An Interview with

HEINZ ZEMANEK

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Abstract

Zemanek, an Austrian computer scientist, begins by describing his early life in Vienna, Austria and experiences in Nazi-occupied Austria. He discusses his engineering education and work in radar technology during World War II. Zemanek then focuses on the development of computers in Austria. Topics include: magnetic drums and magnetic memory, the MAILUFTERL computer (which Zemanek designed and built), the LOGALGOL and other compilers, the University of Vienna where Zemanek worked on his computer, the subsequent sponsorship of the project by International Business Machines Europe, and ALGOL and PL/I language standards development. The interview concludes with Zemanek offering a brief overview of the computer industry in Europe from the end of World War II to the 1980s.

HEINZ ZEMANEK INTERVIEW

DATE: 14 February 1987 INTERVIEWER: William Aspray

LOCATION: Vienna, Austria

ASPRAY: This is an interview with Dr. Heinz Zemanek in his home in Vienna on the 14th of February 1987. The topic is primarily the career and life of Dr. Zemanek. Let's begin by having you tell us briefly something about your early childhood, what your parents were like, how you were trained early in your life, and so on.

ZEMANEK: This should probably be answered by saying that I am a true Austrian. I was born in Vienna.[1] My ancestors came from Austria in the old sense, namely 3/4 of them from the part which is today Czechoslovakia and 1/4 from a little east of Vienna. My father, in the early years, had a position in Yugoslavia near Lubiana,[2] where he was in the iron industry; so that I spent my first four years on the countryside in Slovenia.[3]

ASPRAY: This would have been what years?

ZEMANEK: 1920 to 1924. I was coming back again and again to Vienna, of course, in between. But essentially I was living in a house which still exists. Many times afterwards I was in this place and now the people are all dead, but at the time we even had good friends there. We came back to Austria[4] and all the time from the year 1926 to the end of the second World War we had an apartment in the fifth district[5]. My grandfather, the father of my mother, had been

[1] on 1 January 1920

[2] Ljubljana in Slovene, Laibach in German

[3] in the village of Domzale, about 15 km north of Ljubljana

[4] we returned to Vienna on 20 November 1924

[5] VIENNA 5 (or area code A-1050) Schonbrunnerstrasse 69/7. My parents continued to live there. My father until his death on 16 June 1973, my mother until the fall of 1980; she died in a Home for Retired People in Styria on 14 May 1982.

an employee to the Spanish Riding School, which roots me really in the Imperial Castle in Vienna. There is the court behind the Spanish Riding School building, which was practically the property in the sense of a child, of my sister and me, because nobody else had access to this court. The condition was to be silent. All the people living around would have protested and would have made difficulties for my grandfather if we would have been too loud. But otherwise we were free to live in this very nice Imperial -- well, Imperial Court sounds too fantastic -- it's a simple court. Part of it is ground for training the horses. Part of it is of garden character. It was nicer in my time than it is now, but it's still in existence. There is even still a doorbell with my grandmother's name,[6] because when they transformed the Stallburg back into its Renaissance shape, (and that's the only important Renaissance building we have in Vienna), they removed her kitchen where the bell ended; and so the room doesn't exist. Nobody removed the line and the doorknob and so you can still read my grandmother's name.

I was not bad at school. I had excellent tests and notes, as we call them in German. So, I could have gone in many directions for my profession. I practically fulfilled an idea of my father. He had seen, as a business or bookkeeping man in the industry, that it was the engineer who understood the business. So he wanted me to become an engineer and I agreed. I mean, I could have gone into theology or languages, whatever. I think today, if I were not in the computer field, not in telecommunication engineering, I might have become a researcher in the field of family names -- to give you just an idea. So, I accepted engineering. I accepted electric engineering, which was his field in Vienna. What I did not accept was his proposition to remain in power engineering. I made the switch to telecommunications. That was already my decision.

ASPRAY: Before you go on, tell me a little bit about your early education. Was it a classical education? Was there a lot of science and mathematics taught? Did you go to a special school or to the regular schools?

