. I cannot be sure of the date, since Prof. Urabe's seminar, although small in scale, was held every year. After my report, at any rate, Prof. Urabe admonished me personally. "What you saw was simply the essence of quasi-periodic oscillations," he said. "You are too young to make conceptual observations." The existence of random oscillations (chaos) was so obvious in my mind, that the negative comment did not crush me. Even so, I was deeply disappointed that no one understood it no matter how hard I tried to explain. From then on, I became even more careful in my research efforts.

I think it was 1971 when I sent off the second paper based on my December 1970 oral presentation at the Research Institute for Mathematical Sciences mentioned above. Although I have very little record of it on hand, the Electronics and Communication Society must have some data. The paper was rejected. My only collaborator in random oscillation research, Akamatsu, had gone back to Tokushima, and I was all alone. I spent nearly a year rewriting it and sent it again on Sept. 7, 1972. After going through the review process, it was finally accepted [16]. The summary of the discrete dynamical system theory I included in the appendix was by far the most difficult work. I will never forget how nervous I was, wondering whether or not I had a full and accurate grasp of the concept. The mathematicians who valued rigorous proofs were, in a way, my bane. They can set up any unrealistic assumptions in their heads and live in their world of abstractions, but we are living in the real world. While I wanted them to hear me out a little more sympathetically, I also idolized mathematics. In the appendix, I summarized the essence of the papers by Birkhoff, Nemytskii and Stepanov, etc., but I simply could not understand the transfinite ordinal of the second ordinal class. As I reread this paper [16] now, I am a bit embarrassed by my poor writing. And yet this was where I advocated the existence of chaotically transitional oscillation. I feel that this paper holds true even today, except for a seemingly erroneous description of structural stability, and for the fact that the numerical examples in Figs. 8 and 9 obtained from the analog computer data, turned out to be non-chaotic, according to some later digital simulations which showed periodic oscillations instead. (These periodic oscillations are exceptional, and at typical nearby parameter values, both digital and analog simulations show chaotic oscillations.) Or rather, I should say my qualitative understanding of the steady chaotic phenomenon has not changed or advanced since then. Akamatsu came from Tokushima to write a clean copy of this paper for me. I had intentionally sent off the paper during Prof. Hayashi's absence, so that I had a good excuse for not having it reviewed by him, and it probably lacks certain fine editing because of it. But there was no way I could show the paper to Prof. Hayashi, since I knew he would make drastic changes and cut out what seemed essential to me. I could not compromise, however. It would have been more troublesome to show it to him and then clash with him than not show it at all. I was truly desperate. Even so, I had to consider the protocol of our research, as well as Akamatsu's position since he had not submitted his dissertation yet. Prof. Hayashi's name had to be included as a co-author. Ignoring such protocol may have been a lot simpler, but I could not do that. I have published several papers since then, and I always try to be very careful in selecting data. I would like to continue this practice in the future as well. I also try not to forget that there are junior colleagues who are plagued with insecurity but are clinging to the hope of a better future. It is regrettable that there are some people who are not ashamed to use them to their own advantage. If you have a chance to read Prof. Hayashi's paper on the Duffing equation [17] written around this time, the credibility of my account should be clear to you. In

September of 1972, Prof. Hayashi was attending the Sixth International Conference on Nonlinear Oscillation held at Poznán. It sounds silly now, but I learned the hard way that changing the already established order in this world was truly a difficult task. Prof. Hayashi kindly disregarded this incident, but my paper [16] was not included in his *Selected Papers on Nonlinear Oscillations* published in 1975 [17]. The circumstance was such that I could not personally translate or proofread the English version of the paper [16] published in *Scripta*.

The End of the Chihiro Hayashi Laboratory

Prof. Hayashi retired in the spring of 1975. I was just an Associate Professor at that time and had no idea what had transpired. But the Hayashi Laboratory was dismantled several years later. I have never been clear on the reason for this. We were a group of lost souls without a leader, but I have fond memories of being able to do my work freely for the first time. In addition to the chaos research, I was studying nonlinear systems with time delay with Yoshiaki Inoue. I was also doing calculation of the power spectrum of chaos with the collaboration of the Institute of Plasma Physics, Nagoya University. After that I was invited into the laboratory of Prof. Chikasa Uenosono of Kyoto University where I have been allowed to stay, in the Engineering Dept., to this day. This period was also a chaotic time in my own life. In the spring of 1978, at the time of my move to the Uenosono Laboratory, my paper [18] was published in the *Journal of the Institute of Electrical Engineers*. This was the paper in which the strange attractor (Fig. 16)—the one for which Prof. David Ruelle coined the name "Japanese attractor"—was reported [19, 20]. What was new in the paper was the addition of the power spectrum of random oscillation and related properties to my earlier paper [16], but considering the five years between the two papers, I don't find much progress in it. As I was planning to present this paper to mark the end of my nonlinear oscillation research, I took special care in writing the whole paper. It was around the time that I first heard the term "chaos." But I had never imagined that the enormous amount of the data on the Duffing equation system, etc. which I had accumulated, actually represented chaos itself, and that they would draw such wide attention later on.

