Although as far as I know this topic has not yet been described by other scientists, I unfortunately never published this paper. When I had completed it, I came back with the family from Mexico to Champaign; and immediately after our arrival, I founded the Biological Computer Lab at the University of Illinois. That took so much energy, that I no longer had any time to concern myself with this muscle paper. And thus the muscle paper, to this day, exists only as a manuscript. It's all still handwritten.

We had a visa for exactly one hundred and eighty days. On the one hundred and eightieth day we again crossed the border to America and came back to Illinois. I then told my superiors at the university: "I would like to found a new laboratory."

THE *BIOLOGICAL COMPUTER LAB*

Laszlo Goldstein and others take over the *Electron Tube Research Lab*

I had set up the Electron Tube Research Lab in such a way that I could easily distance myself from it, without it falling apart. I invited four excellent people to collaborate. They were hired by the University of Illinois. One of them was Laszlo Goldstein, a Hungarian physicist. He was a very famous soccer player, a professional soccer player. He had earned the money for his studies as a soccer player on the French team.

He wrote his doctoral dissertation under Marie Curie—an incredible dissertation, two hundred pages. He gave it to her, and she took it home. After a month she returned the dissertation and said, "Not good!" — "Well, what can I do?" — "Not good!" So he went home and read the dissertation. He didn't have a clue as to what was not good. And so he writes another dissertation of two hundred pages and hands it to Madame Curie. She keeps it for a month, and then says, "Not good! Not good!" Well, he didn't know what to do with that, goes back home, writes a third dissertation and gives it to Madame Curie. She takes it; two months. "Good!"

Accepts it. Those are the pedagogical devices of the French scientists.

Laszlo Goldstein took over plasma oscillation. That was my field, plasma physics, and Goldstein was very famous for his plasma research. The University gave him a professorship.

Another was Paul Coleman, a high-tension physicist. He took over the entire high-tension department. Then there were two more people, extraordinary physicists, wonderful people. I could see, the laboratory was in good hands. The students who had worked with me had all graduated. So I could leave the lab, without causing any damage.

The financial basis

I went back to Washington, to the people who had financed the Electron Tube Laboratory, the people of the Office of Naval Research. In the meantime McCulloch, who had known that I wanted to found a new lab, told people high up in the Office of Naval Research: "A young man by the name of Heinz von Foerster, who wants to found a new laboratory, will come to see you. Do help him as best you can!" Well, I got to my sponsors and said, "I would like to found a new laboratory." They said, "Yes, Heinz, that is a good idea. We will give you a small advance, so that you can found your new lab. How do you want to call it?" I said, "I would like to call it *Biological Computer Lab*, BCL."

The original idea: Parallel computing

The original idea, which I had developed in the course of the months that I worked with McCulloch and Rosenblueth, was the idea of parallel computing.

Through the study of neural networks I learned that in the case of living beings, all the operations run in parallel. What surprised me early on was that the individual nerves are very slow and yet are capable of executing incredible operations that a very fast computer is not capable of.

John von Neumann invented this ingenious machine, where a central processor does all these great tricks with data that it is fed with.

The von Neumann computer idea was that when processes are carried out, the result is forwarded to a new station, where new processes are carried out. And the program that you input into the machine says exactly what is to be done at each individual step. One calls that "sequential," that is, the operations are carried out in a sequence, until the end result is spit out.

Computing with a central processor naturally goes incredibly fast, so many operations can be executed.

The eye, for example, is a parallel computer. You have ten million cones and rods on your retina that are all illuminated simultaneously and then send their signals on to where everything is computed in parallel and runs to the brain in parallel. The brain hears, sees, tastes, feels everything simultaneously and out of this simultaneity produces certain results that then turn into behavior.

A visit to a professor in Boston, who devoted his time to the study of praying mantises—they are insects—was particularly instructive for me. He glued the praying mantis onto a little stick, so that it couldn't fly away. Then he had a fly pass by. The praying mantis sits there and with its forefeet snaps the fly out of the air, as it flies by.

He said, "It was very tiresome to always have to catch the flies. We started to breed flies. And so I went to the Art Department and asked: 'Couldn't your students make artificial flies for us?' Whereupon the students produced artificial flies." And there he showed me a cardboard box with maybe thirty flies: Five rows and six rows down. Then he asked me: "Which one is an artificial fly?" So I look at it and say, "There is no artificial fly among them." He says, "They are all artificial." They were done so well that you were absolutely unable to detect that they were artificial flies.

What was really nice was: When they passed by an artificial fly, the praying mantis leaned back and continued reading its newspaper. It was bored to tears, when an artificial fly went by. When a real fly came along, it made *snap* and caught it.

A praying mantis has big, facetted eyes. There are perhaps ten thousand eyes on each ball, and the brain, the computer, is one millimeter to the left and one millimeter to the right; a small cube of one cubic millimeter. This cubic millimeter computes: "Is that a fly or a joke? Where is it flying? How fast is it flying? In what direction is it flying?" For me that was the example for how one should actually build computers. And it is, of course, a parallel operation: The ten thousand *ommatidia* of the eye of the praying mantis send their signals to this computer, one cubic millimeter big, which then computes from this data, where the fly is flying to, if it is a fly at all, and how fast it is flying, so that it can direct the muscles in such a way that the praying mantis can—*snap!*—catch this fly out of the air. So, when you see these magical operations in the biological domain, you say that sequential computing is not a good solution.

When you compute in parallel, the individual computer can be very slow, as for example a neuron. A neuron is not nearly as fast as a microchip in a computer. A microchip can compute in a millionth of a second; a neuron needs about a tenth of a second before it can say "Mu." But if you can compute with ten million neurons simultaneously, you are a hundred times faster than a microchip.

So when you compute in parallel, you can compute a lot more in less time. The method of computing is totally different. Well, that was the basic idea with which I started the Biological Computer Lab in the beginning of the year 1958.

Namur

Gordon Pask

The people in the Navy, who had immediately supported me, said, "We just heard of a very interesting conference that will take place in Namur. That is the second international congress of the International Association for Cybernetics. We are sending you to Namur; you should participate there." And so I flew to Namur and gave a lecture. It was called "Some Aspects in the Design of

Biological Computers.” There I presented my first ideas. That was in September of 1958.

Fortunately I knew that with the French, the chairman of a conference takes the opportunity to finally speak without anyone being able to interrupt him. So at a lecture series of three or four speakers, who have to end the session in the course of one and a half hours, the French chairman usually tends to speak for an hour, and to leave five or ten minutes for each of the three or four speakers. I knew: “I’ve got to tie up my lecture into ten minutes,” although in the program it was scheduled for half an hour. So the conference begins. The chairman speaks for an hour. The first speaker gets up. He says, “Ladies and Gentlemen, I would like to present my ideas to you.” The chairman knocks on the table: “Lecture finished. Thank you very much. The next speaker, please!” — “Ladies and Gentlemen, I would like to tell you about this and that.” — “Thank you very much, the next speaker, please.” The next and last speaker was I. But I was prepared for that and was able to rattle down my story in three or four minutes. So one could understand what I was saying.

After my presentation a few young people approached me and said, “We heard another speaker here. He talked in a very interesting way, about topics very similar to yours. You’ve got to meet him!” — “Well, I’ll be delighted to meet him. Who is it?” — “His name is Gordon Pask and he is probably sitting in some coffee house in Namur, with a whole swarm of people around him, continually wanting to ask him questions.” I say, “Let’s go! Let’s search the coffee houses of Namur!” And so we walk through the streets, coffee house after coffee house. Then in one coffee house, we really see a swarm of people around a table outside. They say, “There he is! There he is!” So we go there.

A little man, indeed, sits there, looking like a leprechaun or a garden dwarf: Small, with black hair that stands up in all directions; with a white shirt, a black suit and a black tie; a long cigarette holder, at its end a smoking cigarette, being held in his hand. In a beautiful British accent he elegantly answers the barrage of questions that rush in on him. Well, I fought my way through these people and pose some questions of my own to this funny fellow. He answers with great enthusiasm. I answer with great enthusiasm. We start a fun conversation. I sit down at the table and order a cup of coffee. Within an hour we were the best of friends. At the end of this conversation I thought to myself: "I would like to invite this man to the BCL." So Gordon Pask was my first guest at the BCL. Directly after the conference, he came to America.

Stafford Beer and management

There were two other people whom I also met then in Namur. One of them was Stafford Beer. Stafford Beer was the so-called "King of Management," who had incredible ideas about management. He wrote the book *Cybernetics and Management*. Stafford Beer had an ingenious management technique, with which he saved the English steel industry. England had not been as bombed and destroyed by the war. The English therefore still had those very old machines. The German factories had all been bombed out and, through the Marshall Plan, were able to buy the newest machines. So the English were totally behind in steel manufacture, in the manufacture of sheet metal, and could not accomplish anything for the automotive industry. Their problem was that their rolling mills still worked so imprecisely, that when a car manufacturer ordered sheet metal that was supposed to be exactly 1.1 millimeters thick, they could not carry out the order. The sheet metal would turn out to be either 1.2 or 1 millimeter thick. Stafford Beer, the manager, said, "The fact that we cannot roll it with precision is not a disadvantage, for we can measure the result precisely. When a piece of sheet metal comes out, we measure it to a one hundredth of a millimeter. It may measure 1.111 millimeters; we put it on the 1.111-millimeter pile, on which we only have sheet metal measuring that. Then we have another pile with sheet metal that is 1.112 millimeters thick. So we can produce the greatest exactitude by sorting rather than by buying new machines." That is how he revitalized the English steel industry. He thought cybernetically.

Another example: Lion's is a sort of McDonald's in England. You come in, help yourself to fork and knife, get yourself a plate, get your salad, or whatever you want to buy, go to the exit and pay; exactly as in a cafeteria. Those were, I believe, the first cafeterias in the world.

Beer told them: “Just look! People come in, don’t know yet what they are eating, help themselves to everything: A fork, two plates, two spoons, two knives, et cetera, et cetera. That’s crazy! Don’t put the tableware out until people know what they are eating. Let them pick the food first, and then let them help themselves to the tableware. You’ll save yourself the trouble of washing many, many forks and knives that haven’t even been used.” So that is yet another example of how Stafford Beer applied cybernetic insights to management.

Ross Ashby and the mechanical chess player

Then there was Ross Ashby, whom I already knew from the Macy Conferences. He was also in Namur. And Stafford Beer, who was very rich then, invited all of us to the castle, to a very expensive restaurant, high up on the mountain in Namur. Pask, Ashby, Heinz, and another two or three people dined there as guests of Stafford Beer.

Ashby had been invited by McCulloch to give a lecture at a Macy conference, entitled: “Can a mechanical chess player outplay its designer?” He concluded: “Yes.” But he couldn’t hold the lecture, for as soon as he began to speak: “A mechanical chess player …” Julian Bigelow says, “What do you mean by ‘mechanical chess player’?” Whereupon Ross says, “A system that is built for a person, that knows the rules of chess, has strategies for responding to specific moves.” — “What do you mean by response?” Ross says, “Not a response, but a countermove.” — “Oh, a countermove. Well, what else is this ‘mechanical chess player’ supposed to know how to do?” — “He doesn’t have to know anything else. He only needs to be fed the moves of his opponent, and then he can make a countermove.” So Julian Bigelow, actually a person who had been part of the cybernetics group from very early on, never let Ashby get a word in, and always asked these terrible questions: “What do you mean by ….?”

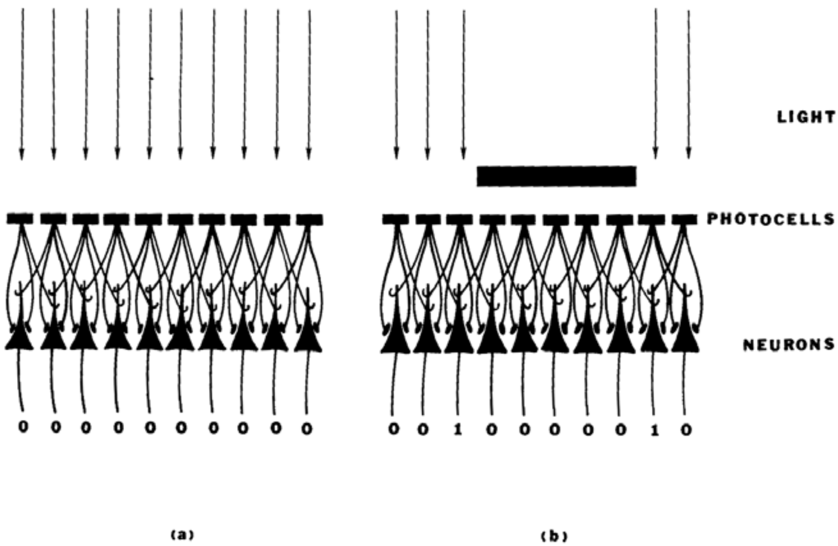
I, who was the editor after all, did not want to allow this to go on, for what would I then do with the rubbish that would then be part of the transcript? So I found it horrible that these terrible attacks were being unleashed against dear Ross Ashby.