ZEMANEK: The first four years in Austria elementary school are equalized over the country. There's nothing

[6] After the death of my grandfather Josef Renner on 28 June 1944, my grandmother got a retirement apartment in the "Stallburg", a former castle -- in the Renaissance style -- a part of the Imperial Castle complex. The apartment was above the stables of the Lippizan horses. She lived there until her death in 1956.

special. I had some experience because I had to switch. I was in three different schools, first because we moved from the first district to the fifth; and then one year they had problems with the size of classes, and since I was one of the better pupils they sent me for a year to a different school and called me back again. But, otherwise it was a normal elementary school. Then the decision was, I would go to what is called Realschule. That is the science and technology preparing variation of the language-oriented Gymnasium. So I had Latin, for instance, only as a voluntary course of several years. My languages were: French -- 7 years, and English -- 4 years. Mathematics all the way through. I had very good mathematics teachers most of the time. I learned a lot in mathematics. I was probably far ahead of the average as far as mathematics was concerned. But nothing conspicuous, no special school. It was simply the Realschule of the neighboring district where we had been living. So, up to this point, I have nothing special to report.

ASPRAY: Before you go on to your later education, what about hobbies or special interests of your family that would bring you into contact with engineering or science; anything of those sorts?

ZEMANEK: Apart from the fact that my father was in electrical industry, his company was mainly producing boilers for heating devices, none. The hobby of the family, if there was one, was music. My father performed on a full set of instruments and, also in the heavy times, when he had no position or even when he had one and was underpaid, he would earn additional money by performing music instruments somewhere in a band or in an evening locale. My mother played the zither. So there was always music around me, and I was first invited to learn the violin, which didn't work. I couldn't hear myself playing my mistakes and made myself too nervous. But piano worked out. Although my father was not very patient, I never had another teacher; so the remainder was self-education. I play up to this day on the piano, classic music, certain kinds of dance music, and so on. For instance, during the wartime, I had an improvement of my low position by being the company musician. I had an accordion which I brought from Vienna to Salonika and I was borrowed out to entertain the evenings in the Army. The advantage was: I got to drink, which I was not much interested in; but I got, of course, also, more to eat than the others. That was of some importance in wartime. So the hobby was music.

Then, the other point was, I was relatively heavy and thick in my early school years and not very good in gymnastics. Therefore my father said, "First of all it's this and secondly, you are too much family-bound. I want you to have a different environment," and he started to send me to a gymnastics association once a week to exercise. And it didn't work, of course, because I had no chance of becoming good. And secondly, he made the wrong choice in the sense that I was in something that today would be called a Nazi environment. That's the wrong term. They were just in Austria what was called national (in their philosophy). And that was not to my mind. I didn't feel well there. So his next attempts were the Boy Scouts. There he made a similar wrong choice in the other direction. I fell into a totally Jewish group -- to which I didn't object, but first of all, I again did not fully belong to them, simply because I was not Jewish; and secondly, that particular group was very rich Jewish people having fantastic apartments. I was the poor guy, which didn't fit properly. Then the third attempt with the Catholic Boy Scouts was the success. I see myself still, a little unhappy walking up to the place. Then after a quarter of an hour, the whole thing turned around. I was in the right environment and I worked. First, I was educated by the Boy Scouts, a very good preparation for my later military service. Secondly, after World War II, I helped them to reorganize, to build the organization which of course, the Nazis had stopped totally. I remember fighting with Nazis on the Ring.[7] But I soon gave up because I realized it didn't make any sense. After 1945, I helped to build up the Boy Scouts again; I ended up as International Commissioner, which is a kind of Foreign Minister of the Boy Scouts for Austria. I had my fingers a little in the organization of the Austrian Jamboree, the big international camp in 1951.[8] But, I was really the Austrian representative for the organization of the international Boy Scouts conference in 1951 in Salzburg.

ASPRAY: Any science background? Ham radio? Radio?