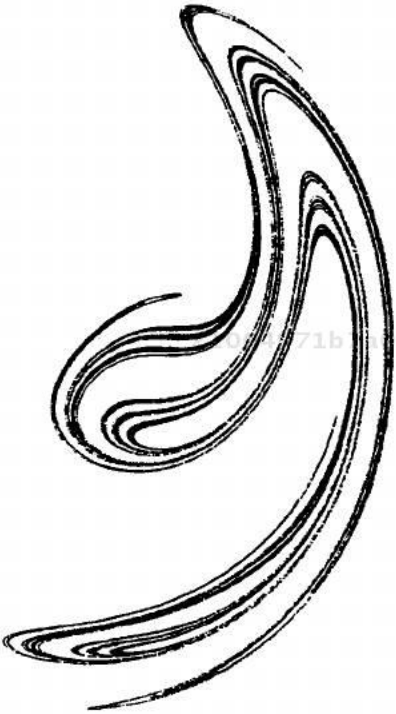

Fig. 16 Japanese attractor.

The Original Data that were Preserved

My original data concerning the Japanese attractor, including the response curves of the shattered egg mentioned earlier, calculated by hand, and the output diagram of the analog computer, are preserved at present in the Brookhaven National Laboratory. In order to explain how they got there, I have to start with my meeting with Dr. Hugh Bruce Stewart.

It was June 1986 when I met Dr. Stewart for the first time. He had come all the way to Henniker, New Hampshire to see me, and attend SIAM's Conference on Qualitative Methods for the Analysis of Nonlinear Dynamics held under the leadership of F. M. A. Salam and M. L. Levi. For that entire week at Henniker, we had long talks. His interest was not limited to technical discussions. He was also interested in the origin of my chaos research. So I began with the shattered egg and reminisced about how it came about. On the last day of our meeting, he handed me a hand-written note which said, "I have gotten approval to acquire and preserve all of your chaos research material. Would you give it in its entirety to the Brookhaven National Laboratory? We will take the full responsibility for preserving it properly." He eventually typed up this letter and sent it to me in Japan. To me, the material had only some sentimental value, but it would have been worthless in Japan. Thinking that I would probably throw it away when leaving the University, I selected some relevant material and sent a boxful to Brookhaven. That gave me a good excuse to put in order my original data that had filled my office all these years. Except for a few things, the whole mess got cleared away, making much needed room in my office.

In April of 1988, Bruce came to Japan. While we were talking, he popped the question of when had I seen the Japanese attractor? I began telling him about the circumstances I had mentioned above, but he did not seem to be convinced. His usual warmth and friendliness gradually turned into a skeptical demeanor. I felt like I was being put on the spot. The day was April 11th, 1988. I fished out some files that I had not sent to BNL, and showed them to him. He studied the rumpled report papers thoroughly and checked my scribbled numbers with his calculator. Finally he seemed to be convinced, and returned to his usual self. I thought this was a good opportunity to clean up some more of the mess, and tossed the files into the wastebasket. But Mr. Bruce said, "Here, give them to me!" I wasn't particularly proud of my rumpled messy files, but agreed. He immediately attached a memo and sent them to Prof. Ralph Abraham at the University of California Santa Cruz. I have seen Bruce many times since then, but haven't seen that stern face ever again. He was probably thinking, "no matter what Yoshi says, I have to examine the actual evidence myself before deciding whether or not to believe it"—this must have been his credo, and I deeply respect that. Many of my results have been cited in his books and papers, but from what I heard, he decided to select them only after checking them carefully until he was absolutely convinced [21]. I believe that every scientist should practice such rigor.

Epilogue

What I have been telling you is nothing but my subjective account. Nevertheless, I intentionally mentioned real names, as I wanted this memoir to be accountable and traceable by the data behind it. Regrettably, Profs. Chihiro Hayashi and Minoru Urabe are no longer with us and won't be able to dispute my account. In order to keep the context intact, however, I could not avoid telling this obviously one-sided story.

