Chapter 2
Entering Theoretical Biology

You are now 26 years old. Our last point was that you lost your position at Max Planck because your stipend had finished. Now, please, continue: What happened after that?

I turned 27 that year. It was the year I would get married. I worked at the University of Marburg for a while to finish my medical training as a "Medizinalassistent." Before getting the approbation (license to work as a doctor) one had to work for 2 years as a supervised medical intern in different departments. In my spare time I continued with my scientific notes.

Is there anything important about getting married? From the Copenhagen viewpoint it is a spike in your psi-function.

I agree—many things happen when you make such decisions in your life, it is not the responsibility of one's own self alone, so the situation is very complex. You are at the mercy of another person in a symmetric, compounded way. It is a miracle. It is much easier to continue on science. After leaving Seewiesen I continued scribbling down notes on my thoughts on the origin of life. A short unpublished paper emerged. I also was allowed to give a talk on my theoretical approach to evolution in the medical clinic in Marburg in the lecture hall. It was received with interest. It was my first public talk after the two seminar talks held at Seewiesen. I learned to talk slowly. The auditorium felt that I seemed to belong to the 19th century—an opinion my wife heard. The origin of life remained my topic while working in medicine and I had also another meeting with Carl-Friedrich von Weizsäcker in Marburg. His encouragement continued.

What exactly was the project you were thinking about?

It had to do with Teilhard de Chardin. I realized that he had tried to build a consistent theory of evolution, based on a pervasive drive in nature seen by him that propels everything towards becoming more and more highly organized. He coined the notion of the "Point Omega"—the first attractor in history—which attracts everything in the cosmos (first spotted by him in 1915, when he was a chaplain at the front, under the name "Point Theta" at first—a bull's eye).

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Everything in the universe is propelled towards an asymptotic state of a maximally highly organized complexity. I could support this idea of Teilhard's eventually with my chemical-kinetic equations.

Then I was allowed to visit the institute of a famous man, a freshly decorated Nobel Prize winner Manfred Eigen, who kindly invited me to give a talk—an event which was consummated before his whole institute on a skiing vacation in Hoch-Sölden in Austria. It was an important experience because I completely flunked the test. I gave a talk about my ideas on how chemical substances influence each other so that eventually you get autocatalysis in a network that is branching out. In the discussion I was asked whether I could define mathematically what a differential equation is. I answered that I had just presented many examples of such equations, what was it that was meant by defining it exactly? I was told that since I would not answer formally, it was no use talking to me any longer. Such an experience can make you stronger.

Who told you so?

The highly decorated scientist thereby effectively forced me to be more formal than I had been. Much later both he and I would independently write a paper on mad-cow disease. So our thoughts apparently went along parallel lines over decades.

And then you modeled your idea as an equation?

Yes, because a good fate intervened. I was using libraries very often and wrote a big paper on chemical evolution that by happenstance never appeared. I submitted it to the Journal of Theoretical Biology in Buffalo, N.Y. and got a favorable review but was told I should translate it from German into English even though the journal in principle accepted German manuscripts. My English was not good enough, I felt, a drawback I could not overcome at that time. Strangely, the same institution in Buffalo had posted an announcement for a stipend in the Journal of Theoretical Biology. Since I knew that my wife was interested in going to the U.S., I applied for this stipend. Later I learned that I was the only one out of about 170 applicants who got it. So by this stroke from heaven I suddenly found myself working at the Center for Theoretical Biology. It may have had to do with my paper submitted to the same institution. It gave me this opportunity to meet a whole new continent; America.

You were 27 years old? And how long did you stay at the institute in Buffalo?

It was only a bit more than half a year. It was in 1969, so I was 29 years old.

Did you do any special work during the 6 months there?

Actually no. My wife had a position at the famous Immunological Institute at the same university, with Professor Ernest Witebsky. I was just sitting in my room in the institute at Ridge Lea Road, writing notes and talking with people at the institute, mostly with Bob Rosen who had just finished the manuscript for his book

"Dynamical System Theory in Biology." I was reading these notes and we talked, and I was attending talks of visitors, including Ludwig von Bertalanffy who was there, too—to whom I had written my first scientific letter 5 years before which, as you may remember, he apparently never got. It was very nice to talk with him about evolution, the origin of life, theoretical biology and the way he had invented it as a science. I also attended a talk by Stu Kauffman at the Buffalo Institute. Being a medical doctor, too, he had independently invented almost the same theory of chemical evolution as I had. A lifelong friendship with Bob Rosen followed, and with Stu.