I turn to the chairman—that was Warren McCulloch—and say, "Mr. McCulloch, I would like to make a motion: Why don't we allow Professor Ashby to finish his lecture, and set a time for the discussion at the end of the lecture?" Whereupon McCulloch turns to the other members of the group and says, "Please, let's not interrupt Ashby anymore now, but allow him to finish his lecture." After the meeting, McCulloch's mother-in-law invited all the Macy people to her New York apartment, where she served coffee, brandy, wine and a lot of such goodies. So we get upstairs for this meeting, and Ross Ashby turns to me and says, "Dear Heinz von Foerster, I will never forget that you rescued me from this embarrassing situation."

Paul Weston and the *Numarete*

Slowly I began to build up the Biological Computer Lab. In the meantime, the word had gotten around at the university: "There is that strange laboratory, where they are interested in cognitive processes, computers, systemics, et cetera." Young people came pouring in and asked: "May we play along?"

Then a very interesting young man arrived: Paul Weston. I consider Paul Weston to be a genius. He built the first parallel computer in the world. It was called *Numarete,* a numbers retina. This machine consisted of four hundred small photocells that were arranged in a square of twenty by twenty. When you lay down anything on this square photocell system—two coins, a wallet, and a ring—and then pressed a button, the machine said "four." Why four? Because four objects lay on the machine. The machine computed the number of objects, irrespective of the illumination, irrespective of the size, the position and the form of the objects. So that was a very general counter, which did not count in the sense of "one," "two," "three," "four," "five," but that saw the twenty-five-ness, and said "twenty-five." That was the idea of abstraction. You

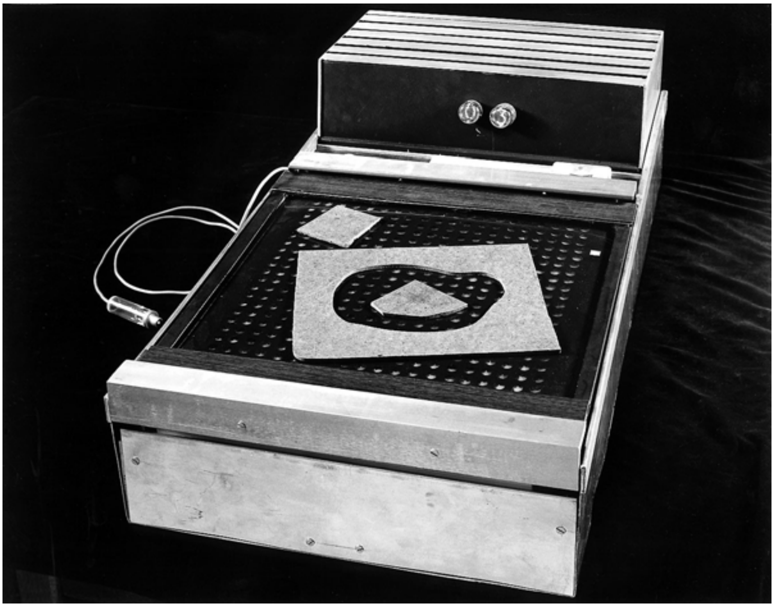


Model of the structure and function of a special network in a one-dimensional retina. This system is able to detect an object at once, and sees different numbers of objects as different properties: "seven-ness," "twenty-ness," etc., analogous to the human perception of "redness" and "greenness." As a periodic network, the system calculates the invariant "edge" regardless of the position and size of the objects, and regardless of the strength of the light.

abstract from the form, et cetera. You only say, "Here is one unit, and there is another unit."

John von Neumann visited us frequently. Of course we gave him a demonstration of the first parallel computer. He said, "I will run a test right away. Please bring me a pretzel." He lays the pretzel on the Numarete, puts a coin in each curve of the pretzel, I think, and presses the button. The machine says, "Three." — "Wow!" he says, "How did you do that?" Anyway, he tried to fool the machine. But the machine always gave the right answer.

The rumor about this interesting machine, of course, even reached those TV people who report about science. CBS, that is, the Columbia Broadcasting System, invited me to demonstrate the machine: "We'll pay for your flight from Champaign to New York. Bring the machine with you, and give us a demonstration." I say,

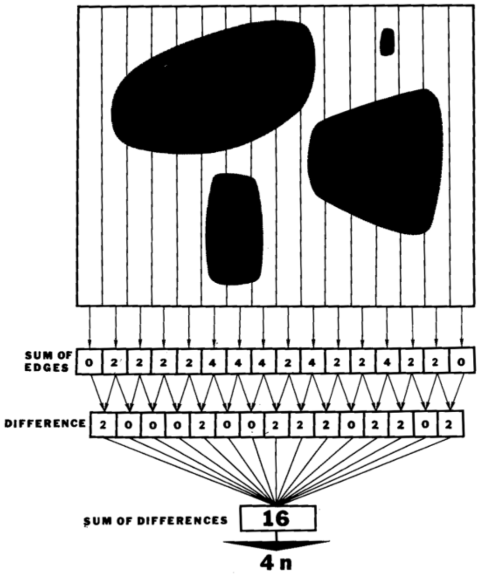


The Numarete, an artificial "retina" that counted the number of objects regardless of their size, position, and shape, and regardless of the strength of the light

"Fine, splendid," convinced Paul Weston to put the machine in a suitcase, and flew with him and the suitcase to New York.

Unfortunately, the men dropped the big suitcase with the heavy machine, before putting it on the plane. So we feared the worst. We arrive at CBS, plug the machine in, and naturally, nothing works. The machine is blind. So there we were with our parallel computer. "What are we going to do now? Fix it? No, we can't fix the machine. That would take one or two days." So I said, "We'll simply pretend that the machine is functioning."

Well, the CBS people immediately were prepared to do that. My suggestion was: I stand on a ladder in the studio, look down from up there at how many objects they put on the Numarete, and if I see twenty-five from up there, I press the button for twenty-five, and then the Numarete says; "Twenty-five." That's how we did it. Paul Weston spoke about it beautifully: "Now, ladies and gentlemen, I put five objects on the *Numarete."* I press the button—*blip*. Paul



Functionality of the Numarete: The two-dimensional network can count convex objects instantly regardless of their size and position, and regardless of the strength of the light.

Weston says, "Do you see what the machine says? Five." That is how we demonstrated the machine, without it being able to do anything.

Gordon Pask and his *whiskers*

Gordon Pask built beautiful self-organizing systems. Actually, he produced metallic solutions. When one attached electrodes, the most beautiful ferns, metallic ferns and strange blossoms with little shags sticking out in all directions, grew in these solutions.

He called them "whiskers." They organized themselves. Electricity separated the metal components. They stuck to one another and formed a self-organizing system. At that time, 1958/59, the term "self-organizing systems" suddenly turned up. It had been invented in 1954 by two researchers, Belmont Farley and Wesley Clark, who worked at the Lincoln Laboratory at MIT in Massachusetts.

The order-from-noise principle

We heard about self-organizing systems and were totally enchanted by this idea. I said, “That is a fabulous concept. We are very interested in that.” I knew that the Office of Naval Research, our sponsors, knew of Farley and Clark. And so I went to Washington and said, “You’ve absolutely got to organize a conference on self-organizing systems.” They said, “Good, that sounds very interesting. We’ll do that. We’ll pay for that.” That is how the Conference on Self-Organizing Systems came about in the year 1959. The organizer was the son of the very important and ingenious mathematician Hermann Weyl.

I believe I delivered a very interesting paper there. It was called “On Self-Organizing Systems and Their Environments.” There I introduced a measure of order that was then picked up by many people and became well publicized, was mentioned all over in various articles. I used this measure of order, among other things, to develop the *order-from-noise* principle, that is, the idea of an “order from disorder.”

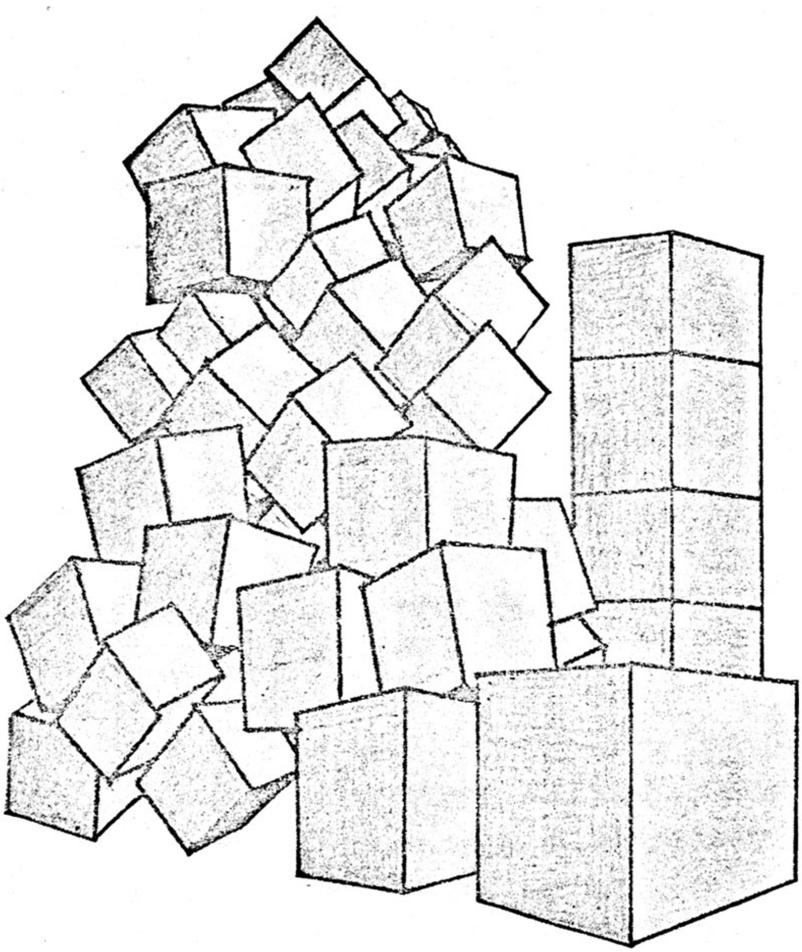
Erwin Schrödinger, the Nobel Prize winner, the inventor of wave mechanics, published a very nice book in 1946: *What is Life?* In it he said, “The astonishing thing about life is that it always maintains a high level of organization, a high degree of order, even though there are disturbances, from without and from within, that take place all the time. Think of a gene, which for generations creates the same characteristics in its heirs, as for example the gene for the Habsburg lip. Look at Charles V. He is the one who started with the Habsburg-lip gene, and all of the German emperors who followed him have this nice Habsburg-lip gene. This gene retains the same chemical organization, even though it travels through bodies that all have a very high temperature, 36.6 degrees (97.88 °F), which actually could have already destroyed such an organization a long time ago. No, the Habsburg-lip gene sustains itself for generations and generations.” Why? Schrödinger says that the living organization imports order from outside, that is, creates order out of order. I picked that up and said, “It is by no means necessary to import order from order. One can also create order from disorder.” Thus I introduced into the idea of self-organization that when there are potential connections present in a system, all you need is a little bit of unorganized energy, that is, *noise,* to bring about this relational structure.

We did some very fine experiments in this direction. For example, we built small wooden cubes, out of very light wood, so that they swam in water. Then we cut out small, flat magnets and glued them onto these wooden cubes in such a way that they pointed either to the North Pole or the South Pole.

When you throw these cubes with the glued-on magnets into the water and begin to stir, that is, introduce *noise,* they stick to each other, forming the most fabulous shapes, the craziest structures. You think that you’ve created a surrealistic world from this stirring. By introducing *noise,* that is, undirected energy, into this system, the potential relational structures lying within the single elements become realized. That was my proposal on self-organization.

The idea is the following: You have a system composed of elements that at first are in no way connected to one another; but each element has the potential for a connection. Take for instance a company. At first it is an aggregate of people that have nothing to do with one another, but each one of them can coalesce with another.