As for Prof. Chihiro Hayashi, for sixteen long years, from my graduate student days to the day of his retirement, I received his guidance. He was the last of the true breed of Meiji Era imperial university professors in the Electrical Engineering Dept. of the School of Engineering, Kyoto University. I tried to tone down the description of his personality, but as you can see in this account, he had a very strong personality. He was the emperor of his laboratory, and yet outwardly he was a mild-mannered gentleman. I believed at that time that his was the most feudalistic of any laboratory in the world, and the wall of his authority was impenetrable during his reign, and I still believe it. But because of that, we were not swept away by worldly concerns and could concentrate on our research, being faithful to our own ideas. For this I am truly thankful. It is obvious that my research was not possible without Prof. Hayashi's presence. The rigor, will, and courage needed for one's own research—I trust that these were Prof. Hayashi's legacy. However, I was hounded by his obsession for cleanliness in diagrams.

Later I received guidance from Prof. Chikasa Uenosono. Under his guidance, I learned the rigor and intensity of the world of technology. At the same time I had an opportunity to reaffirm my belief in the importance of experiments as well as accurate perception of phenomena. Prof. Uenosono also taught me the rules and order of the human world and how to handle them. These things have greatly enriched my life. Furthermore, I would not have been sitting in my present position, had it not been for Prof. Uenosono. Needless to say, I am deeply indebted to these two professors. However, the greatest influence came from Prof. Michiyoshi Kuwahara, to whom I am most grateful.

As I have focused this account on my chaos research alone, you may have had the impression that I reached the idea of randomly transitional phenomena directly without going astray. In reality, however, it was a long, meandering and groping process, as you can see if you draw the time line of my career.

What I have been working on during the period I described can be called research in nonlinear oscillation, or from a larger point of view, in basic electrical engineering or applied mathematics. I have a feeling that the people who are involved in these fields are more or less criticized continually by both mathematicians and engineers just the way I have been. The criticisms can be summarized as follows: "Has it been proven?" "Is it useful in some way?" These are reasonable questions indeed, and I have always been at a loss for an answer. I would like to take this opportunity to bring forth a counter-argument. To mathematicians, I would like to ask "How sound is your proposition?" and to engineers I would like to recommend a reading of Bob Johnstone's article entitled "Research and Innovation: No chaos in the classroom" in the *Far Eastern Economic Review* [22]. In the article he quotes Tien-Yien Li's observation (Li of the Li-Yorke's theorem), "If the applicability of chaos becomes apparent, the Japanese will show a fierce interest in it."

Whatever the case may be, I believe it is most important, especially for Japanese researchers, to make an unbiased evaluation of one another's positions based on accurate communication. In this sense, the role of this symposium is truly momentous.

References

[1] C. Hayashi, H. Shibayama and Y. Ueda, Quasi-periodic oscillations in self-oscillatory systems with external force (in Japanese), *IECE Technical Report, Nonlinear Theory* (Dec. 16, 1961).

[2] C. Hayashi, *Nonlinear Oscillations in Physical Systems.* McGraw-Hill, New York, 1964; Reissue, Princeton Univ. Press, Princeton, NJ, 1984.

[3] C. Hayashi and Y. Nishikawa, Initial conditions leading to different types of periodic solutions for Duffing's equation, *Proc. Int. Symp. Nonlinear Oscillations, Kiev, 2* (1963), 377-393.

[4] C. Hayashi, *Forced Oscillations in Nonlinear Systems.* Nippon Printing and Publishing Co., Osaka, Japan (1953).

[5] C. Hayashi, Y. Nishikawa and Y. Ueda, Higher-harmonic oscillations in nonlinear circuits (in Japanese), *IECE Technical Report, Nonlinear Theory* (Nov. 29, 1963).

[6] C. Hayashi, Higher harmonic oscillations in nonlinear forced systems, *Colloq. Int. CNRS, Marseilles, 148* (1965), 267-285.

[7] K.W. Blair and W.S. Loud, Periodic solutions of $x'' + cx' + g(x) = Ef(t)$ under variation of certain parameters, *J. Soc. Ind. Appl. Math. 8,* (1960), 74-101.

