From Vienna to California:
A journey across disciplines

An interview with Heinz von Foerster[*]

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In 1994, while working on a collection of articles on the interaction between research in Artificial Intelligence and Cybernetics and the Humanities, Stefano Franchi and Güven Güzeldere visited Heinz von Foerster in Pescadero, California, the small town on the Pacific coast about 40 miles south of San Francisco he had settled in after retiring from the University of Illinois. There were several reasons that prompted us to seek a meeting with Heinz. We both had a long-standing interest in the history of mechanized intelligence, and we shared the conviction that cybernetics, in all its different varieties, had not been given the credit it deserved by the standard accounts. We also shared the conviction that one of the most interesting aspects of Heinz von Foerster's work was his willingness to forgo disciplinary boundaries and let his research dictate the boundaries of his discipline. We felt that his radical attitude toward the vexed issue of interdisciplinarity could provide a useful perspective in a period of acknowledged stasis in Artificial Intelligence research, and we were eager to listen to a first-hand account of its benefits and perils. And last but not least, we thought that it would be improper to defy the goddess of Fortune by ignoring the opportunity she was giving us: at the time we were both at Stanford, a mere 40 min drive from the residence of a scientist who had proved so important in the development of our field of studies and whose work had been a determinant factor in Stefano's decision to leave his home country and continue his studies in the US. Our questions were shaped by these interests, and they mostly focus on the theoretical and institutional history of Cybernetics and its often tumultuous relationship with AI. We cannot underestimate how much we learned about history and scientific methodology from that afternoon with Heinz in Pescadero and from the ensuing exchanges.

The interview was conducted by Stefano Franchi and Güven Güzeldere. Eric Minch transcribed it from the original tapes and provided editorial assistance. The authors would like to thank the Stanford Humanities Center for the permission to publish it in this special issue of *Kybernetes*.

Stefano Franchi and Güven Güzeldere (FG): One of our research goals is to promote a multidisciplinary dialogue on Artificial Intelligence and the Humanities. We think you are most qualified to facilitate such a dialogue since you have trotted along many disciplinary paths in

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your career, ranging from mathematics and physics to biophysics and hematology, to pioneering work on cybernetics, to philosophy, and even family therapy. One could even say that "transdisciplinarity" has been your expertise...

Heinz von Foerster (HvF): I don't know where my expertise is; my expertise is no disciplines. I would recommend to drop disciplinarity wherever one can. Disciplines are an outgrowth of academia. In academia you appoint somebody and then in order to give him a name he must be a historian, a physicist, a chemist, a biologist, a biophysicist; he has to have a name. Here is a human being: Joe Smith – he suddenly has a label around the neck: biophysicist. Now he has to live up to that label and push away everything that is not biophysics; otherwise people will doubt that he is a biophysicist. If he's talking to somebody about astronomy, they will say "I don't know, you are not talking about your area of competence, you're talking about astronomy, and there is the department of astronomy, those are the people over there", and things of that sort. Disciplines are an after-effect of the institutional situation.

My personal history has been different. If somebody asks me "Heinz, how is it that although you studied physics, mathematics, and all that, you are always with artists and musicians, etc.?" I think it is because I grew up in Vienna, at a fascinating time of Viennese history. I was born in 1911, looking back over almost the whole 20th century, with just 11 percent missing at the beginning and a 6 percent missing at the end. So I had the pleasure of traveling through the 20th century from its early stages. At that time – in the late 19th century – Vienna was an extraordinary place. It had a remarkable medical faculty, fascinating philosophers, it had great art (a new artistic revolution was taking place in Vienna under the name "Jugendstil" (or Art Nouveau, as it was then known in all of Europe); fascinating painters, an explosion of artistic activity, music, Mahler, dance, etc. In all fields there was a breakaway from the classic and from the standards of the 19th century perspective. I had the luck to be born into a family which was participating in all that activity. As a little boy I was already associated with them: I was sitting under the piano and listening while the grownups were talking with each other. It was always fascinating to listen to what they were saying. My mother's and my father's house was a very open house, but the really open house was the house of my maternal grandmother. She happened to be one of the early and leading feminists who published the first women's journal in all Europe – *Documents of Women*. Politicians, writers, journalists, theater people, etc. were in my grandmother's house. We, as kids, were of course always looking and listening; we were immersed in a world that had no specifics, no disciplines. I mean, everybody was connected and arguing about what art, politics, philosophy, should be. Growing up in such a world brings you into a state of affairs where you have difficulties looking at single disciplines. I see practically every sentence we are speaking as already like a millipede connected with 500 – oh but a millipede, so a thousand – other notions. And to talk about a single notion has a built-in sterility which does not allow you to make the semantic connections to all those other concepts.

[FG] What kind of education did you receive in school, after you were too old to remain under the piano listening to grownups?

[HvF] Later, there were two phases in my education. Somehow by nature, mathematics was no problem, physics was no problem. I was a very good student in mathematics, but I was not acknowledged. In the Austrian school system of that time, you went into the classical Gymnasium, which taught you Latin and Greek and history. Mathematics and physics were peripheral. I was a very lousy student in Greek and in Latin. I was always threatened with flunking this and flunking that. My poor parents suffered with this boy who would not learn anything. But in mathematics and physics I was very good. However, you could not get good grade in mathematics and physics if you had very bad grades in Latin. So, I was over the