The only thing that you do is feed them. That is, you introduce energy, so that they don’t die. After a while they will begin to talk with one another, will form a club; one of them collects stamps, the other looks at pornographic books, et cetera, et cetera. After a while there is order in the system. What does order consist of? My proposition is: You can see order when the description of a system becomes shorter and shorter. When the description of a system is still very long, the system is not ordered. When the system is not ordered, I have to indicate for each element of the system as to where it is located, in what direction it is pointing, in which direction it is looking. The moment a relationship is established

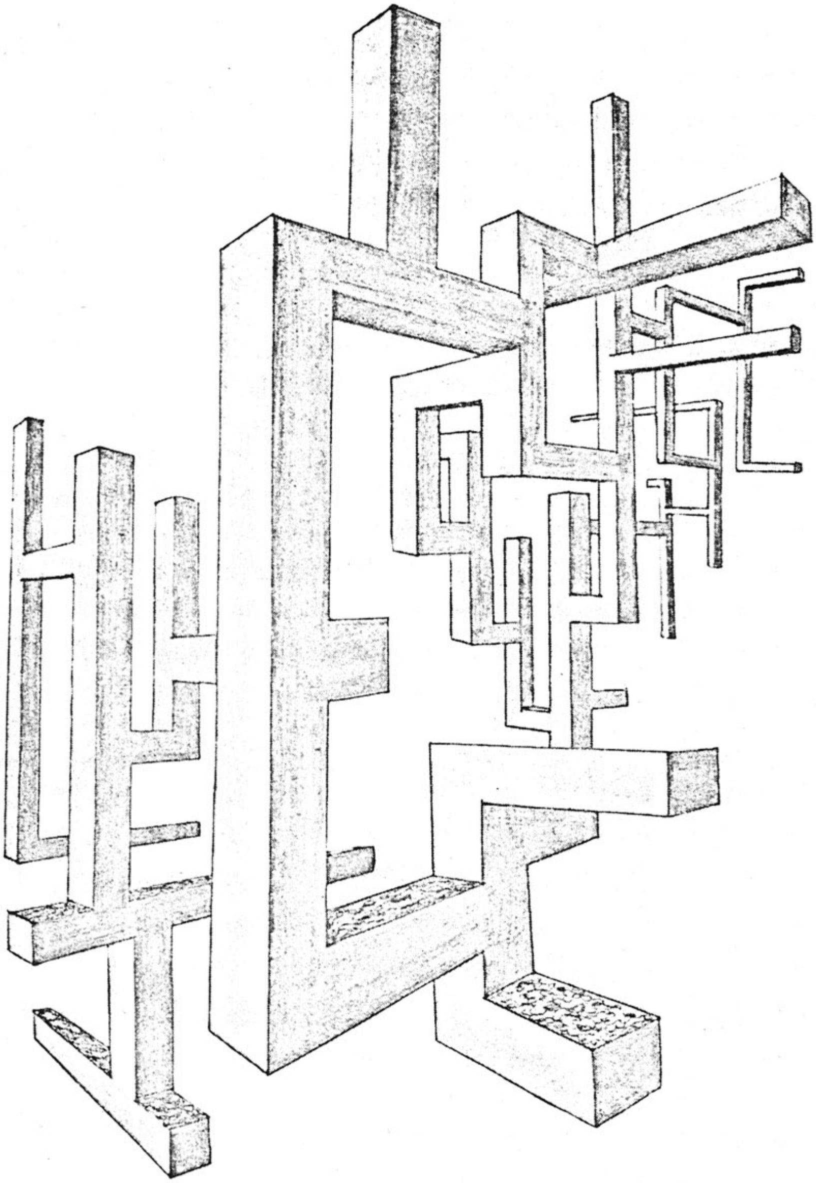


Wooden cubes: before ...

between the elements, the descriptions become briefer; for when I say about a group with only three such elements, “Those are located in the left corner,” the description has already become shorter.

Paul Weston and the pentagon-dodecahedron

Paul Weston produced the first recursive operators. He built a pentagon-dodecahedron, that is, a five-angled, twelve-planed object—it looks like a soccer ball. He glued a tiny light bulb on each one of these angles, and in the middle there was a battery. The little



... and after

light bulbs were connected in such a way that when one tiny bulb lit up, another one turned off. So the tiny little bulbs lit up beautifully, like lightning, all around the soccer ball; first in a totally confusing, chaotic way: One could hardly see any pattern. After a while, one could recognize with greater and greater ease: First this little bulb lit up, then that one. Finally it turned into a totally stable pattern.

Today people would call it an "attractor." At that time we called it "recursive functions," with "eigenvalues," since I was influenced by nineteenth century mathematics, by Hilbert and similar people. Today it is called "chaos theory" with "attractors." It is exactly the same thing, just furnished with a propagandistic terminology. Everybody listens when I speak about chaos theory; nobody listens when I speak about the theory of recursive functions. A few years earlier, a Frenchman, René Thom, had given the name of "catastrophe theory" to an amusing theory. Naturally, it became incredibly well known. Everybody talked about "catastrophe theory." That, of course, was very old hat, stemming from the nineteenth century; just that it wasn't called catastrophe theory, but bifurcation theory. So when one gives these things a new name, they immediately become very modern; and the more catastrophic the names, the more popular they become.

So that was the Conference on Self-Organizing Systems. Gordon Pask came with his "whiskers." Paul Weston with his incredible apparatus.

An automatic flycatcher

Next to Paul Weston and Gordon Pask there were some quite brilliant and enjoyable students at the BCL. They worked there and were paid on a contractual basis. I always said, "What would you like to do?" Then the student said, "I would love to catch flies." — "Excellent; then you can begin to think about building an automatic flycatcher." Then he thought about building an automatic flycatcher. People loved to work there, because they could do as they pleased. And the moment you let people do what they want to do, they are very good. Then somebody invents everything that is needed to build a flycatcher.

The *Principles of Self-Organization* conference

After the Conference on Self-Organizing-Systems, which was a big success, I told the Office of Naval Research: "The idea of self-organization is a fascinating problem. I would like to arrange for a conference with the title 'Principles of Self-Organization' at the University of Illinois. What are the fundamental principles under which self-organization can take place?" The Office of Naval Research accepted the proposal.

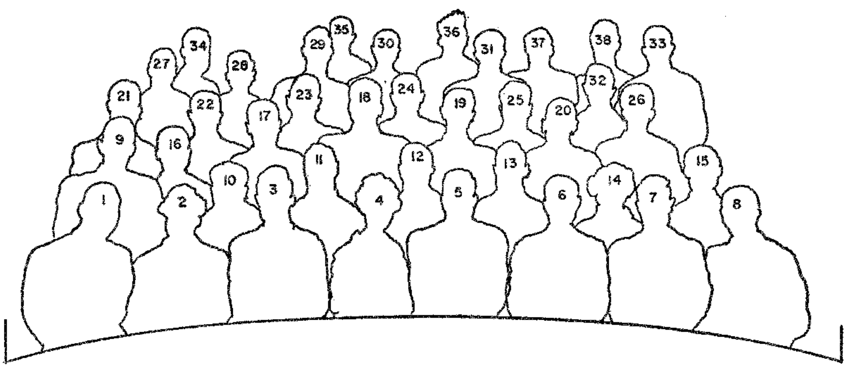
The conference "Principles of Self-Organization" was then actually held in the year 1960 at our university in Illinois. And I really invited an outstanding group of people. Two Nobel Prize winners were among them. One of them was Roger Sperry, the other Friedrich von Hayek, who had gotten a Nobel Prize in Economics. Then there were other fabulous people: Ross Ashby, Stafford Beer, Ludwig von Bertalanffy, et cetera, et cetera.

I had invited all these very smart, big professors from all over the world, but no woman was among them. Whereupon I said to my dear friend Cornelia Schaefer: "Cornelia, come, be with us! As a beautiful woman, you can play the part of the catalyst, so that these stiff professors finally loosen up and perhaps talk away merrily." Cornelia played along with enthusiasm.

It was the most beautiful picture that you can imagine. Cornelia sat like a queen in an enormous club fauteuil, and the Nobel Prize winners, mad about her, kneeled before her on the floor, and told her about their theories. Cornelia said, "Oh, how interesting! Tell me more."

She told very amusing stories about von Bertalanffy. He had just begun to study cancer cells, and wanted to tell her about his theory on cancer. And so he continuously came running with pictures. They were pictures of cells that were either cancerous or not. "Look, here is one of the most beautiful pictures I ever took!" And then he showed her yet another broken cell. "And here is an even more beautiful picture!" She was totally enchanted about how enthusiastic people were about their own projects.

Participants in the Principles of Self-Organization conference, 8 and 9 June 1960



1 Saul Amarel, 2 Gordon Pask, 3 Manuel Blum, 4 Kathy Forbes, 5 Peter Greene, 6 Ross Ashby, 7 Jack Cowan, 8 Heinz von Foerster, 9 Alfred Inselberg, 10 Ludwig von Bertalanffy, 11 Scott Cameron, 12 Murray Babcock, 13 John Tooley, 14 Cornelia Schaeffer, 15 Stephen Sherwood, 16 George Jacobi, 17 Hans Oestreicher, 18 John Bowman, 19 Jack Steele, 20 Friedrich von Hajek, 21 Hewitt Crane, 22 Anatol Rapaport, 23 Raymond Beurle, 24 Jerome Elkind, 25 John Platt, 26 Charles Rosen, 27 Roger Sperry, 28 Frank Rosenblatt, 29 Joseph Hawkins, 30 Albert Novikoff, 31 Stafford Beer, 32 Paul Weston, 33 David Willis, 34 George Zopf Jr., 35 Albert Mullin, 36 Warren McCulloch, 37 Marshall Yovitz, 38 Leo Verbeek

The Ashbys

During a break at this conference, Stafford Beer takes me aside and says, "Dear Heinz, Ross Ashby is very unhappy as director of this very big psychiatric asylum in England. Why don't you invite him to become a member of the BCL?" I say, "Ross Ashby, the big psychiatrist? I should invite *him* to the BCL?" — "Yes, he would be very happy. Why don't you suggest it to him?" Then we're all sitting at a table, eating our lunch, and I turn to Ross Ashby: "Dear Ross Ashby, would you accept an invitation to the BCL?"

Whereupon Ross Ashby gets up and says, "There's nothing that I would rather do. Permit me to call my wife." He goes to the telephone and calls his wife in England. After five minutes he returns, with a very long, sad face. "I tell her that I would like to go to America in order to work at the BCL. Do you know what she says? 'Over my dead body'." — She absolutely did not want to come. Whereupon I say, "That is very sad. Well, but why not?" — "Well, we just bought ourselves a beautiful house in England. She designed one of the most beautiful stone gardens. She can no longer leave all of that." I say, "That is very sad. Perhaps I could talk to your wife?"

Indeed: One month later, I was invited to a conference in Locarno, in a beautiful Swiss castle. And so I get there. And there Ross Ashby arrives with his wife Rosebud; and her maiden name was Thorn; she was Rosebud Thorn. Very close to Locarno there is a very famous puppet show, and the organizers said, "These guests from all over the world must absolutely go to this puppet show." And so we went there. It was a rather small room, not more than thirty or forty people can sit there, and I had no place to sit. And there, Rosebud thought that I could sit on her lap. Whereupon I sat down on her lap. And there I talked to her once more about coming to America. And I succeeded, during this puppet show, to convince Rosebud Ashby to accept the invitation to America.—I only want to describe how the Biological Computer Lab came about.

Ross and Rosebud arrived that very same winter in New York, in their own car. Ross liked to drive very fast, with small sports cars. He had a Jaguar racing car, a convertible. And so he gets to New York, unloads the convertible from the ship, and the coldest winter of the century begins to spread over New York and Pennsylvania. So Ross and Rosebud drove in this convertible from New York to Illinois; through the entire snowscapes, with snowplows just ahead.

Finally they arrived in Illinois, directly in front of our house. I see: There comes this small convertible; Ross gets out; and a giant gets out: that was Rosebud, wearing a Russian fur coat. Both came inside, with a bottle of champagne in hand. Ross gives me the bottle of champagne, a greeting from Stafford Beer; and the moment he gives it to me, it makes *bubb!*, bursts, and all of the champagne trickles out of the bottle, in the form of snowflakes.

Rosebud comes into the room with her Russian fur coat and says to Mai: “Don’t expect me to get much smaller underneath. Once I weighed as much as my husband, our three children, and the dog.” She takes off her Russian fur coat and, really, was not much smaller or thinner after she got out of the fur coat.

Ross stayed with me for ten years. He had just written this incredible book, *An Introduction to Cybernetics*, and I asked him: “Would you perhaps use this book as the basis for your course ‘An Introduction to Cybernetics’?” Ross said, “I’ll do that with great pleasure.” And so we announced the course, and Ross started to teach the course in the spring. After about one or two weeks, students from this course came to me. They wanted to talk to me. They looked very strict and very serious. I said, “Well, what is the matter?” — “We want to complain.” — “Well, what do you want to complain about?” — “We are in Ross Ashby’s course. And do you know what he is teaching us” — “Why, of course, ‘Introduction to Cybernetics.’”— “No, trivialities.” — “Trivialities? But ...” — “No, no, we don’t come to the university for that.” I said, “Well, calm down, and wait two or three weeks. It will surely become more difficult.” They leave my office somewhat dissatisfied. Five minutes later Ross Ashby comes to my office. I say, “Ross, it’s great that you are here. A group of students was just here. They complained about your course.” Ross looks at me with big eyes: “They complained about *me?"* — "Yes, they said you taught them trivialities." Whereupon Ross looks at me, beaming all over his face. "It took me twenty years to make these ideas appear as if they were trivialities." There you've got Ross Ashby in a nutshell.

