Point of Reference for Research

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It is a great honor to be here today on the occasion of this study meeting planned by my kind colleagues to celebrate my 60th birthday. I am truly grateful that so many of you have taken time away from your busy schedules to join me. I would like to particularly thank Dr. Kohda of Kyushu University, Dr. Oishi of Waseda University, Dr. Okamoto of Kochi University, my colleague, Dr. Okumura who proposed this meeting: and Dr. Hikihara of Kansai University who volunteered to act as the secretary.

Giving a talk in the presence of such a distinguished audience is certainly a great honor, but I would like to speak plainly as there is no point for me to be pretentious before all these chaos experts!

The history of my chaos research has already been published in March 1997 (The Impact of Chaos on Science and Society) in the Proceedings of the 1991 International Symposium held under the joint auspices of the United Nations University and Tokyo University, and in a Memoir Column of the May 1994 issue of Denshi Jyoho Tsushin Gakkaishi (Journal of the Institute of Electronics, Information and Communication Engineers). So today I would like to reminisce about things I did not discuss in detail in those publications.

This transparency (Fig.1) is a group photograph of members of Dr. Chihiro Hayashi's seminar, taken around the time of my encounter with chaos some thirty-five years ago. Here are Drs. Hayashi, Shibayama, Kuwahara, Kashimura, Sakawa, Hirai, Mr. Nishikawa, Abe, Tamura; and myself. Drs. Sakawa and Hirai were on the borderline of being called with a friendly suffix of "–san" instead of more formal "–sensei". In those days, research assistants were addressed with the suffix "–san".

Figure 1: Members of Dr. Chihiro Hayashi's seminar.

Front row left to right: Takehara, Hirai, Hashimoto, Tamura, Tsuchida, Murai, Sugata, Enami. Back row left to right: Ueda, Endo, Suzuki, Nishikawa, Murakami, Sakawa, Abe, Prof. Shibayama, Inoue, Prof. Hayashi, Prof. Kashimura, Prof. Kuwahara, Namihira, Kambara, Morino, Kurita, Ohkochi, Hiraoka.

Dr. Shibayama taught me the rudiments of research, while Dr. Kashimura often took me to little bars after work. My time with Drs. Shibayama and Kuwahara has been detailed in the publications I mentioned earlier.

I owe a great deal to Drs. Nishikawa and Abe. Dr. Nishikawa taught me about the linear differential equation with periodic coefficient (Hill's equation) which concerns the stability criterion for periodic solutions of Duffing's equation containing higher harmonic components. He also taught me how to draw Hayashi-style diagrams. Dr. Abe not only taught me the use of analog computers, but also helped me greatly with the work itself. During his retirement lecture given at Denki Sogokan on February 28, 1997, he reminisced about actually building a homemade analog computer. Some of the details were news to me and quite interesting. He named me the heaviest user of that computer–there is no question about it.

As Dr. Hirai came back to Kyoto from Kanazawa University when I was a graduate student, I did not have much opportunity to work with him. I remember having visited his home uninvited however, with Hiroshi Tamura of a class above and a classmate, Inoue, to get a glimpse of his beautiful new wife. Dr. Hirai may look gentle and diminutive, but as you probably know he is a man of the mountain who successfully scaled and left a flag atop Chogolisa. After reading his autobiography Hatsutocho, I have no doubt in my mind that he is actually a mountain climber at heart, and his research was an avocation. I hope he will forgive me for teasing him like this as usual. He often introduced my work in his textbooks and newspaper columns. It was also Dr. Hirai who encouraged me every time we met. "Congratulations, Ueda. Prof. Hayashi and others didn't understand you because...." and so forth.

This photograph (Fig.2) shows an analog computer of the period, in which I observed chaos on that fateful day, Nov. 27, 1961. A drum-type recorder with pencil is seen in front of Prof. Kashimura (far left). We used to have two or three of those recorders, but none are left now since Bruce Stewart took the last remaining one to Brookhaven National Laboratory.