fringe, so to speak. Nevertheless, I became very interested in philosophical issues, and mathematical issues as well. And when I was through with high school I went to the Institute of Technology in Vienna to study technical physics. This was a new field, and I enjoyed it very much. Very early in my curriculum, perhaps during the first semester, one of my student colleagues came to me and said: "Heinz, I was just coming from the University and there are a series of lectures that are so fascinating that you should come along. You absolutely have to come along with me". So I said "Good, let's go and see", and the next lecture to which I went was by a professor by the name of Scheminsky, and the theme was "Is it possible to generate life artificially?" He was lecturing about whether it was possible to make AL, artificial life. Remember, this was in 1931. Of course, I thought it was fascinating to ask such a question, and I went to attend the lecture. The room was absolutely full, with lots of young people there – and all the big professors in the first row: physics, theology, mathematics, biology, etc. Then the chairman of the session announced: "Professor Scheminsky will talk about 'Is it possible to produce life artificially?'" The first row stood up in protest and walked out. How can you possibly produce life artificially? It is a scandal even to ask that question; it is blasphemy. Of course, for a eighteen- or nineteen-year-old boy this was the best propaganda effort one could possibly make, because we immediately thought "This is what we have to listen to". When the orthodoxy is walking out, it must be the doxy you would like to become interested in. So I went again and again to these lectures, and it turned out to be a lecture series generated by the Vienna Circle of philosophers: Wittgenstein, Carnap, Hahn, and so on. I was very early already absorbed by these thinkers. At that time I stumbled over some of the Wittgenstein writings and particularly the *Tractatus Logico-Philosophicus*, and I fell in love with it. I thought "This is it!" Of course, I was raised with classical philosophy, I knew my Schopenhauer, I knew my Kant, I knew my this and that. . .but suddenly here comes Wittgenstein . . . Wow! I almost knew the *Tractatus* by heart. I had one other friend who also knew the *Tractatus* almost by heart. This was a cousin of mine, a nephew of Wittgenstein, Paul Wittgenstein. We both tested each other whether we could rattle off some of the propositions. Paul used to ask me, "Heinz, what is 3.21?" and I had to rattle it out, and I'd ask him, "Paul, what is 5.12?" and he came with his rattling. Anyway, I became absorbed in the logic, in the mathematics, in the philosophy of the Vienna Circle. And here you see again, the possibilities are wide open; they reach into almost all sorts of thinking: how do we do it, how do we do things, how do we think about it, how can we describe our descriptions, how can we think about our thinking? Thus, very early in my life I was exposed to what I call today a second-order notion – the notion that speaks about notion, which I believe is one of the crucial concepts of second-order cybernetics and reflexive thinking in general. A logic which is associated with reflexive thinking or with a closed semantic system is a very different logic than the classical one, and I think it contains insights which can be applicable practically to anything – whether you take education or philosophy or mathematics or physics. Consider, for instance, what today people call chaos theory – of course, it is a wonderful, very fascinating name, but it is recursive function theory, which was developed already in late 19th century by Koch, Hilbert, and others.

This, then, is Heinz von Foerster's background as a human being: a very broad basis which is associated with dancing, with music, with philosophy, with mathematics, with physics, and with a particular touch of the thinking of the Vienna Circle people, including Wittgenstein and Carnap. It is with this package of things plus my rucksack and my sleeping bag that I more or less entered the United States in 1949.

[FG] Was there anyone who was particularly influential on your thinking in the Vienna Circle, besides Carnap and Ludwig Wittgenstein?

[HvF] I would say Wittgenstein was an essential. Then I went to the lectures of Schlick and the lectures of Carnap. Now I would say Wittgenstein was most influential, but Rudolf Carnap and his logic were also very important to me as well. I would also add Hans Hahn. Hahn was a leading mathematician at the time in the Vienna Circle, and already very early I was very much impressed by one of his lectures on recursive functions, where he discussed undifferentiable curves, which is the basis for the fractals – the fractal notion was already there in about 1905. At that time the problem was: "Can we construct curves which cannot be differentiated?" That means curves where you can't find the tangent because every point has infinite directions. Which is of course lots of fun, and if you are interested in this kind of mental juggling – which I was as a kid – you enjoy things which cannot be done.

[FG] *What prompted you to leave Vienna and come to the United States?*

[HvF] That's very easy to answer. When I left Vienna, it was still occupied by the Russians. I had returned to Vienna in 1945 and it was completely bombed out; it was a very very miserable time. I could not find an apartment to live in with my wife and three kids. I had two crazy jobs at that time in Vienna, just to earn some money for bread and cigarettes. One I found through relatives who owned a telephone company in Vienna associated with the Ericsson Company in Sweden. They manufactured telephone relays and the whole switching systems for telephones, and I was working there, as they were rebuilding the whole company – it was completely destroyed in the war. The first thing we did was to build one lathe out of some parts we could find. And then this lathe produced another lathe, and then we could start building things. This was one job. The other job was related to the American occupation forces – there were four, the Russians, the French, the English, and the Americans, and each one controlled a district in Vienna. The Americans saw in Vienna a fabulous opportunity to broadcast Western information to the East and therefore, they were interested in establishing a radio station. By a series of incidents, they invited me to participate in that. I was one of the very early members of that American radio station established in Vienna by the Information Service Branch. I was taking charge of the scientific and cultural department: I interviewed the scientists, the musicians, the conductors, the dancers, etc. With these two jobs I earned the bread for my family through my work with the telephone company and I could buy my black market cigarettes as a reporter with the American radio station Red White Red, named after the colors of the Austrian flag.

My wife had a very dear friend, whom she grew up with when they were both at the same high school. This woman, Ilse, had established herself in America; she was living in New York, married to a successful stockbroker. They said "Heinz, we will lend you the money for a trip to New York, and may be you can find a place where you could work in the United States". So she bought me a ticket on the Queen Mary and I sailed to New York.

[FG] *This was in 19. . .?*

[HvF] This was 1949. Just before my departure I had finished writing a little book which dealt with problems of memory. It was a quantum theory of memory. I brought that little book with me – as a business card, so to speak – when I came to the United States.

[FG] *Could you give us a micro-account of your early theory of memory?*

[HvF] As I told you I was a very bad student in history, a bad student in geography, a bad student in all of these things where you have to memorize data. So I said to myself "History is absolutely catastrophic. I can't remember whether Caesar came before Augustus or after Augustus or aacchhh". So I thought I would make myself a table, a historical table. When I made this table I observed that the closer to the present you came, the more densely filled the paper

was with data; conversely, the further you went back the thinner the table. As it went back to the 15th century there was almost nothing, a century or more with nobody to mention; then you go to Rome, of course there are a couple of wars, which you know. And then you go: 300 BC, 400 BC, empty. 500 BC you get one entry or something. This is a bad way of presenting things. The best way to represent such data is to shrink the extension of the graph the longer you go away, and the only method to do that – or one elegant method to do that – is to plot it logarithmically. Every decade, or every power of ten, covers the same distance and that means that, as you go further back into the past, 10 years are as big as 1 year, and then 100 years are as big as 10 year, etc. Thousand years the same, ten thousand the same, hundred thousand, they're all the same size. With this kind of representation you get the same density everywhere. You can go back, and you have not only Caesar's assassination, you also have the extinction of the dinosaurs, and you have not only the extinction of the dinosaurs, you have the beginning of life, and a little bit further down you have the beginning of the solar system, and one step further you have the beginning of the universe. The whole thing fits in one table if you use a logarithmic time scale.