Then the word about this Lab spread further and further. People came from all over the world.

Lars Löfgren

Lars Löfgren, a Swede, a logician from Lund, had heard about this Biological Computer Lab, knew about us from the papers, and he was in Los Angeles.

There he said to himself: "Perhaps I could visit Foerster in Urbana. But first I have to buy myself a car." A car dealer calls me from Los Angeles and says, "Tell me, do you know a Lars Löfgren?" I say, "Well, of course." — "He is here right now and would like to buy a car." — "Well congratulations, you've been lucky." — "Can I trust him?" — "Well, of course. It's Lars Löfgren, a world famous professor. He can pay for all that. You can sell him a car, no problem." Lars Löfgren bought the car there and drove it on famous Route 66 to Champaign-Urbana. One afternoon, suddenly a giant comes into my garden in Champaign, and says, "I am Lars Löfgren." He then played along with this group and was a dear, smart, and outstanding individual. He worked with us for many years and wrote wonderful papers.

Baby and the computer-generated star catalog

Many of such people came. One day a young woman, a student of astronomy, came to me crying, saying that she had told the head of the Astronomy Department: "I want to create a catalog of the stars on the computer, so that by simply pressing a button for any of the stars, we would be able to state the characteristics, the temperature, size and speed of this star."

Whereupon the head of the Astronomy Department had said, "Don't you know that departments of astronomy do not have computers? We have telescopes. If you want to do something with computers, you have to go to the Computer Department." So that is how she came to me. Of course I immediately gave her a position. That's Lenore Sarasan. We called her Baby. An incredible graphic artist. In Chicago she's now got the biggest catalog of all museum collections on the computer. If you would like to know where to find a Frans Hals, from 1615, with a gray background, you input that, and the computer says, "The Staempfli Gallery has that Hals."

Humberto Maturana

Of course I went to all the conferences that dealt with information processing and the nervous system and so on. Ralph Gerard, a former participant of the Macy Conferences, organized a big conference with the title "Information Processing in the Nervous System" in Leyden, in the year 1962, and I flew there.

The conference had actually been set for Monday and Tuesday, but happily for me, Ralph Gerard had already invited us for Friday. I found that to be an ingenious idea, since people coming from America have jet lag. And then there is Scheveningen: marvelous beaches right around the corner. So I was delighted. I arrived on Friday evening and found a piece of paper in my room. Ralph Gerard writes: "There will be a small welcome reception tomorrow. Come to the university at nine o'clock." And so I got there at nine. Ralph Gerard stands there and says, "Ladies and gentlemen, I am happy that you are all here. For today and tomorrow, Saturday and Sunday, I have planned a general rehearsal of our conference on Monday and Tuesday. So I would ask each of you to present your lecture today or tomorrow, take in the critique from the different members, and then present your corrected version on Monday and Tuesday." I thought I would go crazy. A conference serves the purpose of having people talk at the conference, not to have a general rehearsal beforehand. So I say to myself, "I have no intention of participating," and carefully sneak out of the room, which had many doors leading to a long corridor. So I sneak out, close the door carefully behind me, and look down the corridor. At the other end, someone else very carefully sneaks out of the room as well, and closes the door.

So I say, “Tell me, do you intend to participate in this pre-conference?” He says, “No, not at all. That’s much too boring.” I said, “Would you perhaps like to go with me to Amsterdam? We could go to the museum and dine very well. Would you do that?” — “Yes, I would be delighted,” he says. I say, “My name is Heinz von Foerster,” and he says, “My name is Humberto Maturana.” That is how we met.

Already then, Maturana had published a very famous paper together with McCulloch, Pitts, and Lettvin: “What the Frog’s Eye Tells the Frog’s Brain.” It is about the following. In neurophysiology they always thought that the little picture that is projected from the lens onto the retina is then conducted through the nerves into the visual cortex, and that one then “sees” there.

Maturana and Lettvin found out that that is not at all the case. What the cortex receives is an incredible, precomputed construct that has nothing to do anymore with the original picture. That is an astounding insight: The information about very fundamental characteristics, as for example, whether an object is round or angular, goes directly from the eye into the cortex, and is *not* first computed in the cortex. You already receive an analyzed picture in your cortex, before you even begin to reflect about what that all is. It is analyzed by the neural nets, which, from the very beginning, we concerned ourselves with intensely at the Biological Computer Lab.

So Maturana and I were the black sheep. When on Monday or Tuesday we were back in Leyden, the people no longer talked to us. It either was out of envy, because we had had a good time in Amsterdam, or they were really angry because we did not play this game. I immediately invited Maturana to come to the Biological Computer Lab, and that he of course he was delighted to accept. That was already a good start.

These personal relationships formed the basis for the group at the Biological Computer Lab. All were friends; all of them were probably, more or less, terrible kids.

Atmosphere of trust at the BCL

The atmosphere at the Biological Computer Lab was an atmosphere of trust. It helped create a network of friends that made the individual people so very productive and happy, whether it was the leading minds like Ross Ashby or Gordon Pask, or the students that fell into this net, who felt incredibly well, and suddenly saw that their so-called professors were their friends.

In a certain way, I would also say, it reflects on ethics. The ethics of such a system is based on trust. We could have taken each other for a ride. For example, I could have dropped Ashby. I could have taken Bigelow's side, who had said, "What do you want with your funny chess player?" No, Ashby trusted me, I trusted him. Maturana trusted me. "We can do that together." And there one also sees: He felt extremely well. Nobody doubted him. Everyone trusted him.

I could have withdrawn my trust. But I never withdrew it, for the essence of trust is that it lasts, otherwise it isn't trust. You must stand firm with it. That is to say, it is not, "You have to"; it is at least a part of my life. I trust the other, and I maintain: Trust is contagious. If I trust the other, he will suddenly say, "I cannot take Heinz for a ride. He is too naïf. He is too simple. He is like a child. He trusts me." So this connection of mutual trust made for the basic atmosphere at the Biological Computer Lab. Still today, many people come to me and ask: "How did you get this small group—so many interesting people—to work together so marvelously? What was the magic that held you all together and made you so happy?"

Ross Ashby wrote to me from England, after retiring: "Dear Heinz, these ten years that I spent with you were the most beautiful years of my life." That is very moving, isn't it?

Gotthard Günther and his place value logic

One night—at twelve o'clock in Champaign—Warren McCulloch called me from Georgia: "Heinz, I met an incredible human being. I gave a lecture, and he was the only one who posed really good and tricky questions. You've got to invite him." — "Well, why do I have to invite him?" — "Well, no one understands him. But the only one who will understand him is you. He is fabulous. His name is Gotthard Günther." Well, OK, if Warren McCulloch says that I should invite someone, I invite him, of course. And so I invite Gotthard Günther to give a lecture at the BCL Seminar. We had a lecture like that every week. Gordon Pask would join or whoever else wanted to. The BCL met in a classroom, and one was chosen to give a lecture.

So Gotthard gives a very amusing lecture. He always mixed it so well with stories. When he spoke about anyone, he immediately told a few anecdotes about that person. So this logic that he developed had a very human touch.

The logic itself was totally crazy. Yet I immediately saw that that was a very important contribution. Standard logic is two-valued, as it is usually called: A statement that one makes, for example, "It's raining today," is either true or false. A statement thus has two values: True or false.

Gotthard introduced something ingenious: He introduced a place where one can utter a statement. He called it *place value* logic.

That is to say, you first have to have a place in order to make a remark like, "It's raining today." Then you can decide: "Shall I put this statement in this place? Should I make the remark?" After all, you could say, "I don't want to put this statement in there," and use the place for something else.

Gotthard thus introduced a "rejection value." That is to say, you don't have to evaluate the statement, if you first reject it.

I found that to be very important for revolutionaries. Revolutionaries have always wanted to tear down the king or the university's administration: "Down with the administration!" — "Down with the king!" I have always told them: "But kids, you are making propaganda for the king, if you yell: 'Down with the king.'" Or: "You are making propaganda for the administration, when you yell: 'Down with the administration.' This way, 'administration' keeps coming up. You've got to leave out 'administration,' speak about the weather or about Marlene Dietrich, but not about administration." I found it to be important that the revolutionaries understand that—that in both cases, whether they say "yes" or "no," the king or the administration is there. But when the king or the administration is not there at all, you can say neither "no" nor "yes."

Well, Günther's place value logic was great fun for me. And he developed it further, into very complicated logical structures.

Günther originally came from Silesia; he was a Silesian peasant boy. Already as a child he annoyed his teachers. When they said, "You cannot count two prunes and three apples together," he would say, "I can so do that. I say, 'These are three apples and two prunes.' Where's the problem?" — "No, one can't count them together. One can only count apples together and then only prunes." He was a naughty kid. And then he studied logic, began to concern himself with Hegel. He married a Jewish woman, and when the Nazis came, he had to sign the so-called oath of confidence. And there he said, "I would be delighted to sign this oath, but unfortunately I cannot do that." — "Well, why not?" — "It is written in such poor German; I simply cannot sign that." — "But you have to!" — "No, no, I cannot sign such poor German."

Then, when he was on a lecture tour in Italy, his friends wrote to him: "Dear Gotthard, stay in Italy. Don't come back." His wife then followed him to Italy, and when the Italians then too allied themselves with the Nazis, they both moved to South Africa where, I believe, his wife had some distant relatives. There, in Stellenbosch, he taught logic at the university. He finally succeeded in emigrating to America, where he taught black children in some Southern school. That is where Warren McCulloch met him.

Well, I invited Gotthard Günther and offered him a position in the Department of Electrical Engineering, under the greatest of difficulties. For of course the chairman of my department said, "But he is a logician. He concerns himself with Hegel. He represents Far Eastern philosophy. We are a faculty for electrical engineering." I said, "That is why we need a logician, who is familiar with Far Eastern philosophy." Well, anyway, Günter became a professor with us. He was incredibly happy. Suddenly he could feel the ground under his feet again. His wife was also incredibly happy.

Murray Babcock's *dynamic signal analyzer*

I have already mentioned that an essential contribution that we tried to make to the entire idea of perception, of cognition, was the idea of parallel computing, as a possibility of calculating structures that are not computable actually with sequential operations such as the linear operations of John von Neumann.

One example is the so-called acoustical paradox. The acoustical paradox emerged with the study of human hearing. Helmholtz came up with a great proposition as to why we can perceive tones so exactly and precisely. He assumed that there were a lot of resonators in the ear that functioned like the taut string of a violin beginning to swing along with a specific tone that you play alongside it on another instrument.

Now there is this interesting rule in acoustics: The more precisely a resonator reacts to a specific tone, the longer it takes to gain resonance. That is, the more precisely the mechanism reacts to the tone, the longer it takes for it to know that the tone is there. The human ear, however, hears very precisely and immediately. That is to say, the human ear does not fulfill the law that it takes longer to recognize the tone, the more precisely it is perceived.

So either the law is not applicable to the human ear, or the human ear does not understand how it is supposed to behave in a physically correct manner. We then said, "Probably the human ear plays a very different role. Probably is doesn't play 'resonance,' but does something else. Perhaps the human ear is a parallel computer, which, as soon as a tone appears, computes in what area of the basal membrane this tone is at its highest." That is also a theory of resonance, so to speak, where, however, a simultaneous computation takes place to determine on which of the many resonators there are any vibrations.

Murray Babcock then built a parallel computer, the *dynamic signal analyzer.* When a tone was played, it immediately, with about one hundred or two hundred parallel sensors, ascertained what kind of tone that was. This machine not only solved the acoustical paradox, but was also able to analyze musical instruments that

The dynamic signal analyzer (Murray Babcock at left)

could not have been analyzed with normal microphones, for they had been built in accordance with the Helmholtz model.

When you blow a trumpet or play a violin, you can usually not tell what instrument it is, if you have not let it play for a while. But as soon as a tone sprang up, the dynamic signal analyzer was able to say what kind of a tone it was. And it could determine the variation of different instruments, whether they gave the right tone that then allows for the instrument to be recognized.

David Freedman and the first analog-to-digital converters

David Freedman, a musically inclined person, said, "We can also convert the dynamic signal analyzer, when we translate the continuous sound vibrations into a digital system." David Freedman was the first to build an analog-to-digital converter, so that a tone was then replayed in a series of zeros and ones. David Freedman built the first, very fast analog-to-digital converters. When you played any piece of music, it immediately came out on the other side in the form of digitals. You could then feed that into a computer and analyze it mathematically.