ZEMANEK: Yes, that is true -- my father liked to make his own radio equipment, starting with very primitive detectors. Slowly, one tube amplifier and then more complicated ones. But his technical knowledge was not very

[7] in April 1938

[8] A Jamboree is organized every four years. I attended 1937 in Holland, 1947 in France (there was none in between because of the war), 1951 in Austria, and I visited 1963 in Greece.

deep, and normally when he had built it up, it didn't work and he had to look for some of those advice organizations and helping organizations to get it really going.

ASPRAY: Did you participate in this?

ZEMANEK: He was not very patient with me; a little bit I did, but not very much.

I could mention many events in my life. In particular, for instance, how I experienced the year 1934 with the uproars in Austria. There was the kind of attempted socialistic revolution and then the Nazis tried to take over power. But the 'Christian-social' government remained in power. I didn't feel well with them either. So, politically seen, I was a little isolated all the time. I did not like any one of the easily offered directions. Until today, I have kept out of politics. I have never touched any political party. I would be ready to help any of them if it fits to what I have to say. And I have done so. I have, as I told you, given lessons to one of our Prime Ministers. I have advised Mr. Kreisky once or twice, but I was never involved really in any particular political party as such.

ASPRAY: Why don't we turn then back to your later education?

ZEMANEK: Okay. The decisive point was, would I go to the university or not? My grandparents, who did a lot for our support when the hard years were coming along, feared I wouldn't make it -- not because of lack of talent, but because of the financial, the economic difficulties. But finally, the decision was made: yes I will make it. And I made it. So, I enrolled at the University of Technology in Vienna (Technische Hochschule at the time) for electrotechnics, and then I decided one day, I would made it on the telecommunication side.

ASPRAY: Can you remember what the reason was that you chose to go into the communications side of engineering rather than the power side?

ZEMANEK: One item was, I liked it, but there is another answer I can give you. I was not very good in drawing. In telecommunications, you merely had to make drawings of principal importance. Nobody really required you to make a drawing to one millimeter precise. But probably, I also felt that telecommunications had the more closed theories. I never have liked, in my life, engineering activities where you cannot fit the theory to what you are doing. I'll give you one example. Often I have been invited to go within telecommunications on the high frequency side. I never really did that for the simple reason that[9]... Let me give you one example. There is no way to explain how the signal goes off the antenna. There are equations. There are lots of stories you get. You have equations and you have this and that that could be said. But essentially, I have never understood how the wave gets off the antenna in all the literature I have studied; while in telecommunications, you have the network theory, which was very developed also over here, mainly on the basis of Feldtkeller's books[10] (who afterwards became the professor for my first thesis).

I felt, mentally, in better shape in telecommunications and I found that low frequency was a much more ordered field, supported by better and cleaner theories. So this is a serious argument. But the drawing was not without importance.

The problem was that in 1938, there was the occupation of Austria by the Nazis. I suddenly was confronted with a political direction I didn't like. They were sitting at the university. One had to find a way how to deal with these conditions, and it was not easy. I used this nice psychological trick. I went to them and said, "I'm not a Nazi, but I give you an opportunity to make one out of me." They were deeply impressed by this. They attached me to one of their units. I never had to formally go in. The second trick was, in contrast to most of these people in Austria, I had read Hitler's *Mein Kampf.* Not only had I read it, I had a set of quotations to be used for defense or when I was opposed to what they were talking. So, in a debate, I very often would give a sign and say, "But, doesn't the Fuhrer say so and do in *Mein Kampf* on page number so and so?" That heavily impressed them. I was not recognized, but I

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[9] that I did not see the theoretical base clearly and invitingly enough

[10] The two basic books by R. Feldtkeller were on filter theory:
R. Feldtkeller: Einfuhrung in die Theorie der Rundfunksiebschaltungen, S. Hirzel, Leipzig 1940; 168 pp.
R. Feldtkeller : Einfuhrung in die Vierpoltheorie der elektrischen Nachrichtentechnik, S. Hirzel, Leipzig 1934; 142 pp.

was respected. I was never troubled. The whole thing was, anyway, only two years. Then the war broke out in 1939, and I was called into the army in 1940.