I was very proud of that table (Figure 1), and I thought it was a wonderful thing because it is so simple to plot, and everything has a place, and there is no waste of paper. Later on I thought "Aha, here is a funny thing. If the past is logarithmically compressed, may be our memory is working in a similar way". That means that things which are very far back disappear slowly, and only the most recent things are remembered. I was very proud – what a beautiful theory – but I had no way to prove it.
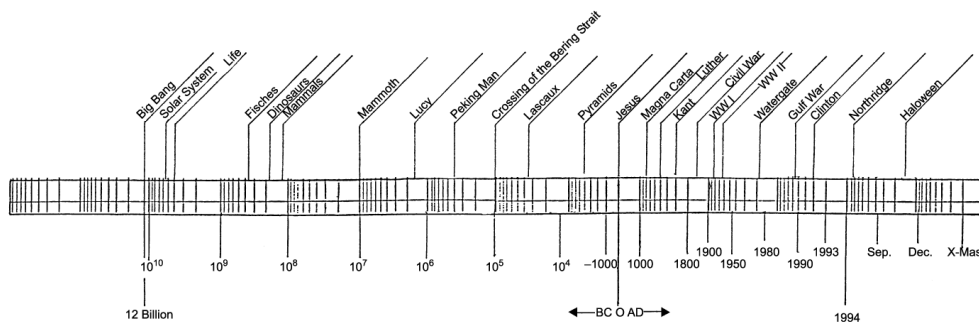

Notes: $T = \log(t_0 - t)$, where T = representational time; t_0 = asymptotic moment; and t = historical time. Asymptotic moment (Now!): t_0 = midnight, December 31, 1994

Figure 1. Table of historical events drawn on logarithmic paper

I was back in Vienna after the war, and of course one always browses through bookstores, and here was a bookstore with old second-hand books sitting out in big boxes in the open air. I pulled out a book that was called, I think, *Introduction to Psychology*, I opened it in one place, and there was a graph titled "Ebbinghaus's Forgetting Curve". I thought "Wow, there is somebody who actually measured these things". I began to read it and I learnt that Mr Ebbinghaus, at the turn of the century, measured the forgetting speed of people who tried to remember something. I bought that book – it was probably 25 or maybe 50 cents – took it home, studied the curve, and said to myself that I should analyze the data to see whether the exponential decay is indeed the way in which our memory works. The method by which Ebbinghaus measured forgetting was as follows: he gave to some students a sequence of nonsense syllables to learn, perhaps fifty, like "foh", "ping", "kah", "pu", etc. And he would say: "You learn it to-

day, and then you test yourself and see how many you remember; you now remember fifty, OK, then I will question you every two or three days to see how many of these syllables you retain". He collected the data from the students, and drew a curve to represent these data. This was Ebbinghaus's Forgetting Curve (Figure 2), a very famous result in the history of psychology.

So I immediately set myself to analyze it, to see if it would fit my exponential law of memory decay. I wanted to see if my mathematical description of forgetting would fit Ebbinghaus empirical curve, but whatever I tried did not work. Well, I thought, my beautiful exponential decay notion of memory goes down the Danube to the Black Sea.
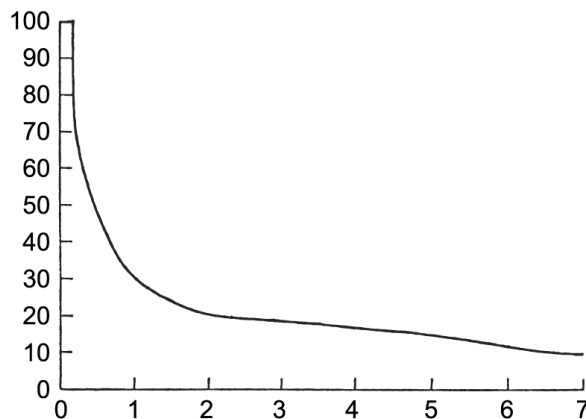
Notes: Reprinted from the "25 cents" book, e.g., Hubert Rohracher, *Einführung in die Psychologie*, Wien: Urban & Schwarzenberg, second edition (1947, p. 247)

Figure 2. Forgetting curve. On the horizontal axis the number of days, on the vertical axis the number of retained words, in percentage.

Then I began to study how Ebbinghaus did it, and realized that he did it exactly as I described it to you, by always asking the students how many syllables they remembered. But when you ask somebody what he remembers, you refresh their memory. It is, in a certain sense, like a feedback mechanism, where you re-establish the memory. "Aha", I said, "my differential equation for decay is incomplete. I have to bring in also the period where questions are being made, and how often a notion is being remembered, actively remembered". I put this second element into the equation and – incredible thing – I got an absolutely perfect match with Mr Ebbinghaus's curve. I could establish the two parameters – the forgetting parameter and learning parameter – and everything was fine. I started to look at some other papers, and the fascinating thing was that most of the forgetting follows a decay which is exactly the same as radioactive decay – perhaps half a day or something like that – and one remembers only by actively regurgitating what one still knows. Look outside.

[A deer appears suddenly in the backyard garden of the house and looks intensively at us as we speak. Heinz von Foerster cannot help admiring the animal but adds that they have become a real pest, since they eat whatever plant they have been trying to grow. Nonetheless, the sheer elegance of the deer wandering amidst the bushes keeps us silent for a few seconds.]

Anyway, I did that kind of research and then noticed that some very interesting facts were coming out, namely that the decay constant for some macromolecules – biological molecules – is exactly the same. Thus, my argument was "Aha, there must be a link between the quantum mechanical interpretation of large biological molecules and our way of keeping things in our mind or forgetting it".

I wrote that up in a little booklet *The Memory – a Quantum Mechanical Treatise*, and it had some also interesting history. There was a man, an absolutely fabulous man, Viktor Frankl – a psychiatrist who survived the concentration camp. He was captured very early when the Nazis came to Vienna, and he was brought into Belsen into one of those destruction camps, but survived it, remarkably. Then he came back to Vienna, and through my radio job, I got to know him very well. I invited him again and again to speak, because his comments were very essential to make people understand that under tremendous stress you can still survive. I gave him my account of memory and he got fascinated by it and said it should be published. His publisher was a gentleman by the name of Deutieke. Frankl called his publisher, Mr Deutieke, and said "Here is a wonderful little story in about a hundred pages. I think you should print it. It is very important". Deutieke of course respected Frankl as a psychiatrist, but there was a lot of mathematics and physics in the manuscript, and Deutieke correctly insisted that he had to have another judgment by a physicist. Now, Erwin Schrödinger happened to be an author for Deutieke as well. So Deutieke sent my manuscript to Schrödinger, who was in Ireland at that time, and asked him of his opinion. It didn't take very long for Schrödinger to send back a note, saying "Dear Mr Deutieke, I don't believe a word of what this thing is saying, but I couldn't find any mistake". So Deutieke said: "I don't care what Schrödinger believes, I only want to know whether there is a mistake, and since there is no mistake I think we can print it". So he published the manuscript; I thought it was a very interesting judgment on a good man who is a publisher.