Herbert Brün

By way of our work with music we came upon Herbert Brün. I had met Herbert earlier on, since he had also emigrated from Europe, and was also at the University of Illinois in Urbana.

Herbert Brün was very interested in our musical studies. He was a professor of composition at the School of Music and worked in the Computer Music Lab and in the Experimental Music Studios at the University of Illinois. He did not only compose with notes for musical instruments, but composed directly into the computer; that is, he prescribed what tones and combination of tones were to be created by indicating the tonal frequencies. He wrote programs then that—very amusingly so—could be represented in two ways: In the form of graphics and in the form of music. That meant that you could hear as well as see what had been composed.

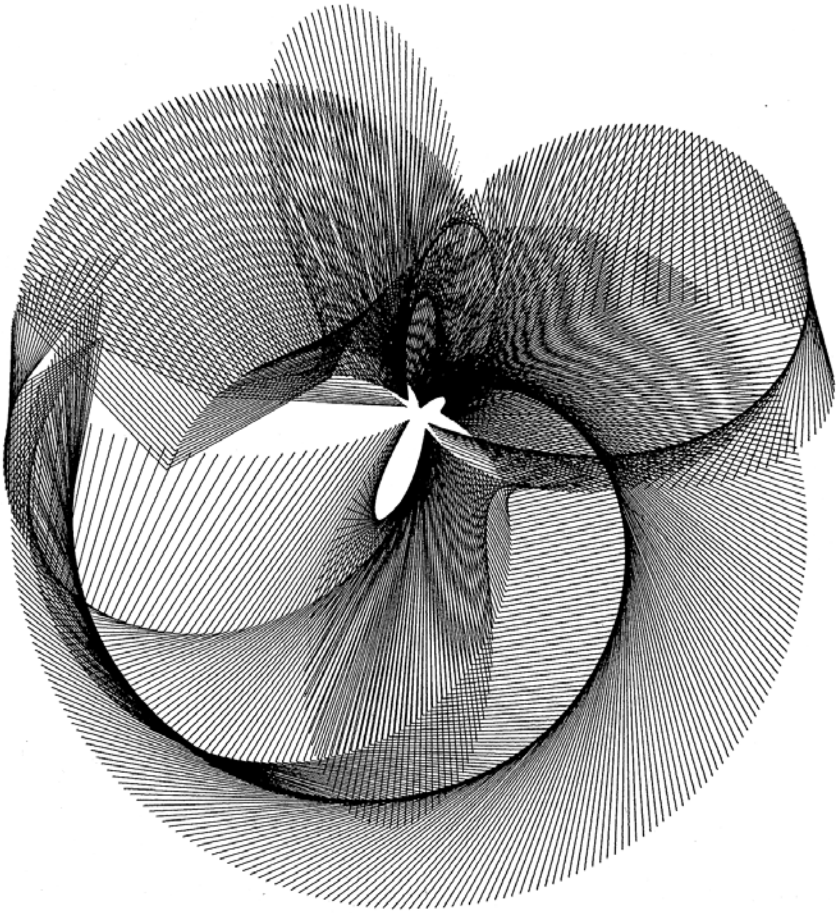
After some time David Freedman thought: “We have so many interesting insights regarding computation and music, let’s organize a lecture series for the 1966 Fall Joint Computer Conference.” That is a big circus. Computer experts come from all over the world.

So David Freedman and I incorporated a small session. Our session, entitled ”Computers in Music,” was a tremendous success. It resulted in a nice book: *Music by Computers.*

We worked very closely with Herbert because he was not only interested in computers, but also in perception and understanding in general. He was a close friend of our entire group.

Research projects: *Heuristics I and II*

Every year in the spring, the students of the University of Illinois arrange a big festival, where they convert the entire campus into a circus. They call it “Illioskee.” I believe the Indians that lived in Illinois were called Illioskee. They set up small booths. There they prepare their sausages and play their music. Small orchestras play



Graphic by Herbert Brün

there. The combos. And they have singers. And they've got agitators, who yell like crazy: "Down!" and so on and so forth.

Well, Herbert Brün and I used to stroll together through the masses of people and look for any talent we could find. We were on a talent hunt, so to speak. As we were strolling along, suddenly a young man approaches me. A giant, a beautiful young man; he looked like a Siegfried: blond hair, a very beautiful face. He approaches me, looking very serious, grabs me by my lapel and says, "Do you want to teach a course on heuristics?" Whereupon I ask: "Heuristics—in what sense? In the sense of a specific strategy to solve mathematical problems, or in the sense of an educational system, where the students find their own solutions?" I saw that he wasn't familiar with this difference. But smart, as he was, he says, "Both!" Whereupon I turn to Herbert, who is standing next to me: "Herbert, would you teach a course with me on heuristics?" Herbert says, "Every course suggested by students I will teach, for we are here for the students, not the students for us." Whereupon I say, "Yes, come to my office tomorrow; there we will work out something for this course." On the following day he comes to my office and says, "Dear Mr. von Foerster, I went from door to door throughout the entire university, in order to have them introduce a course on heuristics. All the people who decide what courses will be offered said, 'We don't do that.' I went to the Dean of Research and he said, 'I don't know anything about heuristics. We don't need that at this university.' I've undertaken all the steps to introduce this course, and you say, 'Yes, why not?'" I say, "All right; let's do the course. I'll be responsible for the course, you for the students."

So we announced the course for the fall semester of 1968. About fifty people enrolled. Since they came from so many different disciplines, such as sociology, biology, management, et cetera, et cetera, it was very difficult to find a common time to meet. I finally called the Center for Advanced Studies, whose director I knew very well. I told him about our situation. He said, "Heinz, I invite you to give your courses with us in the evening." Those were beautiful, huge rooms, with beautiful leather club chairs and an adjacent bar. Everything one needed was there: Devices for projecting, blackboards, everything.

And so we then met there in the evening. What was nice about it was that it didn't have a school atmosphere, but a club atmosphere; so that the speaker also became part of the class; not one man up front on the podium, telling the people what it is all about. One sat together, to get working on a problem.

Thankfully at the time I had some guests and members at the BCL, who played along enthusiastically. Humberto Maturana was already there at that time. John Lilly visited me from California at that time. John Lilly conducted interesting studies with dolphins:

Can one teach dolphins English, or can the dolphins teach us "Dolphinese"? So there were very interesting people there. I invited them all to tell us about their projects. And the students were very curious and asked: "Tell us, how is that? How do you do that? How did you get the idea?" Et cetera, et cetera. So the whole thing was actually a discussion club, where very interesting topics were being discussed. Students are actually very interested in certain problematic concepts that have never been explained or discussed anywhere. So they enthusiastically threw themselves into these topics.

Then we had an arrangement: The students themselves directed the course. At every meeting a different student was the chairman and leader of the course. The chairman decided who was to speak now and who wasn't. When a discussion started, we always listened to the discussion leader as to whether we should now say something or not. We subjected ourselves completely to the elements of the class, the students, because we told ourselves: "We are a university. We want to do something for the students. When they have questions, we will make an effort to answer them."

One of the students stuttered heavily. He could hardly talk. He always said, "I-I-I-I wou-wou-wou-would ..." et cetera, et cetera. Of course he was terribly afraid of having to become a class leader himself, and have to stand up front and say, "Ladies and gentlemen, the speaker today is Herbert Brün," or, "Today the speaker is Humberto Maturana," without being able to utter a single word. We completely ignored that. When it became his turn and he wanted to say something, we did not concern ourselves at all with whether he stuttered or not. When it became his turn to make his little speech, he managed to do it without any difficulty. We were, so to speak, a psychotherapeutic group as well. And the students knew that. "We know that we can support and change this situation or that, if we conduct ourselves thus and thus."

So the first semester worked out wonderfully. Everyone was thrilled. And we said, "We'll do a second semester." The second semester was even better.

There I had some other interesting visitors from out of town, all of whom presented with enthusiasm. In the second semester, in the spring of 1969, there were about seventy students.

Since these young people were so different, had come to this course for such differing reasons, one sometimes didn't see the nature of the questions: "What does this person want, when he asks this question? What does he want to talk about?" And there we introduced the following: I hung three cardboard disks around my neck that had different colors: a red one, a green one, and a black disk. And when a student asked any question: "Why are we now learning something about the retina?" I saw, "He doesn't want to know what can be said about the retina, but wants to know why we talk about the retina." And there I said, "Aha, that is a question regarding the course," and—let's say—put it on red. Then we said, "OK, let's talk about why we talk about this topic!" Or when someone didn't understand what a post-retinal network is, what a network is that is behind the retina—he simply wanted to understand the mechanics of the retina—I put it on green, and said, "So let's talk about the retina now!" The third was: "Why are we sitting here together in a course on heuristics?" Then I put it on black.

That was marvelous and contributed a lot to the discussion and also gave the students a clear picture: "What do we talk about now?" In private life that is a problem as well, that one doesn't know: "What *do* we talk about now? Do we talk about the fact that we don't like each other? Do we talk about the fact that we have difficulties living together? Or do we talk about your being of another political opinion that I am?"

We interpreted heuristics to be what heuristics meant originally, namely "to find something." The Greek word *heurískein* means "to find." *"Heureka!*—I found it" is what Archimedes called out when he sat down in his bathtub and noticed that he had become lighter. There he invented the famous Archimedean principle: that a body in the water becomes lighter relatively to the mass of water it displaces. He suddenly saw that. He had an insight. And there he called out, "Eureka! Eureka!—I found it." And since then, *heureka* is the basis for the idea of heuristics. How does one go about finding something? The mathematicians took that on. And the pedagogues took that on; for a technique of giving a course, teaching a course, where the students themselves develop the finding of a solution, not where the professors tell them the solution.

At every meeting there was a scribe. It was a student, who recorded what was being discussed in class. Students took turns.

Research project: *Heuristics III—The Whole University Catalog*

When the second semester was over I said, "Those were two beautiful semesters on heuristics. Unfortunately I have to stop giving that class." Herbert Brün was going on sabbatical, Humberto Maturana had to go back to Chile, John Lilly went back to California; these very interesting colleagues were all gone. And I had one new big research commission that demanded a lot of work.

I said, "Unfortunately I cannot do that anymore." The students objected strongly: "That's not possible! You've got to do it! We will help you do this course. So please, announce it again." And since I am a sucker, I naturally gave in: "OK, if you help me, we can do it."

So I announce the course once more. The fall semester of 1969 rolls around. The first day is, of course, announced in the university bulletin: "First meeting in room such and such, in building such and such, at three in the afternoon." I go there. It was at the time of the student revolts. The students ran around the campus and said, "We are against the administration!" — "We don't have the right courses!" — "The teachers are too lazy!" — "The testing system is lousy!" — Everything is a scandal!" The students spent more time on the street than in the classroom. So I get to the building where the course is supposed to take place, and I don't get in. All the entrances are blocked by young people. I work my way through, want to get to the room; but nothing doing. The hallways are full of young people. I finally ask one of them: "Tell me, what is happening today? Why can't one get in?" — "Well, we would like to get into one course." — "And into what course do you want to get?" — "We would like to enroll in the course on heuristics." — "All of you want to get into Heuristics?" — "Yes, today is the first day."

Well, I immediately saw: This Room 212, in which we were supposed to meet, that could fit perhaps thirty people, won't do anymore. I immediately went to the administration and asked whether we could have the big auditorium. We got the auditorium. Everybody marched into the auditorium. There were about one hundred and sixty students. While I go over there, I look around for the students who had wanted to help me give the course. None of them showed up. Oh well, they were graduate students, who did ask me to continue with the course, but naturally, didn't have the time to play along. They are writing their doctoral dissertation, they write this, they write that. They also have to go to their classes. They couldn't do it with me. So I walk over there: "Heinz, now you've got to think fast. I am now supposed to give a course to those one hundred and sixty people. What can I do?" And so I wander over there, as slowly as I can. I get there. And the following occurs to me: The students ran around in the corridors, because they are not happy with the university. And so I thought to myself: "Perhaps we could write a book as to what a university should be like, or how they would like a university to be."