[8] N. Levinson, Transformation theory of nonlinear differential equations of the second order, *Ann. Math. 45* (1944), 723-737.
[9] C. Hayashi, Y. Ueda and H. Kawakami, Transformation theory as applied to the solutions of non-linear differential equations of the second order, *Int. J. Non-Linear Mech. 4* (1969), 235-255.
[10] C. Hayashi, Y. Ueda, H. Kawakami and S. Shirai, Analysis of Duffing's equation by using mapping procedure (2) (in Japanese), *IECE Technical Report,* NLP67-13 (Dec. 8, 1967).
[11] T. Endo and T. Saito, Chaos in electrical and electronic circuits and systems, *Trans. IEICE 73-E* (1990), 763-771.
[12] C. Hayashi, Y. Ueda and H. Kawakami, Solution of Duffing's equation using mapping concepts, *Proc. Fourth Int. Conf. on Nonlinear Oscillations, Prague,* (1968), pp. 25-40.
[13] C. Hayashi and Y. Ueda, Behavior of solutions for certain types of nonlinear differential equations of the second order, *Proc. Sixth Int. Conf. on Non-linear Oscillations, Poznán, 14* (1973), 341-351.
[14] C. Hayashi, Y. Ueda, N. Akamatsu and H. Itakura, On the behavior of self-oscillatory systems with external force (in Japanese), *Trans. IECE Japan 53-A,* (1970), 150-158.
[15] C. Hayashi, Y. Ueda and N. Akamatsu, On steady-state solutions of a nonlinear differential equation of the second order (in Japanese), *Research Report RIMS, Kyoto University,* No. 113 (1971), pp. 1-27.
[16] Y. Ueda, N. Akamatsu and C. Hayashi, Computer simulation of nonlinear differential equations and non-periodic oscillations (in Japanese), *Trans. IECE Japan 56-A* (1973), 218-225.
[17] C. Hayashi, *Selected Papers on Nonlinear Oscillations.* Nippon Printing and Publishing Co., Osaka, Japan, 1975.
[18] Y. Ueda, Random phenomena resulting from nonlinearity—in the system described by Duffing's equation (in Japanese), *Trans. IEE Japan 98-A* (1978), 167-173.
[19] D. Ruelle, Les attracteurs étranges, *La Recherche 11,* (1980), 132-144.
[20] D. Ruelle, Strange Attractors, *The Mathematical Intelligencer 2,* (1980), 126-137.
[21] J. M. T. Thompson and H. B. Stewart, *Nonlinear Dynamics and Chaos.* John Wiley and Sons, Chichester, 1986.
[22] B. Johnstone, No chaos in the classroom, *Far Eastern Economic Review* (22 June 1989), 55.
[23] Y. Ueda, Steady motions exhibited by Duffing's equation: A picture book of regular and chaotic motions, *New Approaches to Nonlinear Problems in Dynamics* (edited by P. J. Holmes). SIAM, Philadelphia, 1980, pp. 311-322.
[24] J. Gleick, *CHAOS: Making a New Science.* Viking Penguin Inc., New York, 1987.

Acknowledgments

This article originally written in Japanese was translated by Mrs. Masako Ohnuki. The sequence of the events is as follows. The book *CHAOS: Making a New Science* by James Gleick was published in 1987 by Viking Penguin Inc. [24]. This book was very popular and stayed on the New York Times best-seller list for more than half a year, and was widely admired for its skillful explanations. The present author received a letter from Mr. James Gleick when this book was to be translated into Japanese by his nominated translator, Mrs. Masako Ohnuki. He requested me to supervise her translation. When the author read her translation, he was completely struck with admiration. The translation is accurate and there is no sense of incompatibility peculiar to a translation.

The author lacks the skill to write on delicate matters in English, and he asked her to translate the article, and she kindly agreed. He also requested Dr. Hugh Bruce Stewart to review her translation and to check the contents of this article for historical accuracy. The author would like to express his sincere thanks to Mrs. Masako Ohnuki and Dr. Hugh Bruce Stewart. He also expresses his thanks to the following colleagues:

Chihiro Hayashi	deceased
Minoru Urabe	deceased
Chikasa Uenosono	Professor Emeritus, Kyoto University
Hiroshi Shibayama	Professor Emeritus, Osaka Institute of Technology
Michiyoshi Kuwahara	Professor Emeritus, Kyoto University
Yoshikazu Nishikawa	Kyoto University
Minoru Abe	Kyoto University
Nobuo Sannomiya	Kyoto Institute of Technology
Masami Kuramitsu	Kyoto University
Hiroshi Kawakami	Tokushima University
Norio Akamatsu	Tokushima University

This page is intentionally left blank