[FG] So this is the book you had with you when you crossed the Atlantic Ocean.

[HvF] Exactly. I brought a couple of copies of that book to the United States. Whenever I had a scientific friend in the United States I sent them that little booklet with a note and said: "I'm still alive, this is the nonsense I was doing while I was gone". One of the people to whom I sent it was a very dear friend of my youth: Maja Unna, the wife of a Professor of Pharmacology at the Medical School of the University of Illinois in Chicago. A couple of days later I got a telegram from her, which said "Heinz, come immediately to Chicago. I showed your book to a professor here and he really wants to discuss your memory theory with you". So I immediately took a night flight with Capitol Airlines leaving at one o'clock in the morning and arriving in Chicago at 8 a.m. It took 7 h to fly from New York to Chicago, and it was – I remember – 17 dollars, which was cheaper than traveling by bus. So I flew to Chicago, went to the Medical School, and met Maja. She introduced me to a very fascinating gentleman who happened to be Warren McCulloch. He was the head of the Department of Neuropsychiatry at the University of Illinois at Chicago. He wanted to know the details of my theory. My English was very poor, and his German was practically nil, but the fascinating thing was that we could understand each other, because we were talking about scientific method. No problem when you go to the equation: if you can't say something you just point to the numbers or to the expressions. It turned out that about 2 or 3 months before I came – it was February 1949 – there had been a large meeting of some big scientists in America about memory. And they all had lots of data, but no theory. The fascinating thing is that this little booklet of mine had numbers that were matching exactly the data they had. This was absolutely fascinating. I remember that Warren said: "This is too good to be true", and asked me to give a lecture on the spot. I replied I couldn't give a lecture because of my linguistic inability, but there were so many immigrants –

German and Austrian immigrants – at the University of Illinois at that time, that I just had to say it more or less in German and they translated it all very nicely. Then Warren invited me to give another talk. He said "In two weeks we have a conference in New York; since you're living in New York we invite you", and this was one of the now very famous and legendary Macy meetings.

[FG] Were these the conferences which Norbert Wiener, Gregory Bateson, Margaret Mead, etc. took part in?

[HvF] Yes, exactly. There was an organization, the Josiah Macy Jr Foundation, which sponsored meetings in a variety of medical fields – liver injuries, glaucoma – fundamental medical problems that did not have known solutions at the time. One of these conferences was entitled Circular Causality and Feedback Mechanisms in Biological and Social Systems, and Warren McCulloch was chairing it. The participants in that conference included Norbert Wiener, Gregory Bateson, Margaret Mead, John von Neumann, Julian Bigelow who, with Wiener and Rosenbluth, wrote the famous paper on Teleology (Bigelow *et al.* , 1943); Heinrich Kluever, who completed the great book on *The Vertebrate Visual System* (Polyak, 1943) by the late Stephen Polyak by adding a bibliography of about 10,000 (yes, ten-thousand) items; Claude Shannon from Bell Labs, the inventor of "Information Theory", and many others of that caliber, "la crème de la crème" of American scientists. And he asked me to present my memory story to that group. I was in New York and I went to that conference. I was not frightened by these people, because luckily I did not know anything about them. After I presented my story I left, since I was a guest, and after a while they called me in and said: "Heinz, the story you told us is a very fascinating one, but the way you presented it is abominable". And I replied: "Yes, I'm only starting to learn the language" they knew that, and therefore, they decided to appoint me as the editor of their transactions to improve my English. I thought I could not do that, but then I said to myself "No, if they appoint me why wouldn't I accept it?" So I said "OK, its wonderful, I accept that!" but I couldn't even pronounce the title of the conference.

Fortunately, Warren McCulloch, at my first meeting in Chicago had told me to read a book by Norbert Wiener, titled *Cybernetics*, before going to that conference. I had done that and I thought to myself: "Instead of calling that conference Circular Causality and Feedback Mechanisms in Biological and Social Systems, why not call it Cybernetics?" So I asked them: "Why don't you call your conference Cybernetics?" Everybody seemed to like the idea, so they applauded. Norbert Wiener was so touched by his colleagues' accepting the concept of cybernetics, he had tears in his eyes and he had to get up and walk out of the room to hide his embarrassment. With this new title accepted, I got the transcripts of ten or twenty speeches about 2 months later. I bought myself two dictionaries plus a medical dictionary, and I set off learning my way through all these papers. I think at the end we had a good result.

[FG] One group of people who were influential on your intellectual development was the Vienna Circle. This was in your youth. Could we regard the group that consisted of McCulloch, Wiener, von Neumann, Bateson and so forth, as a second intellectual circle, the "Cybernetic Circle", in your mature years?

[HvF] Yes! An excellent suggestion! This was fascinating. The papers coming out of the Cybernetic group were very much in the same direction as the Vienna Circle people approached problems. Think of the famous paper by Warren McCulloch, "A logical calculus of ideas immanent in nervous activity", for example. If you look at the whole thing, the formal machinery is Carnap's machinery which I grew up with, the same machinery which is at the basis of the *Principia Mathematica*. So I felt immediately at home with the "Cybernetic Circle". I love differential equations because they provide a connection between geometrical

thinking and algebraic thinking. Such formalisms are like bicycle riding or swimming to me, they come very naturally.

[FG] Was your involvement with the Macy group limited to the editorship of the Proceedings?