ASPRAY: Before we go on to the war period, can you tell me what you would have learned in a communications engineering course of study at the time that you were in school? What sorts of topics were covered, and what was not covered that would be covered later on?

ZEMANEK: Oh yes, that's easy to answer. The main subject, low-frequency technology, which concerned me and which was the entry part of the whole study concerned network theory. It was a nice theory and very much on my hand with a lot of complex computations. So I learned early how to deal with the slide rule and how to work out functions in the complex domain. There you have to switch between amount and angle on one-hand side and real and imaginary part on the other. All those computations you had to do by hand with the slide rule. So I had some feeling for numerical computation in this sense.

The second big chapter of theory was amplifiers. How do you compute the conditions under which an end amplifier works -- tubes of course at a time, the transistor wasn't there yet. Well, and all the other fields of telecommunications, for instance: telephone switching was one of my particular fields. Normally, this is a side field for telecommunication engineers. In my case, my first bigger income apart from giving lessons to students, which I did from my very early years on, was that I was hired by a former assistant professor who wrote a book[11] on the telephone switching system, and used me as a first reader. In other words, I had to read what he had written, and I had to tell him when I didn't understand something. By this you learn enormously. Since then, I had a perfect understanding of the switching systems. As a matter of fact, we had one exercise in switching at the University of Technology, which usually took the group 6 or 7 hours. I made it in 30 minutes because I came, well-prepared with my knowledge, supported by a prefabricated list: we had to interconnect the points and, of course, it went without any mistake and they were very

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[11] E. Winkel: Einfuhrung in die Wahltechnik. - R. Oldenbourg, Munchen 1942
H. Zemanek [list of works of Heinz Zemanek in "Formal Models in Programming" (E.J. Neuhold, G. Chroust, Eds.), Amsterdam 1985] (1962)

deeply impressed. I also learned telegraph machineries, the early, fast printing telegraphs and so on, teletypewriters. These helped me again then in the army, because I came to a telephone unit, but I was used as a teletypist. I was then the only one who really had studied the technology behind.

Another example: we were using carrier frequency units for more than one communication on a line in different frequency ranges. So I had the practical experience with telecommunications. I had a useful preparation for later life. The Nazis did another thing. They shortened the study of telecommunications, from 9 semesters or 8 to 7. And when the war broke out, they said, "Oh, this is a short war. It does not pay to open the university before the war ends." Now this was a slight mistake, and when they realized it was a mistake, they opened the university this year (1939) on the first of November instead of, as is usual in Austria, October. At the same time, they introduced trimesters, so that the 'semester' which used to be half a year, or at least 5 months, was suddenly only 2 months. The next one was January, February and March, and the third one was April, May and June. So I did the second half of my academic studies within one year, which I made as far as lectures and exercises (that is laboratory exercises) are concerned. What I did not make were all the examinations. So I had to go into the army in the fall of 1940 without having completed all the examinations. That remained open. But I had completed all lectures and exercises. There was no need to reenter the auditories. I only had to make the examinations, which I did between 1943 and 1944. Actually I passed my diploma engineering examination three days before Christmas 1944. What happened was this: I was for three years in the German army. I was sent to a telephone company, telephone and teletype. I had been trained as a teletypist, but they put me to the telephone. I should say that I saved myself from being brought to the army at the opening of the war because I had just visited Boy Scout friends in Paris. I reentered Germany only three days before the war broke out. That's a separate story, too long for this account, a very interesting one. Anyway, when they called me then to the army in October 1940, I first was in Vienna for basic training. I was immediately myself a teacher because I understood teletypes. I could better explain them than the teacher they had. Over Christmas I was in some headquarters here in Vienna. Then in January, they sent me down to the 'teaching troops' in Romania. From there we then moved into Bulgaria. They threw flowers on us, not with the pots, you know, really flowers. To greet us; it was very nice of them. Well, I mean, I did not like the army, that's another point. But relatively I was in a good state. In April 1941, we entered Salonika only a couple of days after the German army had taken it. This was then for almost 3 years of my military service my place. I did the following when we entered the city: I simply got hold of a soldering iron and joined the guy who was responsible for what is called 'Hauptverteiler', that's the point where all the lines come from outside and from where they are arranged to go into the switchboard. The guy looked very suspiciously at me, but after a while he started liking me. He saw I understood what I was doing. So I became one of the key technicians of the company. That helped me, of course, in many respects. I had been for one summer in Athens, but then they put the H.Q. back to Salonika. And I was a temporary teacher at the German Army School for Telecommunications in Salonika.