At that time there happened to be a magazine that was fabulously laid out. It was entitled *Whole Earth Catalog.* It had been developed by Stewart Brand in California. The *Whole Earth Catalog* described everything you needed to know, if you wanted to be a revolutionary. There was a section on *nomadics*. One could buy tents there, made by reliable firms. One could buy megaphones. You could buy small ovens, that you could then use in the park in the tent to heat up or to fry your potatoes, et cetera, et cetera. A wonderful collection. And books that you ought to read so that you become a good revolutionary. Everything could be found in this *Whole Earth Catalog.* And so I thought to myself: "Since that is so popular"—each one of the students had a *Whole Earth Catalog*—"we'll simply make a *Whole University Catalog."*

And so I said: "I am very happy that we are all here. My plan is to write a new book together with you, in which you can say how you would like a university to run; and we'll call it the *Whole University Catalog.* In order to do that, we have to cooperate; we all have to work together. So at first, lots of groups should be formed of those interested in similar topics. Some are interested in history. Some are interested in photography. Some are interested in politics. Some are interested in this or that. I'll now distribute some pieces of paper, and you'll write on them your name and what you are studying: journalism, biology, whatever. Then you'll write down what you are interested in: whether you would like to be revolutionaries, or whether you'd like to paint, whether you would like to do this or that! Write it all down! And your address and your telephone number."

They all did that. On the following day I produced a little booklet from it in the Biological Computer Lab, a "Directory of Heuristics." That in itself is, in my opinion, a very interesting document.

If you wanted to know who else was a photographer, you looked it up in the index. It said that Numbers 5, 11, and 17 were photographers. And so, actually, groups could be formed. This Directory is a very important cultural document for me, for it shows what kind of a position the students took vis-à-vis the university. For example, about thirty percent of the students did not take my suggestion of doing such a project seriously. Under "Interest," which was shortened to BAG for background, many of them wrote "sex," or "sleeping," or "doing nothing."

A girl, for example, wrote: "Trying to get laid all the time."

Of course I included that in the Directory. So when she then got it on the following day, she found herself. She came to my office, drenched in tears, and said, "How can you print such a thing? How can you do that to me?" I said, "But why? After all I said, 'Everyone writes down what he would like to do,' and if you had rather wanted to do something else, you would probably have written something else."

The first lesson that the students got from this course, in my opinion, was: One can be taken seriously. That was something that they were not used to anymore. That when you say something, the other can listen and say, "I understand what it is that you want." That in itself was already an incredible insight. The groups formed the way I had hoped they would: The photographers met each other, the writers met, those interested in similar topics met each other. So many different little groups formed that studied this or that.

A girl in a wheelchair—she had polio—came rolling into my Lab and asked if I had a camera. I said, "Of course I've got a camera." — "Well, could I borrow the camera?" She wanted to photograph the graffiti in the ladies' toilets. Of course I gave her a camera. She then rolled around in her wheelchair and photographed the graffiti of the ladies in the toilets. Then she came back. I said, "I am very curious how the photographing went." She said, "I made a mistake opening the film cartridge. Everything is ruined." I asked, "But why didn't you go to one of the other photographers?" who for sure would have helped her develop these films right away. Well, she didn't dare trust him to have played along. Could she have another roll of film? I said, "OK, here's another roll of film." She gave up the toilet program and instead photographed trees: "I thought the trees of the campus were so beautiful."

At the end of the semester they put their results together. They always pasted and painted the projects on a big piece of paper. One of them had friends at a newspaper published in Champaign. I think it was the *Champaign-Urbana Courier*. And so he succeeded in having it printed on newsprint, a very cheap way of producing it. One thousand copies were printed. They were sold for one dollar, and the net profit went to one of the institutes of the university that helped the handicapped students—to have ramps everywhere, et cetera, et cetera. They accepted that very happily.

The last page states what the class decided to do with the money. The mere development of something like this is so very interesting: That they always debated, discussed, and then chose this form and printed it on the back of the *Catalog*.

What was interesting was that when the university found out about this production, it said, "This can't be. We have to squelch it."

THE PUBLICATION OF THIS CATALOGUE WAS MADE POSSIBLE BY THE FINANCIAL SUPPORT OF ALL STUDENTS WHO PARTICIPATED IN A COURSE ON HEURISTICS (EE 271; EE 497; ENGL. 199; FALL 1969), A GROUP OF GRADUATE STUDENTS WHO WISH TO REMAIN ANONYMOUS, AND THE SCHOOL OF LIBERAL ARTS AND SCIENCES THROUGH ITS PLANNING COMMITTEE FOR THE L.A.S. SYMPOSIUM ON APRIL 12-17, 1970; AND BY THE IMMEASURABLE ENTHUSIASM AND UNCOUNTABLE HOURS OF WORK CONTRIBUTED TO THE ORGANIZATION AND ASSEMBLY OF THIS PUBLICATION BY MRS. ALEXIS PETERSON AND MISS JANET FICKEN OF THE BIOLOGICAL COMPUTER LABORATORY. ONE THOUSAND (1000) COPIES OF THIS CATALOGUE WERE PRINTED BY THE RANTOUL PRESS, RANTOUL, ILLINOIS AND THEY ARE SOLD FOR THE PRICE OF $1.00 EACH. SHOULD THERE BE ANY PROFITS MADE THROUGH THIS SALE THEY WILL GO TO THE UNIVERSITY FOUNDATION WITH THE SPECIFICATION TO PROVIDE FINANCIAL AID TO THE SPECIAL EDUCATION OPPORTUNITIES PROGRAM (S.E.O.P.) OF THE UNIVERSITY OF ILLINOIS IN URBANA.

Front and back covers of The Whole University Catalog

The administration of the University of Illinois did not allow this production to be identified with the university. Originally, it said *University of Illinois* at the top, and at the bottom, I believe, *Biological Computer Lab* or *Department of Electrical Engineering*. They said, “It can’t say that!” They did not want to be identified with something as disgusting as what we had produced. Therefore the students were asked to paste black strips over the identifying words.

One student, for instance, had taken a whole page to describe how to cultivate marijuana, how to cut it, how to make cigarettes out of it and how to smoke it. What was great about it was that since every student knew how to do that, he could afford to make a joke about it, and described everything the way it is *not* done. And of course we included it as a joke in the *Catalog*. The good professors fell for it and said, “Now Foerster publishes how to turn people into drug freaks. In the name of the university, in the name of teaching, Foerster teaches these nice blue-blooded young citizens of the state of Illinois in the use of drugs."

The senator from Illinois then cited me to attend a hearing. That was incredibly funny, since they didn't know that the whole thing was a joke. Mai was also there. The senator was totally crazed. I sat there and laughed myself silly. I only gave funny answers. I asked: "Did you study the marijuana contribution?" — "Of course! Scandalous!" — "Well, did you try whether it works?" — "We wouldn't do something like that!" — "Well, why didn't you try it? Then you would have seen that it doesn't work, that the whole thing is a hoax."

In the end, of course, they had to release me, but the university really tried to get rid of me. The entire *Whole University Catalog* was a thorn in their eyes. But they couldn't kick me out, since I had this wonderful tenure.

But you can see how through these and other similar incidents tensions arose between the university and me. For example, the president was also cited to this hearing, and he too had to say how he could support such a thing. And the first thing they asked me was: "Did you show that to the superiors of your university?" I said, "Well, of course. That was the first thing I did; show the *Catalog* to the president." — "Well, and what did he say to that?" — "He was shocked. He said, 'For Heaven's sake, what have you done to me?'" Naturally, everybody laughed again.

Then there was a break from Heuristics.

Research project on ecology: *The Ecological Sourcebook*

I was elected by the students to teach the honors class of the College of Engineering in the spring semester of 1970. Under the umbrella of the College of Engineering are the physicists, the chemists, the electrical engineers, the mechanical engineers.

The best students of every class are in the honors class, the ones who have only gotten A's in their coursework. They have the right to decide on a course, with a teacher of their choosing. And so one year they picked me as the one to conduct this course.

Everybody, of course, got sick over it. For Foerster, after all, is a child seducer, who incites the students to deviate from the straight and narrow.

So they elected me to further lead them astray. And I went there and asked myself: “What am I going to do with them?” At that time the problem of ecology had surfaced. “What are we doing?” — “We are destroying the world.” — “We are destroying the lakes.” — “We are destroying the air.” — “What are we doing?” And there I suggested to the students: “Why don’t we write an ‘Ecological Sourcebook,’ where someone interested in ecological problems can look up: ‘What are the problems?’ ... ‘What can I do to eliminate these problems?’ … ‘Where can I find sources, for reading up on what we can do with the woods and the lakes?’ … ‘How can one prevent the destruction?’ … ‘To which senators do I write about what?’ Let’s make such a book!” They adopted this proposition with great enthusiasm.

We did it, produced it and again printed it at the local newspaper, on newsprint as well. It turned out very nicely. It has around three hundred pages, so it is quite thick, with preformulated letters. When someone wants to write to a senator, “We’ve got to do this or that to stop the pollution of the air,” the letter is already contained in the book. All you have to do is copy the letter. The senator’s address is in the book as well. You take an envelope, stick the letter in it and send it to the senator. This *Ecological Sourcebook* contained many such points of reference, and the students who had made it were totally enchanted.

The assistant dean wrote a letter to the head of the Department of Electrical Engineering, telling him with how much enthusiasm the young people had participated in this course, and that he was very grateful to me for taking up ecology, the ecological problem.

Research project: *Heuristics IV—Cybernetics of Cybernetics*

I remembered that Margaret Mead had always told me: “Heinz, you have to write a book about cybernetics.” I said, “Dear Margaret, I don't write books. Perhaps I can write an article that stems from a lecture, but I cannot write books."

With the success of the *Ecological Sourcebook* and the *Whole University Catalog*, I said, "Perhaps with a class we could write a book about cybernetics." And so I announced a course for the fall of 1974 entitled "Cybernetics of Cybernetics" because, as I maintained: "We will hold the course on cybernetics cybernetically, so that the entire course will become a cybernetics of cybernetics." Many students came to register for it—forty-five, I believe. The course description already stated that we wanted to write a book that was to have at least five hundred pages, that was to be structured in such and such a way, that was to be richly illustrated, et cetera, et cetera. This course description is of course contained in the book, since it is a cybernetics of cybernetics; it has to be able to describe itself.

By the way, the expression "cybernetics of cybernetics" was also inspired by Margaret Mead. We had invited her to give a lecture at the first meeting of the American Society for Cybernetics in the year 1967. She came and held a wonderful lecture; she speaks totally impromptu; she doesn't need any notes.

We taped all the lectures at that conference, and together with some students, I edited these lectures in order to publish them as a book.

Margaret did not have a title for her lecture and was on the Trobriand Islands or in Samoa at the time, writing her twelfth book, watching how the girls there are thrown into the marriage market—as they call it in Vienna, "coming of age."

But we had to find a title for her lecture. So we always wrote her letters, in which we asked: "What shall we call your paper?" — "Would you like to correct your lecture?" But she never answered.

Finally I said, "I have to give this lecture a title." Now, she spoke cybernetically about cybernetics. She said, "A society has to conduct itself in accordance with its own maxims. If you are a cybernetic society, you need to notice when you are no longer functioning, and then perhaps decide to disband." And so I called her article “Cybernetics of Cybernetics.” That is where that combination appeared for the first time.

At a later opportunity—1974, Klaus Krippendorf had invited me to give a lecture at a conference of the American Society for Cybernetics in Philadelphia—I saw: “This idea of ‘cybernetics of cybernetics’ is a very good one.” Only then did I think of how meaningful these self-referential concepts are, these concepts that apply themselves to themselves. That is when I invented the idea of “second order cybernetics.”

Again, students came from all classes, freshmen just out of high school, students in their first year of college; then postdocs, people who had already completed their doctorate and held some kind of assistantship at the university. It was an incredible success. During the first half I always gave lectures and said what cybernetics is all about, what one concerns oneself with, what fields are touched upon, et cetera, et cetera. The students then investigated everything else from the literature.

They wanted to create a dictionary. So they selected some concepts like “consciousness,” “future,” or “truth,” and wrote to different great research scientists: “We are writing a book on cybernetics. Could you define ‘consciousness’ for us?” — “Could you define ‘intelligence’ for us?” — “Could you define this and that for us?” And these great scientists wrote back; many, almost all of them, answered. What was great was that it turned out that everyone defines “consciousness” differently. That in itself is already an important insight for a young person, who says, “Now I would like to know something about consciousness. And what is it that I am being informed about? About the writer, and not so much about consciousness.” That is how this book then came about. The students worked late into the night. Then they typed all the pages, pasted them up, provided them with illustrations, and laid them out in the corridors of my research lab. There the pages lay next to the pages, next to the pages, next to the pages; until they said, “Yes, these we’ll put here. That we’ll put there.”

Then two supplementary books were invented. The “Metabook” informed about the authors who had written contributions—what kind of people they were and what they were mainly interested in: cognition, the functioning of the nervous system, et cetera, et cetera. The Metabook was appended at the end. We used a Velcro fastening, so that you could take out the booklet and put it back in.

Then there was the so-called "Parabook." The Parabook is the book about the book, so to say; a meta table of contents, so to say. There you could find a list of the topics that were dealt with. There you could find the authors: Who the authors are and what they wrote about. The Parabook is exactly in the middle and has a gray edge, so that you can find it right away. "Where do I find the authors?" — "What topics are dealt with?"