[HvF] I became very fascinated by the group, and they liked my editorial job. Further, I was very fond of the philosophical position which was presented by these people. Here comes an interesting point which might perhaps have a particular relevance to the connection between the humanities and the hard sciences. For the first issue I edited for the Macy foundation, I hadn't written a preface. It so happened that Margaret Mead, who was very supportive of me, and Hans Lukas Teuber, who was a German physiologist, decided to join me in the effort to write a preface to the second volume. I wrote a preface, and sent it to Margaret and to Hans Lukas, but somehow they didn't like it. They thought I was too philosophical, too abstract; the preface should have been more down-to-earth so that people would understand what cybernetics was all about. For me cybernetics was a fascinating notion, because it introduced for the first time – and not only by saying it, but also methodologically – the notion of circularity, of circular causal systems. And I thought that from an epistemological point of view that was very important. So I stressed in my preface the epistemological wit of the notions of circular causality, circular operations, closure, closed system, and so forth. But this was felt to be too abstract for the pragmatic mind of American thinkers. They replied "Heinz, we love your wonderful somersaults and epistemological skills, but we would like to have it down-to-earth". But I said "Well, I'm not a down-to-earth man!" Actually, I am a down-to-earth man, but not in that field. Eventually, Margaret and Hans Lukas wrote another preface which I, in retrospect, find – with apologies – very pedestrian. It presented too narrow a notion of cybernetics, as if it were based only on the notion of feedback. Of course, you can have control theory built on the notion of feedback; control theory was already well developed then. It was a section in electrical engineering, where you learned control theory together with a thorough and consistent mathematical theory. I thought that wasn't the fascination of cybernetics – you can have that in dry control theory. Cybernetics considers systems with some kind of closure, systems that acts on themselves – something which, from a logical point of view, always leads to paradoxes since you encounter the phenomena of self-reference. I thought that cybernetics was trying to bring into view a crucial point in logical theory – a point that is traditionally avoided by logic; one which Russell tried to eliminate by bringing in the theory of types. I thought of the theory of types as a miserable excuse for someone who doesn't want to take on the responsibility of saying "I'm saying that", because you are not supposed to say "I" with the theory of types. While working on these prefaces I got the feeling that some of the essential force of Wiener's arguments were somewhat diluted. And my hunch is that it was perhaps because of this perception, because of this dilution of the notion of cybernetics that artificial intelligence was separating itself from a cybernetics which had become nothing but control theory applied to living organisms. Cybernetics, for me, is the point where you can overcome Russell's theory of types by taking a proper approach to the notions of paradox, self-reference, etc. something that transfers the whole notion of ontology – how things are – to an ontogenesis – how things become.

It is the dynamics of cybernetics that overcomes the paradox. The paradox produces a "yes" when it says a "no", and then produces a "no" from a "yes". It's always a production. In cybernetics you learn that paradox is not bad for you, but it is good for you, if you take the dynamics of the paradox seriously.

[FG] Indeed, it seems that classical artificial intelligence researchers tend to refrain from getting into such paradoxical matters. Seymour Papert once said of Perceptrons – the very influential book he wrote with Marvin Minsky – that if their work was so critical of cybernetics

and neural nets research it was because there was too much romanticism and too little mathe-matical rigor in the field. Do you think he was trying to separate AI research from what he perceived as an overly "philosophical" pursuit?

[HvF] I think you touch an important point here; however, I would put it differently. It seems to me that there are different styles of thinking which, in Warren McCulloch, were still the two sides of the same coin. Let me cite the titles of some of what I would call his "meta-physical" papers, for instance, "What Is A Number That A Man May Know It, And A Man That He May Know A Number?" or "A Logical Calculus Of The Ideas Immanent In Nervous Activity" or "Through The Den Of A Metaphysician"; and then the titles of some embryonic AI papers: "Machines That Think And Want", "Toward Some Circuitry Of Ethical Robots", "Biological Computers", "The Brain As A Computing Machine", etc. (McCulloch, 1965). As you probably guessed, I was touched by Warren's metaphysics, while others were stimulated by his computer metaphors or, as von Neumann put it, by "Warren's Turing side", with refer-ence to Alan Turing's invention of the machine that can compute all computable numbers. There were, of course, other separating forces that pulled these two styles apart. One of which, I sense, may have been the prevailing funding strategies. If I remember correctly, lucky Marvin Minsky succeeded in getting a 10 million dollar grant for an Artificial Intelligence lab as a by-product of MIT operations. There was almost no other money available for any other studies which were not AI-ish, afterwards.

[FG] What year was that, do you remember?

[HvF] 1962, 1963 something like that.

[FG] So, there was actually a kind of funding war?

[HvF] A tremendous funding war, yes. And that happened because although science is a wonderful thing, there is a public relations side to it. Whether or not you address yourself to it, public relations are a very important component of scientific activity, and I must confess that as a greenhorn in America I did not realize that. I was so happy being allowed to do some re-search which I felt was interesting and important to the people sponsoring it that I thought it was a paradise. I thought you didn't have to do the politics, and I was so glad to have shed po-litical concerns. It was only much later that I realized that politics was there, all along.

Anyway, I think there was a bifurcation of these two fields, Cybernetics and Artificial In-telligence, which turned out to be detrimental. Cybernetics was unfortunately interpreted in too narrow a way, and I sometimes get the feeling that this was on purpose, to push it away from another big branch which became Artificial Intelligence. And Artificial Intelligence was prom-ising so much, at the time; it was absolutely incredible. And if you promise something, then of course everybody hopes that this promise will be realizable.

[FG] It was approximately in those years, in the early 1960s that you founded the Biologi-cal Computer Lab at Urbana-Champaign. What Department was it part of?

[HvF] It was in the Departments of Electrical Engineering and of Biophysics. Biophysics. You may remember that the University of Illinois was a leading university in computing; they developed very big machines. The Iliac I, Iliac II, Iliac III, Iliac IV, etc. They really had a tradi-tion of big machines, there. Through the influence of von Neumann and Wiener and others I was of course drawn into the Department of Computer Science. I had two positions: in the De-partment of Electrical Engineering and in the Department of Computer Science. Then, when I became associated more and more with the Macy Group, I thought that the university needed a Department of Biophysics. My background was in physics, and I thought biophysics was way too much neglected. So with some friends in the Department of Physics and in the Department

of Physiology, we wrote a program for establishing a new department. This was about at the same time I founded my Biological Computer Lab. I am now a Professor Emeritus from the two departments of (now joined) Electrical Engineering and Computer Science and Biophysics and Physiology. Early on, I saw that electron tubes will not be the future of electronics know-how, so I planned a transition of the whole Electron Tube Research Lab, which I was directing, to solid state electronics. I invited three gentlemen to take over my part and lead the lab during the transition. I then said to myself, "Now I have to study biology, because I would like to start a new lab". I took advantage of the fabulous institution of sabbatical leave, spent 1 1/2 year with McCulloch at MIT, he was at MIT at that time, another half in Mexico City.