And then you came back from the United States to Germany?

Yes, after having been given a "semi-permanent position" in Buffalo which was a very nice thing to hear. I then got a stipend from the German DFG research organization. I chose to work in Tübingen with it. By happenstance, a scientist whom I had met in Marburg, chemist Friedrich Franz Seelig, had just received a call from Tübingen and was willing to accept me as a stipend holder to work with him at the University of Tübingen. So I found myself working at the Chemistry department there.

As a postdoc, what did you do?

The new chair was busy buying an analog computer at the time. I was sent to a course to professionally use such a machine and was then the one who worked the most with it. It was quite nice to be able to put my mind into these mutually influencing electrical variables (voltages) that one could watch performing on the screen of the oscilloscope. I liked very much this way of understanding dynamics in the same pictorial way in which also spatial intuition works. There was a kind of synchrony between this machine and my mind.

At that time, was there only one such computer?

There was another analog computer from a different firm in the department of biology, but I never saw it. These machines were the state of the art at the time.

You had this computer at the chemistry department?

Yes, and I was virtually free to use it all the time.

So you started simulating dynamical systems on an analog computer?

Yes, and instead of working with the almost infinite number of variables that one has to deal with in the origin-of-life theory, I kind of reduced the number of variables more and more, and soon discovered a 3-variable "chemical multivibrator." I constructed analogies between electronics on the one hand and chemical reactions on the other. "Liquid automata" was a name I coined and proved to be correct. It is quite counterintuitive that you can have an automaton, a discrete-state machine, which is a well stirred fluid. It indeed is a simple digital computer

because the differential equations underlying both the soup and a chip are the same. The computers we are using are, in reality continuous-time differentiable dynamical systems. Automata theory is but a simplified description of these more complicated continuous systems. It was nice to get persuaded by a machine, to see the interaction of smooth dynamical variables everywhere, in chemistry and evolution, and also in electronics. I even already saw some chaotic oscillations on the oscilloscope screen in 1970, but I misinterpreted them as being noise-induced.

You were 30 years old at that time. Did you know about chaotic systems by then?

Not at all. It was only a few years later, during this on-going work with electronic systems and their analogs in chemistry, in continuation of my previous evolutionary networks, that I started to put my mind into the mutual interplay of four, and then three, variables.

When you were working on the analog computer, did you know Edward Lorenz? When you encountered the concept of chaos, did you meet him personally or did you only read his paper?

The paper was a present given to me by my friend Art Winfree in 1975. Years before, my friend Wolfgang Engelmann had introduced me to Art. In 1972, Art invited me after my first paper on the parallelism between electronics and liquid chemical systems to give a talk at Purdue University in his department. Then I saw him again 3 years later in Vienna at a conference on biochronological systems. He had attended my talk and told me afterwards that this was a little bit boring, my deductive theory of chronobiological systems, did I not do something more interesting in the wake of my liquid automata? I told him that I was thinking about a three-variable limit cycle that looks like a knot and cannot be flattened into a more or less circle-like thing but is irreducibly more complicated because it is genuinely three-dimensional. He replied that this sounded to him like chaos. I asked: What do you mean by chaos, I know the term only from traffic jams. He told me there had been a conference in Aspen, Colorado, as usual without proceedings, which he had attended, "on chaos" and that he had collected all the papers written on the subject up to that time and that he would send me a folder with them all. I thought he is a very good friend but people in America are always very positive and kind but possibly nothing will come out of this. Lo and behold, 6 weeks later I received a big folder with all the papers that had been written on chaos up to the time, most still in preprint form.

In that folder, was there a paper that stimulated you especially?

Yes, Ed Lorenz's paper of 1963, whom I would meet a year later, was there and more recent ones like Jim Yorke's and Bob May's and George Oster's, which I remember. Art wrote me explicitly that I should do him the favor of finding a chemical version of the Lorenz attractor.

What do you mean?

Art Winfree thereby made me his pupil by decree if you so wish—by forcing me to work in this area because he said he had no time to do it himself, so I should please do it in his place. Hence I felt I had the duty to try and find a method to implement the Lorenz equation in chemical rate equations, which are unlike Ed Lorenz's non-negative by definition.

So you met the Lorenz equation in these proceedings actually?

As a reprint in the folder Art had sent me.

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