I printed the first three hundred copies at my BCL printing shop. I had them bound by the University Press of the University of Illinois.

The students selected a very pretty cover: a painting by Pieter Brueghel, *Children's Games,* since, as they said: "Game playing is actually a cybernetic activity. One plays with the other; the other with the one. Circularity is already part of children's games."

In the course description I had already written: "We have to invent something so that the linearity of the book is cancelled out; so that one can easily get from one topic to another related topic in another part of the book; so that the topic is dealt with as a whole and not only one-sidedly." That was important, because the students at that time were totally against linearity; linearity was totally out.

One student had the great idea: "We'll simply make holes in the margin, so that when one sticks a pencil through these holes, one can get from one place where the topic is dealt with, to another place where this topic is dealt with again. So if, for example, you want to know something about cognition, you see neurology on page 110. Then you stick a pencil through the holes and get to page 450, where you can then find the philosophical or the logical treatment of cognition. Simply with a small hole puncher you punch holes and can then read this book cross-disciplinarily, so to say; not linearly, but spatially, so to say."

I believe every student got two or three copies. Then a few were left over, and that was actually the end of the story.

Several years later, when I already lived in California, someone accidentally found out about this book. It happened this way: I was at a conference here in California, in Asilomar, sitting at a table with several other people. Of course we talked: "What do you do? What are you doing here?" I asked my neighbor at the table: "What do you do?" He said, "I am a publisher of unpublishable books." I said, "Would you like to see the most unpublishable book?" — "Yes, of course, that would really interest me." — "You can see it at my house." Well, Asilomar is here on the coast, to the south of Santa Cruz. "When are you leaving?" I asked. "I am taking a plane this afternoon." — "From where?" — "From the San Francisco airport." — "Why, then you can come by my house; then I'll show you the most unpublishable book." So he really came by, in the afternoon for coffee. I show him a copy of *Cybernetics of Cybernetics.* We sat outside, on the deck. He looks at it. His mouth simply began to water. "It's not possible! Unbelievable! I've got to publish that." — "Well yes, but that is my last copy. Unfortunately I can't ..." — "No, no, I've got to publish that. You have to let me have the book." — "No, no, I cannot give it to you. It is my last copy." — "No, no, you've got to let me have it." Well, he finally won. He took the book with him and produced a new edition, an improved edition, for he used much better paper. I had only been able to use paper that happened to have been gifted to me by someone. The paper was enough for exactly those three hundred copies. He printed it all on white paper. He computer-enhanced the photographs, so that all pictures turned out to be more beautiful than in the original edition. I believe he printed one thousand copies of the book.

Quantitative hematology and the *von Foerster equation*

An important financial contribution to the Biological Computer Lab came from the National Institutes of Health. Almost thirty percent of the costs were covered by this institute.

We had a program on quantitative hematology with the National Institutes of Health. A little historical precedent for that:

The Viennese in Champaign

From the cosmopolitan city of Vienna, we had come to a tiny provincial city in America's Midwest, in the middle of these gigantic plains; to a small city in the Bible belt and in the corn belt of the United States. Bible belt, because everybody has three bibles lying around at home; the children are usually given biblical names. And the only thing that people live from is, of course, corn, that is, *Kukeruz*, as they say in Vienna.

We knew nobody. But by sheer accident we immediately got into a group of Viennese, who had also ended up in Champaign-Urbana. Why? Mai's friend, Ilse Nelson, who had already lived in New York for many years, who knew all the musicals, was on the best of terms with the opera, and knew all the people of the opera.

The name of the director of the Metropolitan Opera's chorus was Kurt Adler. Kurt Adler had gone to the academic high school in Vienna. A young man by the name of Ludwig Zirner had been in his class. Adler and Zirner became very good friends. Both became musicians. Zirner emigrated to America, because he and his wife were Jewish, and by chance came to Champaign-Urbana to the University of Illinois. When Adler heard in New York that another Viennese, who had gone to the academic high school, was coming to Champaign-Urbana, he said, "Ilse, you have to tell Heinz that a friend of mine, Ludi Zirner, is there. He is the director of the opera of the University of Illinois." So Ludi Zirner was notified that a certain Heinz von Foerster was arriving with his wife. "They are refugees from Vienna. They lost everything; were bombed out, have practically nothing left." The Zirners knew that. When we arrived there, we were invited to spend New Year's Eve—we had arrived near the end of December—at the Zirner's.

As I already told you, through a fortunate happenstance, my giant overseas trunk, my suitcase, which contained everything that I could save from Silesia, had not been affected by the bombing of the train in Kufstein. That suitcase, of course, got to Vienna and then to America.

And in it were our best clothes: Mai's most beautiful evening dresses, with gold lamé and all that you may want, my tails and my tuxedo; the most beautiful pictures; things that one couldn't get anywhere else.

So we got to this New Year's Eve party; I in tails, Mai in her gold lamé dress with the most modern cut, with a bottle of champagne. That is how the refugees from Vienna, who had lost everything, arrived. So you can imagine the astonishment and the laughter: "The refugees from Vienna, just look at them!" They all laughed themselves silly. It was a very beautiful party and we all became very good friends.

Henry Quastler and information theory

Another guest, whom I didn't know, was a doctor, a general practitioner from Vienna, Henry Quastler.

Quastler went to the local clinic and applied for a potential job as a physician. Of course he was not accepted, because he had not taken any American exams. He then took the American medical boards and was hired by the Carle Clinic in the small city of Urbana.

Quastler was an unbelievable human being. He was deeply troubled by the horrendous things that people did to each other during the Second World War: throwing atomic bombs that burned half the people alive. He said, "That is so cruel; one has really got to understand that. I have to examine the horrible effects, the influence of nuclear radiation on living tissue." And so he borrowed from the Department of Physics at the University of Illinois small alpha-emitters.

He conducted very interesting studies and thought: "How can I best represent the results?" He had heard about the so-called information theory and said, "Maybe information theory is the right format for representing the results of such radiation and to operate with them." He knew that I worked together with all the people that developed information theory: Norbert Wiener, Claude Shannon—I knew all of these people personally. And so he came to me: “Tell me, Heinz, can you get me books on information theory?—Can you help me understand this or that matter?” Thus I worked very intensively on information theory with Henry Quastler. I was fascinated with the speed with which he understood it. In 1952 he called a symposium in the state of Illinois, which he documented in his book *Information Theory in Biology*, which appeared in 1953. It was the first conference of its kind. They came from all over the world.

Henry Quastler was so successful with his work, that first the Department of Physics at the University of Illinois said, “Come join us and don’t work there in the basement of that ridiculous Carle Clinic. Work with us at the Department of Physics.” And so he moved and worked there. Then his papers became so well-known that the Brookhaven National Laboratory, which has those very big nuclear accelerators, said, “Come join us and work here.” And so he went to Brookhaven. The first thing that he did: He collaborated critically at a very big conference in Gatlinburg, Tennessee, on the same topic, “Information Theory in Biology.” That was in the year 1956. There is a book on that as well. Then in June of 1957, he organized the very big conference on homeostasis within the framework of the Brookhaven Symposia in Biology.

As an idea, “homeostasis” was already very old, but the whole theory of homeostasis, in connection with information theory as well, was not developed at all. This conference was called “Homeostatic Mechanisms.” He invited me: “Heinz, you shall give the keynote address, because you have such a clever way of talking about these things that other people can then understand them.” And so I held a lecture there: “Basic Concepts of Homeostasis.” I tried very hard to make the depth and the breadth of this concept so clear that people, who don’t live in mathematics, not in information, not in this world, could also appreciate the kind of mental tool that it is.

The world of hematology

During the break of that giant conference—there were hundreds of people, the best hematologists from all over the world—three very dapper looking gentlemen suddenly approach me; in impeccable business suits, with ties and all; well combed and brushed and groomed and all spruced up; they approach me, looking very serious.

"Dear Heinz von Foerster, we are hematologists. We come from the National Institutes of Health. We have no idea of mathematics. We are not mathematicians. We are not physicists. We are not formalists. You held your lecture in such a way that we, who are interested in the structure of the cell, were fascinated with respect to the possibility of applying the concepts that you presented to us. We understood every word of it and are infinitely grateful to you. We would like to invite you to tell us more about your ideas at the National Institutes of Health. We would also like to close a contract with you, so that you can work for us with your people at the university as well." Whereupon I say, "I am delighted with this idea, but gentlemen, it is the very first time altogether that I hear the word 'hematology.' So I cannot come up with any proposal as to how I should do that."

Whereupon they said, "You don't have to worry about that. We'll write the grant proposal and sign it. Then the only thing left is for you to sign that you want to take part in this grant." I say, "But if I don't know what hematology is, how can I sign a grant for hematology?" — "That is no problem at all. We invite you to fly to Bethesda, Maryland, always for two or three days, within a period of several weeks. There we will give you an introductory course on hematology."

In the period of time that followed, I was very touched by two points. The first point: That they were so totally carried away with their profession, dedicated themselves so fully to the development of blood cells and their count in the human body. The second point: That they understood that so well, telling me about it in the simplest of terms, so that I could easily follow.

A completely new world: I learned how cells divide, what the problem in hematology is, et cetera, et cetera. I will only mention one small problem. All blood cells originate from one primeval cell, a mother cell that always looks the same. It is called "stem cell." It divides and creates two cells. One changes a little, also begins to divide and to create daughter cells. They too transform; look different; look different again; different again. That is how the division of cells proceeds: with a simultaneous transformation of the form of the cell. Suddenly, after the fourth, fifth, or sixth generation, there is a cell stemming from a stem cell that looks totally different from that stem cell. The form of this cell can reveal to you the generation this cell is living at the moment, for it is—to put it simply—at first a sphere; then turns into a cube; then into a tetrahedron. So they all look different. In spite of that, it is not easy to differentiate the kind of state this cell is in now, what kind of a face it has. Does it have the face of a "promyelocyte" or that of a "myelocyte"?

Hematology and cognition

The National Institutes of Health sent us microscopic specimens, so that we could learn how they called these cells. When we received these microscopic specimens, we noticed that the cells that all looked like elephants, were sometimes also called "donkey." So we weren't sure whether they knew what such a cell looks like. And so we did an experiment. We cut these specimens up, and turned them by ninety or one hundred and eighty degrees. Then we sent them back and said, "Unfortunately we lost the description of these cells. Could you tell us again what kind of cells they are?" They then sent these specimens back, and of course the cells that were turned by ninety degrees were suddenly called something else.

Of course an elephant changes when you put it on its back; suddenly he looks different, not like an elephant. So we said that the counting of such cells in various life circumstances is a cognitive problem. We tested whether they themselves were consistent, and saw that they were not consistent.

So we ascertained that the counting of such cells is not an absolute matter, but requires further analysis of the cognitive capabilities of the human being observing these cells; and that fell exactly into our research specialty, since we concerned ourselves with cognitive problems.

When I told the organizers of my program about that, they of course were fascinated and said, “What you have found there is highly interesting. So we’ll have to analyze in far greater depth.” Whereupon our program, “Quantitative Hematology,” was extended into many other branches of the Biological Computer Lab, such as into the cognitive branch, where we were interested in cognition, distinction of configurations, form determination, et cetera, et cetera.

That we worked with the National Institutes of Health was a bonus for us.

Additionally, of course, I concerned myself a great deal with the mathematics of such not so well definable bodies. Very soon after the first contact with the National Institutes of Health I was invited to participate in a very big conference of hematologists in Salt Lake City. I was supposed to present a bigger paper and I tried to prepare myself very well. It was an international conference of hematologists, and my lecture was entitled “Some Remarks on Changing Populations.”

Population, because it has to do with the population of red blood cells and white blood cells. The whole matter is a population problem: First black children are born; they slowly turn green. Then they turn blue; then they have tails; then they have horns. So that is a “changing population.”

I made a special effort. First of all, because I found the problem fascinating; second, because nowhere in mathematics did I find links to where similar problems had already been dealt with.

So I tried to think my way into this topic totally anew. Conceptually it is not an easy problem. For days I tried to get my teeth into it. I think that what I experienced there was a kind of meditation. I no longer thought about anything other than how I could capture what I now wanted to describe in such a way that it could be dealt with mathematically.

After days of contemplation I finally thought of a way. I wrote it down. Then I tried to explain this way in even simpler terms, so that everybody would understand it. With these results I flew to Salt Lake City.

$$\boxed{\frac{\partial N_a}{\partial a} + \frac{\partial N_a}{\partial t} = -N_a\theta.}$$

The von Foerster equation

This paper, "Some Remarks on Changing Populations," was published. If I left any footprint in the world of science, it is with this publication. It is the only publication that was incorporated into the really big body of science. It entered the field of mathematics with my name. The "von Foerster equation" is to be found in the textbooks on differential equations, for it is a differential equation with very strange and interesting characteristics. It belongs to the class of hyperbolic differential equations.