I discussed how to study physiology with Norbert Wiener and he recommended to me to go to his friend, Arturo Rosenblueth in Mexico. So our whole family went to Mexico for a half year, and I studied under Arturo. I learned a lot from him; he was really absolutely fabulous. Anyway, I came back after a year of indoctrination in biology, physiology, and I was ready to start the "Biological Computer Lab". Fortunately, all the people from the Macy group were very supportive. This was in 1958.

I was extremely lucky, because very early I got outstanding people to join me – logicians, mathematicians, and physiologists. Maturana was a very early participant, and so was Gordon Pask. The lab was rolling out papers and papers and papers. I think this was very impressive for the sponsors and also kept us very inspired.

[FG] How did Maturana get to hear about BCL and join you?

[HvF] I was invited to a meeting in Leyden, a beautiful city in Holland. Ralph Gerard, who was a member of the Macy meeting, had arranged for a conference on Information Processing in the Nervous System. People from all over the world were invited. So I went, and I was very happy to go there for two reasons. Number one, I was very glad to be in Europe again, and number two, they had this conference on Monday and Tuesday, but they had invited us to be there on Thursday evening, so you had Friday and Saturday to enjoy Leyden and the beautiful beaches of the Eastern Sea and I wanted to go to Amsterdam, and see the Rijksmuseum. The morning after I arrived there, Ralph arranged for a general meeting of all the people, and he said "Ladies and gentlemen, we are here to have a dress rehearsal of our meeting on Monday and Tuesday. On Friday and Saturday everybody will present their papers, and there will be discussions and corrections on these papers. On Monday and Tuesday we will present the corrected papers". As I was listening to that, I thought that this was the most idiotic proposal I had ever heard. I mean, that's what a conference is for; you don't need a dress rehearsal. When Ralph was talking to other people, I sneaked out of the long room through one of the many side doors. And as I was doing this I saw another gentleman doing the same – sneaking out from another door. So I walked to him and said: "Are you taking part in that nonsense, participating in the rehearsal on Friday and Saturday, or would you like to go with me to the Rijksmuseum, to the Mauritshuis, and enjoy Amsterdam?" He said he would prefer to come with me. I said "OK, my name is Heinz von Foerster". He said "My name is Umberto Maturana". So this is how we met and he got to know about the Biological Computer Lab. We were from the beginning the black sheep of that whole conference. When we came back on Monday, nobody was talking with us. It was extremely funny. Afterwards, I invited Maturana to come to the University of Illinois.

[FG] Was he in Chile at the time?

[HvF] Yes, he was. He came to Illinois for a year, and then came again for another year, and then when I had a second sabbatical 7 years later, I went to Chile and worked with him on

some of the theoretical problems in neurophysiology, and of course, learned about what he was doing.

[FG] So BCL was in existence from 1958 until you retired?

[HvF] Yes, until 1975, until I retired.

[Stefano Franchi] BCL was one the reasons behind my decision to go to the University of Illinois as a graduate student in philosophy. Of course, I knew that you had retired in 1975, which was more or less 10 years before I applied, but I thought I would at least find some students of von Foerster's left there working in the Lab. Instead, when I got there, I couldn't find one person who knew of BCL. Instead I found this glass cathedral in the cornfield desert – the Beckman Institute. I was left wondering what happened after you retired.

[HvF] I knew I had to retire – because of compulsory retirement at 65 – and I knew that my whole program would have difficulty being sponsored by the usual sponsors after that. It was an expensive lab – I mean had I brought in 10 or 12 million dollars to the state of Illinois. There were 35 people, and they were expensive people. For instance take Lars Lo¨fgren, a Swedish logician who wrote some fabulous papers. I knew Lars from other meetings, and when I invited him to join us, he said "Heinz, you cannot afford me. The Swedish tax system is built in such a way that one third of my salary will be immediately subtracted when I am working in the United States. So in order to be able to live as a Professor, you have to add thirty-three percent to my salary". So I remember when I proposed to have Lars Lo¨fgren coming in, the chairman of the Department of Electrical Engineering said "You are paying this man more than I'm earning!" I said "That is correct, but I bring the money in from Washington, so why do you object?" So they said OK, and Lars could come. BCL was a very expensive lab, and I always found some considerable amount of funds to run this machine. There were 15 or 20 students who pursued their doctoral dissertations and they were paid for doing that. I mean, it was absolutely unique. That's why students loved to come to BCL. They had very strict and very good advisors, like Umberto Maturana, Lars Lo¨fgren, Gotthard Guenther, Ross Ashby. . .do you know Gotthard Guenther?

[FG] Is he a logician?

[HvF] He was fabulous man, a man who developed a kind of logic which looks, at first glance, as if it were a multivalued logic, but it isn't. Well, it is, and it is not. He called it a place-valued logic, and it is, I think, a very important contribution. Because it gets you out of that yes-no traps, the true-false trap. The essential point in Guenther's contribution is that he argues – correctly, with a very good formalism – that in order to take a proposition to be true or false, you have to have a place in which the proposition stands. That means, when you say "The sun is shining" you have to have a place where that proposition is to be put in, and only then you can say it is true or false. Furthermore, "The sun is shining" could be rejected as a proposition to be considered. That means, he introduced the notion of rejection instead of Boolean "true and false" and by that one has the means to consider a proposition as a whole as being acceptable or not. A couple of years ago, the whole collected works of Gotthard Guenther came out, if you'd like to look at it (Guenther, 1976). You should really know about Gotthard's work. It is very well understood and read in German, but very little in the American and English-speaking domain.

Anyway, when I saw that I had to retire I was very interested that all the students who were working at that time would have completed their theses. All the students eventually went to different directions. There is only one man who was with the group – Ricardo Uribe, one of the co-authors of the famous paper "Autopoiesis: The Organization of Living Systems, its

Characterization and a Model" with Humberto Maturana and Francisco Varela (Maturana et al., 1974) – who is still with the University of Illinois. He did a very nice job of preparing the experimental programs for the Department of Electrical Engineering and Computer Science, and he's doing very well there. So Ricardo is the only remnant of BCL at the University of Illinois.