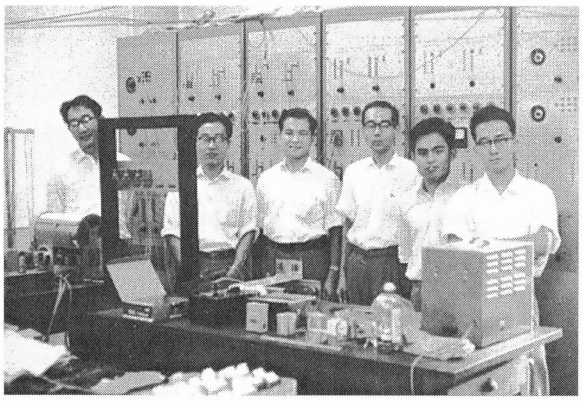

Figure 2: In front of an analog computer of the period. Left to right: Prof. Kashimura, Nishikawa, Abe, Prof. Shibayama, Hashimoto, Ueda.

That was my work environment around the year 1961. It was not until 1969 when I first submitted my paper discussing (or just beginning to discuss) chaos to a scientific journal which actually had reviewers. In other words I spent most of the 1960s in preparation. As I have noted in my published memoirs, I have no idea why I was so determined to pursue chaos, other than to say that I felt something in my bones. But if chaos hadn't become fashionable, I would never have an opportunity like this to talk about my obsession or voice my opinion.

I have often wondered what constituted the reference point for one's research. My feeling is that it differs from individual to individual. From my past experience with many colleagues and students, I have found that many of the most brilliantly written papers often reflected not so much their authors' originality but rather, their references–the papers and books they read or consulted. Here I am not talking about plagiarism, but honest effort to read and digest relevant works. It is only natural since our learning process begins with imitation. But aren't there too many scholars who are uncomfortable if they have no references to fall back on? These people could never have written the clumsy paper I wrote at that time. But to me, what constitutes one's ultimate reference point defines the individual.

Lady Sugi in the NHK TV's historical drama "Mohri Motonari" tells her young step-son that a man's worth is determined by how many people he can deceive. It may be true for politicians, but not for scientists. How not to deceive oneself and others, I believe, is the crux of science. It would be a misfortune for science, if scientists believed otherwise.

If I may speak my mind, the ultimate reference point for one's beliefs and accomplishments ought to be the natural phenomena themselves. Anything less is either a compromise–accepting only the things one can comprehend—or self-deceit–pretending one understands when in truth he does not. To stay away from such traps takes dedication, effort and time.

I would like to tell you of a very bitter experience. Here is a copy of Dr. Birkhoff's paper which I read and reread countless times. There is my comment on the margin dated August 1968. At the time I was struggling with the concept of central motion. Dr. Birkhoff's central motion was the mathematical concept that represented the steady states of the nonlinear system I had been desperately seeking to understand. The minimum unit of the steady state to construct the central motion is the minimal set. In order to arrive at the central motion, Dr. Birkhoff sought the non-wandering set, by removing the wandering points which represent the transient state. Limiting the world within the non-wandering set, he again removed the wandering points from it, and repeated this process. The process may have no end, he wrote. It does not mean the central motion does not exist but, that "It terminates with a definite ordinal of Cantor's second ordinal class." According to the original passage, "Such an aggregate can be at most denumerable, and hence, when arranged as above in a well-ordered sequence, is associated with a definite ordinal r of Cantor's second ordinal class."

I could easily accept that he was writing about the existence of the central motion, but I could not grasp it unless the image formed in my mind. I remember how I struggled in vain for months trying to digest this idea. From morning till night, I kept at it with Japanese set theory textbooks as my guide, but finally gave up. My paper, which was published in 1973 in the Journal of the Institute of Electronics, Information and Communication Engineers, included an appendix written under the pretense that I understood the concept. My heart heavy with guilty conscience and feeling very lonely, I bemoaned the absence of colleagues and helpful mentors with whom I could discuss my problems, the differing viewpoints and research objectives.

There is a sequel to this story. More than a quarter of a century later in 1993, I finally grasped the meaning of the idea I had abandoned decades ago. It was right there in a textbook my students were using for Prof. Koji Shiga's seminar. What I could not understand twenty-five years ago was essentially an axiom, not a theorem! A few years ago, I mentioned this belated discovery to my colleague, Hiroyuki Nakajima, and blamed the quality of Japanese mathematics textbooks. He agreed but then said "you probably didn't understand because you didn't read it carefully." As it may have been the case, I'd better refrain from attacking textbooks, but even so, had my textbook been clearer in defining where the statement of an axiom ends and where the proof of a theorem begins, as Dr. Shiga's book has done, I wouldn't have wasted my precious time. This episode illustrates how important a textbook can be. I

see too many textbooks these days that are carelessly thrown together.