A funny story: A professor of mathematics from the Department of Mathematics of the University of Illinois is invited to a Department of Mathematics in Israel. He gets to a lecture. The professor stands at the blackboard and says, "Now I will present to you the hyperbolic von Foerster differential equation," and begins to write the equation and the problems and the interesting solutions.

The professor from Illinois wonders: He actually knows a von Foerster. It can't be the same. That one is a nobody, who ponders about cognition in Electrical Engineering and gives horrendous courses, where the students practice "Cybernetics of Cybernetics." And so afterwards he goes there: "You mentioned the name von Foerster. What von Foerster is that?" — "Why, he's a colleague of yours. He is at the University of Illinois."

Suicide of the Quastlers

Quastler, who had introduced me into this whole world, had a charming wife, Gertrud, an artist, a painter, who also taught a class for the physicists at the University of Illinois. They all went to her class in the evening and learned how to paint. Unfortunately she had lung cancer, of which she was slowly and very painfully dying. As a physician, Henry of course could put his hands on any means by which you could transport yourself from this life into the next. And so both of them, Gertrud and Henry, got a hold of such a remedy and together committed suicide. The entire scientific world was in deep mourning. He was an incredibly dear and caring human being, who had held this ethical idea of examining the damages that radioactive emissions created in living tissue. A little book was written, an obituary on Quastler—*Henry Quastler* or *The Quastlers*—that I find very touching and beautiful.

Doomsday: Friday, 13 November, A.D. 2026

Back once more to the problems of quantitative hematology. What are the problems? What is one's concern there? In quantitative hematology it is a matter of deducing how many cells come into being or die there and there; it is a theory about the coming into being and the dying of blood cells. The problem is always how many cells of this kind can be found in a little drop of blood. It is therefore a somewhat extended population theory. One would like to find out how many cells of this kind then become how many of that kind. This population problem can of course be extended to other populations: Populations of ants, populations of people, populations of Chinese, et cetera, et cetera. That is a whole field in sociology and is called demography.

Some of my students, who were working with me on that, asked: "Couldn't we apply our theory to the human population? Couldn't we find out what a population looks like when the elements that grow up in the group cooperate with one another?" Most of the elements of a blood sample, or of animal populations in the wild, compete with one another. One cell needs what the other needs as well. That means that they compete for a certain agent that both of them need. So if one cell eats it away from another, the other gets less.

Yet, in the case of the human population it is such that we actually support one another continuously. The farmers plant grain; from the grain the baker can bake bread; with the bread he feeds other people; they look after the electricity, lay the wires going to the farmers. "Can the equations that we developed be applied to a population where the members support rather than fight each other?" We wrote new equations and then came to the conclusion that human society is not stable; that is, it doesn't strive for a certain state of equilibrium, as do for instance rabbits and foxes: The foxes eat the rabbits, until there are no more rabbits. Then the foxes disappear, because they starve to death. When the foxes become less, the rabbits become merry again; then again there are many more rabbits than foxes. And again the foxes break loose and eat the rabbits. So between these two species there is an oscillation: Many rabbits at one time, many foxes at another.

It is not like that with human society. The amount of people rises and rises and rises. We didn't think it possible; and I asked my students: "Please find out in the literature what the demographic estimates are for the number of people living in the various centuries. Go as far back as you can. Find out how many people there were at the time of Jesus, the time of the pharaohs, the time of the medieval crusades, et cetera!"

In a matter of a few months we had found hundreds of such data, and then applied our curve, our equations to these results. To our astonishment, every population estimate of the early societies fitted the curve of our equation exactly. "That is uncanny," we said. "That is unbelievable! We've got to publish this theory, which so correctly captures all the data available. Before that, however, let's still compare what the demographers of the United Nations say about the future of human society and what *we* say about the future of human society."

There was an incredible difference! The demographers of the United Nations said, "Next year there will be so and so many people." We said almost twice as many: "So our theory unfortunately is not correct, for surely the demographers of the United Nations know exactly what is happening." So we wait for half a year, and look at the results of the population counts in the world. Who's right? Our prediction was exactly right, while the United Nations' prediction was totally wrong. Thereupon we said, "Now we have to publish." And so we published in *Science*, the standard scientific magazine in America. The article is entitled "Doomsday: Friday, 13 November, A.D. 2026." That is the day on which human society becomes infinitely big. That is to say, at that point the system will be unstable, collapses, breaks down, and becomes something different.

The article appeared, and the demographers cried out loudly: "That is a scandal! They are maniacs! They are insane! And that a scientific journal like *Science* would publish such a thing!" This paper became so popular that all of the American newspapers and magazines adopted it: the *New York Times*, the *New Yorker*, *Time,* the *Herald Tribune, Newsweek,* et cetera, et cetera; everyone reported on it.

After a few weeks it jumped over to Europe. There, all the big newspapers and magazines of Europe—the *Kölner Illustrierte*, the Viennese *Neue Freie Presse*, the *Frankfurter Allgemeine Zeitung*, et cetera, et cetera—published this report. The Berlin newspapers ran it; then the Russians, the Japanese, et cetera, et cetera. So that in the course of a year, this doomsday story of November 13, 2026, had made it around the entire world.

Doomsday—we took it as a joke. You also know that November 13th is my birthday. By the way, the article was entitled "Doomsday: Friday 13 November, A. D. 2026 plus/minus 4 years," for one can't predict it that exactly.

"Pogo"

Many comics also adopted it. For example, there was a cartoonist, whose cartoons were very popular in America. He had a little strip, with a little animal named Pogo. Pogo always made fun of all sorts of things happening in the world. And one day there appears a Pogo who busies himself with this population mathematics. Pogo unpacks his sandwich, which was wrapped in a newspaper. He looks at it and says to his friend: "It says here that on November 13th, 2026, will be doomsday. On that day human society will collapse." And his friend says, "That's terrible! Before or after lunch?"

The "Pogo" cartoon by Walt Kelly

This Pogo story appeared in Chicago. Then the students at the University of Illinois printed this Pogo strip in their student newspaper the *Daily Illini*, so that the whole university knew: "Heinz von Foerster appears in Pogo with his 'Doomsday, November 13th.' That is a very big honor."

A few days later the people of the Office of Naval Research suddenly came to me and said: "Dear Heinz von Foerster. We have sad news. Unfortunately we cannot extend your contract. But you are not the only one to suffer. We even have to cut the contract of John Bardeen, the Nobel Prize winner." I said, "Well, Bardeen got the Nobel Prize, but I appeared in Pogo." Then they said, "Yes, we know you were in Pogo, but you were in Pogo with a contract from the National Institutes of Health, and not from the Research Department of the Office of Naval Research. That's why we cannot continue to support you." I thought that was great: that to have been in Pogo was as good as to have gotten a Nobel Prize, that they actually took it seriously and defended themselves.

End of the BCL

Thirty or forty percent of the money for the BCL came from the money that I brought in from outside for research assignments. When, for example, the National Institutes of Health gave me one hundred thousand dollars for research, the university got forty thousand. So if I add up how much I brought to the university in the twenty years of the BCL, it is ten million dollars. That is why they always held on to me, because I was a good milking cow. You could always extract money from Heinz von Foerster; and not just a little, but millions. That, of course, convinced many people: "Let's not kick Foerster out, let's let Foerster work here! It is true that it is a crazy school—we haven't got a clue as to what they do there; the worst of our students flourish and thrive there; they must be lunatics—but let's let them do their thing, since we get five hundred thousand dollars from them every year."

What we pursued at the BCL at that time, the heavy leanings towards the cognitive realms—What is cognition?—and then towards the social realms, especially our interest in natural language and in the interaction with computers, really captivated us.

It seems to me that my sponsors, the people of the Office of Naval Research, did not consider any attempts at communicating with computers in natural language to be of importance.

The problem of language is an incredibly difficult problem, which we called "computing in the semantic domain." They are computations of an entirely different dimension than the ones you can do with normal von Neumann computer architectures.

They are very interesting and incredibly fascinating computation strategies, and my feeling was that my sponsors had already noticed that this program was of a depth simply not justifiable to their superiors. And so they distanced themselves more and more from our interests; and when this population project was added, which was actually paid for by the National Institutes of Health, the Navy people said, “Foerster is being maintained by others anyway; we no longer have to support him.”

Now the Office of Naval Research supported exactly those topics that were really close to our hearts, namely, exactly this march into epistemological problems, into cognition and language; but conversing with machines in natural language, semantic structures, et cetera, et cetera, was too deep and too wide for them, and so one day they said, “Dear Heinz von Foerster, we’re afraid that we will no longer be able to support you.”

A great part of the support for the BCL fell away with that. And you have to consider that there were so many young students there; then Ross Ashby, Maturana, and Löfgren. All of that is not cheap.

I could no longer raise these kinds of funds.

I thought: “The most important thing is that all doctoral students working with me be able to finish their doctoral work.” And so there were many dissertations written and examined during the last years of the BCL, and all of these very young people then flew out into the world and got very nice positions everywhere.

Ross Ashby did not feel well healthwise and said, “Unfortunately I can no longer work with you. I am returning to Europe.” Gotthard Günther was invited by the University of Hamburg. That was an action program of the German universities, who said, “We want to invite these great human beings, who under Hitler all had to emigrate to foreign countries and find jobs there, back to Germany; and if they return, they will get honorary professorships until the end of their lives.” Gotthard Günther accepted with pleasure, since he didn't feel that comfortable in America. And so he resigned, went back to Hamburg with his wife Mieke, accepted the honorary professorship from the University of Hamburg, and taught his logic there during the critical years of the student revolution. He did not let himself be led astray by the revolutionaries; and for that reason they liked him very much. Günther's lectures were always well attended, even when the universities were shut down. Maturana was back in Chile and the money slowly ran out.

And I knew that I had to be retired, and said to myself: "If now, at sixty-five, I take my pension, I am still a vigorous man and can start all sorts of new things." And so around my sixty-fifth birthday I presented my request to be pensioned. Then Mai and I considered moving to California, for our son Andreas, an architect, was there. "If we succeeded in finding a small piece of property there, Andreas could build us a house; then we would have a family connection."

Recently I read an article dealing with the history of neural networks. I was endlessly amused. The author writes about the work on neural networks and mentions a hundred researchers; but the networks at the Biological Computer Lab are not mentioned at all.

It is interesting, how "cliqued," that is, split up into small groups, all of these scientists are. A German, Günter Küppers, once drew a relationship network that captures the individual researchers, who in different places worldwide work on the same problems. There are a lot of little groups, and very thin lines lead back and forth between the groups. And the Biological Computer Lab sits there as if in a vacuum.

I believe that that was my fault. I believe I understood too little about the politics of science. I was too naïve. I thought: "Now we'll work." And: "That's going to be very interesting." And: "Those are fascinating problems." I did not think: "How does one sell that? What does one have to do, so that it reaches the public, so that it gets into the newspapers, so that the institutes that give the money find out about it?" I totally failed in public relations. Because I simply didn't take it seriously enough. I was so happy about the work we did; we identified so much with our work that somehow the selling of this work simply didn't occur to me. That is, I simply wasn't with it. And the others understood that only too well: First the propaganda, then the results.

George Spencer Brown and the "Om" Conference

When I was still at the university at the Biological Computer Lab, Stewart Brand, who published this interesting magazine, the *Whole Earth Catalog,* sent me a book with a letter and said, "I sent this book to Gregory Bateson and asked him to write a book review for us. Gregory Bateson sent it to John Lilly and asked him to write a book review. Both said, 'That is a very interesting book,' but they didn't know how to deal with it. 'The only one who can write something for you on that is most probably Heinz von Foerster,' and with that request I am sending you the book."

The title of the book was *Laws of Form,* written by an English logician, George Spencer Brown. Accidentally I knew something about George Spencer Brown, from Gordon Pask. They knew each other very well. George Spencer Brown lived not far from Gordon Pask, who lived in Richmond Surrey.

And Pask had once told me about a crazy friend of his who, although his name was *George* Spencer Brown, usually, when he visited Pask, called himself *James* Spencer Brown, and who liked to go for a walk in Richmond's parks with Pask's small daughters; his appearance being a little like that of the man who wrote *Alice in Wonderland*.

I read the book with great pleasure in the night from Friday to Saturday. On the following morning Mai asks me: "Did Stewart Brand send you a book of jokes? I heard you laughing uninterruptedly." I say, "I laughed, because he makes such funny logical somersaults, that I am totally enchanted and delighted."

So on Friday I got the book, on Saturday I read it; Saturday and Sunday I wrote my book review and on Monday I sent it to Stewart Brand for his *Whole Earth Catalog.* On Wednesday it was published.