[FG] Let's return to the previous point, to the relation between AI and Cybernetics. You hinted earlier that after Marvin Minsky received his huge grant for the establishment of an artificial intelligence lab at MIT there was little money left for anything else. Since Perceptrons came out in 1967, I wonder what it was like to work in cybernetics in the late 1960s and early 1970s, when artificial intelligence was on the rise with its promises of delivering big results very fast.

[HvF] In my case, it was quite interesting. Maybe there are two points which may add interest to your story. The first is that at the University of Illinois there was a remarkable man – Henry Quastler, a Viennese. Henry Quastler had to leave Vienna because of his Jewish origin when the Nazis entered Austria. Later, he established himself as a physician in the United States. Coincidentally, he became a physician at a major clinic in Urbana, close to the University of Illinois. When Hiroshima took place, and so many people were destroyed by radiation, he as a physician said, "It's my responsibility to find out what the causes for destruction in radiation are. So I would like to pursue studies of the destruction of biological tissue through radiation". He started with a small lab at the Clinic, and soon the Department of Physics at the University of Illinois had heard of Quastler's experiments and invited him to work for them. I knew Quastler from the first day we arrived there, and Quastler realized that information theory as it was developed at that time could be a very helpful mathematical tool to account for the effect of radiation on living tissue. Namely, you could work with a notion of "hit rate", or probability of losses, to make certain predictions. He learned the whole mathematics and philosophy of information theory very early and later I proposed that he should also come to the Macy meetings. Quastler was so successful in his work on radiation damage in living tissues that the Brookhaven National Lab invited him to go there. He arranged a very large conference on homeostasis there. Homeostasis was a very important topic. The question was "What are the conditions under which the system stays in or falls out of stability?" In other words, what were the stability conditions of complex systems? So he arranged this conference and invited me to give a very general overview of homeostatic problems. Now, at the very end of this conference – this was very early, 1960 or 1961 – a couple of gentlemen approached me and we had the following conversation:

Mr von Foerster, it's very interesting what you are saying here. You are the first mathematician who talks in such a way that biologists can understand.

I'm sorry, but I'm not a mathematician.

But it sounded as if you were a mathematician. Would you like to work with us at the Department of Hematology at the National Institute of Health?

Of course, I would love to, but I've absolutely no idea of hematology. I never looked in a microscope; I have absolutely no way of understanding your problems.

No, no, no, we will tell you all the hematological facts. You do the mathematics and we'll do the hematology.

Yes, but I can't do the mathematics if I don't understand the hematology.

Just come to us and we will teach you the hematology and then you teach us the mathematics.

Eventually, I came into very close contact with the hematology group, and we did some very successful work together. In fact, there is a paper of mine which deals with a partial differential equation accounting for cellular growth, and this has now become the von Foerster Equation in theoretical hematology. This is even taught in schools; I was very impressed. To return to the funding issue, we had a large contract with the Department of Hematology of the National Institute of Health, and there were some other contracts, for instance, by the National Science Foundation, and other grants from the Department of Education. The Department of Education was established in the 1960s, and I was one of the early grantees of that outfit, because we were very much interested in learning and its underlying psychological processes. So there were several granting systems which supported BCL. And BCL was a very hungry device, because I had responsibility for all the students and visiting scholars.

[FG] Did you ever enter in the close relationship with the Department of Defense, and especially DARPA, that was so characteristic of AI research?

[HvF] Yes, of course. I had no DARPA grants, but the Office of Naval Research supported us. It was my main support when I had the Electron Tube Lab, and since they knew Heinz von Foerster was doing decent work they decided to support me for doing decent work in biology. The early grants of BCL were all from the Office of Naval Research. Later on, the Air Force also participated in supporting it. They had the Information Service Branch, and they supported mostly the logic research. It's very interesting, they supported someone like Lars Lo¨fgren, whose very strange mathematical and logical systems practically nobody else understood.

[FG] So the rising dominance of AI didn't have as big an impact on your lab.

[HvF] No, we were just doing our thing. For example, we developed the first parallel computers. Our first, real first large parallel computing machine was the Numa-Rete. The Numa-Rete was a "RETina" that "saw" NUMbers. The earliest version was a square of 20 £20.400 photocells, whose "post-retinal network" was computing in parallel the number of "perturbations" – that is, objects obstructing its visual field: Five-ness, Ten-ness, 21-ness, or in general N-ness. The registering of this N-ness is of course independent of the shape of the objects, or their position on the sensory surface of the Numa-Rete and, above a certain threshold, also independent of the intensity of the illumination.

When John von Neumann visited us on some occasions, he enjoyed very much trying to tease the machine: placed a pretzel with its intricate topology on top, pressed the button, and the counter showed 1! Then he placed a nickel in each of the loops of the pretzels, pressed the button, and it showed correctly 3, etc.

This early machine had a limit of about 50 objects which it registered in a fraction of a second. Later versions accounted for about 400 entities in a few milliseconds (Weston, 1961).

[FG] We have another question with regard to the historical developments. Cybernetics precedes artificial intelligence as a movement, and one could perhaps expect that those two movements could have, in a way, merged or done something together, but that almost never happened. Somehow the rise of AI was independent of what was happening in cybernetics. and the two disciplines ignored one another or even fought against each other. Why do you think the forces were never joined?

[HvF] I think I understand your question, and it's a very important one. My feeling is that cybernetics introduced a way of thinking which is implicit in so many fields but it is not explicitly referred to as cybernetics. So the notion, the perspective, the way of handling a class of problems, came out of the works of cybernetic thinkers like Wiener, Ashby, Beer, etc. For instance, if you follow Stafford Beer's managerial contributions – which are clearly cybernetic –

nobody will call it cybernetics, but they understand it's a holistic kind of thinking, where you look at the relationship between elements. It brings about a way of looking at the relations instead of looking at separation. So an integrative form of thinking has been introduced by cybernetics. Ways of thinking that do not find explicit expression, but are implicit in the way in which people are doing things. So, from that point of view, I would say cybernetics melted, as a field, into many notions of people who are thinking and working in a variety of other fields. I would like to give you one more example. I have already mentioned that I was slipping into hematology from Cybernetics. This second example concerns Gregory Bateson. When I moved here to California, Gregory Bateson was already in the area, working at the Veterans Hospital in Menlo Park and at the Palo Alto Mental Research Institute, which he had founded with Don Jackson and Paul Watzlawick. I knew Gregory Bateson very well from the Macy conferences and when they were discussing problems about logic Gregory said "But look, I'm not a logician, I don't do all that. But there's Heinz von Foerster, and he just came to California. Why don't you ask him?"