The other day Nakajima and I were discussing this very subject. We agreed that a scholar must clearly comprehend what constitutes the foundation of his research even if it appears to be unrelated to his current subject. I fear today's students are getting too dependent on being spoon-fed with easy instructions.

Now let me go back to the non-wandering set. In 1990 I was asked to write an article for a new journal, "Chaos, Solitons and Fractals" which began back in 1991. Even to a slow writer like myself, the lure of being nominated as one of its honorary editors and seeing a picture of my attractor gracing its cover, was too much to resist. I decided to write a survey paper about the phenomena represented by the Duffing equation. In other words, I chose an easy way out by editing and summarizing several of my old papers. When the manuscript was ready, I asked Bruce at the Brookhaven National Laboratory to check the contents and my English. After careful examination, Bruce commented that "the steady states in a physical system are represented by the first non-wandering set obtained after removing wandering points. There's no need to pursue it any further. This was established in the 1960's Pugh's closing lemma." Although he was quite sure this was correct, he wanted to verify it. "Ralph knows much more about this. Let's go see him," was his suggestion. This was in the fall of 1990, shortly before the EPRI Conference in San Francisco. Bruce set up a retreat at Capitola near Santa Cruz, following EPRI's International Workshop on Application of Chaos. There, Bruce, Michael, Allan and I got together, and after personally checking with Ralph, I completed and published the paper in Vol. 1, no. 3 of the journal, in spite of my fuzzy understanding of this particular point.

Since Bruce and Ralph were mentioned, I would like to digress and talk a little about my monograph "The Road to Chaos" which came into existence solely on account of Bruce and Ralph's generous assistance. Many others helped of course, but except for a part in Chapter 1 and the part mentioned above in Chapter 6, I wrote every single word from scratch. Many students collected data for me, but I personally obtained what I considered the key data. Looking back on my student days, I feel sorry for today's young researchers. They are burdened by constant demand for accomplishments. I am afraid this trend may inflate the number rather than the quality of their papers, as authors hope mainly to gain the reviewer's acceptance. It could encourage a hasty completion of a paper when more clarification is needed. Once it is written most people will forget the uncertainties and move on.

As I have written in my United Nations article, I changed my field in 1978 when Prof. Uenosono took me under his wings. In fact I was convinced at the time that as long as I held onto a field such as nonlinear systems, neither mathematicians nor electrical engineers would take me seriously, and that there would be no opportunity for me to publish a paper. Then a full professorship would be an impossible dream.

Before studying under Prof. Uenosono, I didn't think much of working on synchronous machines, which had already been in use for a long time. Yet as I began working on it, I could vaguely see why Prof. Uenosono himself had built an experimental machine with embedded measuring coils. At the same time, I came to realize the complexity of the problem. Before joining the Uenosono Laboratory, I decided to enforce a principle on myself, that I would draw a line between my work and that of the members of the Uenosono and Okada Laboratories. We were all working on the same machine, but my approach should be my own. I would not meddle with or join others' research projects. I would be completely independent. This was my principle. Unfortunately many scientists seem to overlook this fact, that even if the object of research is identical, the focus and the direction of research are up to the individuals.

I was able to stick to my principle thanks to my good fortune of having understanding colleagues and a very good student. The student, Toshiya Nanahara, is now working at the

Central Research Institute of Electric Power Industry. The first job we tackled together was to change into our work clothes, crawl into the back of the switchboard in the laboratory, and trace the contact wires one by one up to the ceiling; to examine everything, from meters to terminals. After three days of meticulous examination, we finally turned on the synchronous machine according to the student experiment handbook. It feels as if it was only a few years ago, but actually it has been more than twenty years. Prof. Uenosono told us to ignore electric machinery textbooks and papers, and dive right into hands-on experiments. I think this was sound advice, because if one has some hands-on experience with the machinery, he or she can have deeper understanding of what is written in the textbooks and papers about it. One can read between the lines based on his own experience. Of course, this way makes us spot lots of faults in textbooks. Rewriting them to improve on contents would be a daunting task indeed.