So Paul Watzlawick came to me, and we had a very good discussion. He was fascinated by some of the notions which I brought in from the Vienna Circle, paradoxes and all that. He said "Heinz, we have a large group of family therapists and they would be delighted to hear about your perspectives". I said "Very good, I have no idea what family therapists are doing, but if you think my story would interest them, it would be nice". So I was invited to give a keynote address in a family therapy conference that was arranged by the Mental Research Institute in memoriam of Don Jackson. The title of my presentation was: "Paradoxes, Contradictions, Vicious Circles, and Other Creative Devices". The people liked it, and invited me again and again. They invited me without even my understanding why family therapists should invite me, until later on when they finally showed me some actual family therapeutic sessions, so that I could get a grasp of what they were doing. Then I was convinced that they could indeed profit from cybernetic thinking. So cybernetics is a way of looking at things, handling things, handling the language, handling the problematic which is developing in dysfunctional families. It appears in all these things, without explicitly being named.

[FG] Do you not think that artificial intelligence is similarly implicit in other fields?

[HvF] I do not think so. The founders and proponents of Artificial Intelligence were from the beginning very much motivated and extremely competent to go after highly specialized tasks as, for instance, how to build a robot which could rearrange an arrangement of blocks into another desired arrangement.

The performance of these machines are very impressive indeed, but I see them more as witnesses to the extraordinary natural intelligence of their designers, rather than cases of "artificial intelligence".

The anthropomorphization of these machine functions I see insofar as dangerous, because one may be tempted to believe that when we say "this machine 'thinks'" we know now how wethink, for we know how the machine "thinks".

Syntactically, however, the distinction is clear, for when the machines "thinks" they do it between quotes: quote think unquote. Except for the name there is nothing in common between the functions "think" and think!

[FG] This is somewhat reminiscent of some classical critiques of artificial intelligence, for example, Hubert Dreyfus's critique. It seems that you are saying something along Dreyfus's lines because you are saying that although artificial intelligence is claiming that they are working to solve the problem of intelligence at large, indeed they are working within a very narrow definition of cognition or intelligence, ignoring the larger background and context

within which cognition operates. And it seems that your view of cybernetics, or your own work, strives to look at cognition the opposite direction, in its largest possible framework.

[HvF] The way you put your question conjures up in my mind the image of the Roman god of the Beginnings, the Guardian of the Universe, the god Janus. He has a head with two faces that look in opposite directions. Now I see one face watching Aristotle's way of synthesizing imitations of life: "bio-mimesis"; the other face attending to those who follow the Platonic of coming to grips with, as Bateson put it, "mind and Nature, a Necessary Unity".

My sense is that we need to learn to look both ways, like the god Janus.

[FG] The rift you have described between artificial intelligence and cybernetics seems to be echoed in the newer rift between artificial life and the earlier work on Autopoiesis. Certainly if a lot of technically talented people set out to build innovative models, some good ones will result, but a lot of effort is wasted if these people are unaware of the philosophical context the last 40 years of scientific history have given us. What suggestions and advice do you have which might help move these two approaches back together, for their mutual benefit, particularly in this country?

[HvF] Well, I am more or less on the outside of these matters now, but because of that I may be able to see them from a more global point of view. I believe we have to look at the ways in which this research is funded. It used to be that the foundations and agencies with which one dealt were often administered by thoughtful men with fairly broad views and liberal arts backgrounds. You could talk to them, they would consider your views and make their decisions. If you could make a good case for it, they were willing to go out on a limb and take a risk in funding a new idea or project. These institutions have now become bureaucracies, and the typical bureaucrat's highest priority is to cover his ass. He says to himself "No more risks, just make sure you don't make any mistakes for which you could be blamed. Try to do what everybody else is doing". So the drift of science is much more influenced by trends and fashions. One sees this in the predominance of attention paid to the more outrageous claims made in fields like artificial intelligence and virtual reality.

[FG] But why is the situation different for European researchers? Participants in the American ALife conferences are overwhelmingly in favor of diving into the problem and building anything, programming anything, rather than spending time on considering the fundamental and epistemological problems of the field. The European ECAL conferences, on the other hand, appear to have a strong focus on the philosophical approach. And there's very little overlap in the constituencies of these two conference series.

[HvF] Again, it's due to the difference in funding strategies. European scholars, while perhaps not enjoying the most up-to-date facilities, are given the intellectual freedom of security in their positions, which allows them to focus on long term and fundamental issues. They can afford to spend years and years on a single problem. American researchers have a shorter funding cycle, their grants are predominantly for specific targeted research, and they live with the pressure and urgency of a need for practical results. Another difference is in the preparation of the students. Two equally brilliant students in a technical field, one from Europe and one from the US, will be expected to have very different types and levels of skill. One expects the European to know his field as well as some history, some philosophy, geography, literature, art, etc. etc., while the American will be expert in the highly specialized methods and results of the field and ignorant of anything outside of it. They don't seem to read anymore!

This exchange ended our interview with Heinz von Foerster. The influence of his charming personality and sophisticated interpretation of cybernetics, however, did not. As philosophers and historians of science, perhaps the most valuable lesson we learned from Heinz is

how the complexity of scientific practice must be reflected in a very open and flexible attitude toward the objects of inquiry, the research methods, and, ultimately, toward life itself. We hope that some of the open-mindedness that characterized his work and person found a way into ours.

References

Bigelow, J., Wiener, N. and Rosenblueth, A. (1943), "Behavior, purpose and teleology", *Philosophy of Science*, Vol. 10, pp. 18-24.

Guenther, G. (1976), *Beiträge zur Grundlegung einer operationsfähigen Dialektik*, Felix Meiner Verlag, Hamburg.

McCulloch, W. (1965), *Embodiments of Mind*, MIT Press, Cambridge, MA.

Maturana, H., Varela, F. and Uribe, R. (1974), "Biosystems", *Currents in Modern Biology*, Vol. 5, pp. 187-96.

Polyak, S. (1943), *The Vertebrate Visual System*, The University of Chicago Press, Chicago, IL.

Rohracher, H. (1947), *Einführung in die Psychologie*, 2nd ed., Urban & Schwarzenberg, Wien, p. 247.

Weston, P. (1961), "Photocell field counts random objects", *Electronics*, Vol. 34, pp. 46-7.