My understanding of the problem Prof. Uenosono gave me was to grasp and formulate the essence of the inner magnetic flux of rotating electric machines. At the time I was beginning to get the feel of the behavior of magnetic flux, after repeating my experiments hundreds of times with a synchronous machine. I had a feeling I might get an interesting results if I could analyze my data from a mathematical viewpoint, when Hikihara and Ohta joined our Lab. They wanted to work on nonlinear systems, of course, but we enticed them into our synchronous machine research group. And as a sort of consolation prize, I taught them quite effectively everything we had done on nonlinear systems so far, the operation of the computer, diagram drawing–the works. When they became the second graduates of our master's program, Nakajima, who eventually became our fourth graduate, joined us. He, too, ended up writing his thesis on synchronous machines. Since many of you know Hikihara, Ohta and Nakajima, I would rather let them talk about our Laboratory from their perspective, and move on to tell you a little about Sumitani and Prof. Ogura.

During the Chaos boom of the 1980s I was still running the synchronous machine. Prof. Ogura who was three years my senior and teaching at Kyoto Institute of Technology at the time, used to urge me to stick to the chaos research. He probably suspected that I wasn't following his advice, because he later sent Mikio Sumitani over to our Laboratory to work specifically on chaos for his graduate degree. Sumitani was an excellent student and made great accomplishments with the support of Prof. Akamatsu of Tokushima University and Prof. Yoshizawa of Tokyo University. His work, however, had never been submitted to academic journals, due entirely to my negligence. I owe Prof. Ogura a deep apology. True, I did eventually write and publish my own results which had been accumulating without a place of publication since 1978, but I had not worked actively on chaos for several years when Sumitani arrived. Furthermore I knew I could never catch up with the overwhelming number of chaos papers that were streaming out at the time. Frankly, I did not have the capacity to help Sumitani put together his data into a good research paper. As for myself, the data I had kept unpublished would never have seen the light of day had it not been for invitations from overseas.

Since there is not much time left, I would like to touch on the present. As a principle I try to treat everyone in the Laboratory who has advanced beyond our PhD program, as an independent researcher. I try to leave the researchers alone so that they can do their own thing. Unless solicited, I do my best not to force my opinion or give directions. I tell them to be independent and be the principal authors of their papers. Of course, these are my intentions, but my students may have different opinions.

Actually I have never given up hope to write research papers as a principal author. That is precisely why I do not want to get involved too deeply in my students' research. Being still wishful, I am afraid of being unable to hold back my desire to give directions and eventually

lead them to where I want them to go once I get involved. I have been very careful in keeping my work and that of the others separate. However, I am belatedly finding out to my chagrin that this policy should have been a matter of degree.

This may sound contradictory, but I do tell young people to write a lot of papers. I also advise them not to take all the credit but always give credit to others who contributed. After all a general by himself cannot win the battle. Even so, one should never spoon-feed young people. I think it is a questionable practice to let a student be the principal author, when in fact he has not struggled through the arduous process of digging up the topic of his paper, completing research, summarizing the results, submitting the paper, and responding to sometimes hostile reviewers. To complete a paper is by no means an easy task. It is an all-out effort researchers should cherish with pride. Creativity will spring from nurturing one's individuality (and sometimes even prejudice). At the same time we should be reminded of the fact that not all leaders are necessarily creative.

Although I urge people to write papers, I must confess that I am no writer myself. When I was preparing my chaos papers, I carried my notes with me everywhere, writing memos, reading and editing constantly. I confirmed and reconfirmed my data, read relevant papers to create images in my mind, and examined the accuracy of my concepts. Frankly, I do not have that time now. So rather than writing half-hearted papers, I assume a defiant attitude and simply do not write!

When I was young, I felt that all the older professors repeated themselves without making progress. Now that I have reached their age, I realize that I cannot help doing the same.

And yet a man needs to advance with time. In the past I have been such a stubborn man, but from now on, I would like to heed other's advice. I have recently come to realize that it is so much easier and more to my advantage. Jokes aside, I ask your indulgence and constant guidance in the years to come.

Today has been a truly memorable day for me. I thank you for your kind attention.

(Translated by Mrs. Masako Ohnuki and Dr. H. Bruce Stewart.)