ASPRAY: Teaching what sorts of things?

ZEMANEK: Well, they had to educate the members of the telephone companies in technical matters. This was done in specific courses of slightly, but not very much different character in that particular school. I was used as a teacher there. Now, my rank, of course, was only Obergefreiter, which is not even Sergeant (so I made no career in the army). But I had a wonderful time as a teacher. I started preparing myself for returning and completing my thesis. I always had hoped they would do some action that I could grasp. Actually, I have helped do it. It's another long story which I'm not going to tell you. I moved a file card with my name in Berlin a few ranks ahead. What happened was that in 1943, when the Nazis realized that they had a drawback in radar technology, there was an order by the Fuhrer that 50,000 people should be called back from the army and a big radar program should be started. I was two of them; namely, I got a two-fold call back: one to northern Germany and one to near Vienna. Of course, I chose the one near Vienna because I wanted to finish my studies. So for several months I was staying near Vienna, and I made all my examinations up to the end of April 1944. The open problem was I had no diploma work. The condition for the degree was to complete the diploma work. In the middle of my examinations, they suddenly called me off from the neighborhood of Vienna. That is another interesting story. The boss of my institute, who was expected to work for winning the war, with radar technology, was discovered rather to prepare the post-war Austrian technology than for the war. So he was removed and replaced by somebody else. Most of the Austrians were sent into Germany. That hit me too. After my last examination I went to Berlin, and that was nothing for me. I stayed there for 6 weeks or so. Then I managed (I could already play on that machinery at the time) to be moved again. I came near Ulm in Germany (that's up the Danube River, not too far from Stuttgart) into an institute where they said, there I will have an opportunity to complete my thesis. I chose the production of microseconds. I think I am the first one in the German-speaking area who really produced microsecond pulses for telecommunications purposes. You know, the power engineering people had pulses for other reasons, maybe. But really, in the modern sense, in the sense of computer technology, I think I was the first one. With Professor Feldtkeller, the famous German telecommunications professor, who had written the basic book in Germany for Network Theory.[12] He had no idea how to produce microseconds. He first misled me, but we soon found out we were on the wrong track. Then I did the following: imagine a sign wave overamplified to such a degree that what finally appears is almost rectangular, because the upper and lower parts are cut off. When you then differentiate, which you do by a condenser and a resistor, you get the derivation of this function, which are positive and negative pulses. One species of them you use, and then you have your microsecond pulse -- provided your arrangement really makes these short periods; provided, for instance, that the cathode of that tube produces enough electrons. If it's too weak, you don't get out the necessary current within the time. So, with the tube I had been beginning with, I couldn't make less than 20 microseconds. I had to change to a power tube. Then I made my dream, the one microsecond pulse. That was the subject of my diploma work. I went for Christmas 1944, to Vienna, passed my examination, got under ordered condition my diploma, and that problem was resolved. After Christmas, I traveled back to Germany. And in April, as you know, the Russians entered Vienna and I was cut off from my family and from my family.

TAPE 1/SIDE 2

ZEMANEK: When the Americans entered Stuttgart, the Institute was moved from Ulm to Upper Bavaria.[13] So this is

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[12] The two basic books by R. Feldtkeller were on filter theory:
R. Feldtkeller: Einfuhrung in die Theorie der Rundfunksiebschaltungen, S. Hirzel, Leipzig 1940; 168 pp.
R. Feldtkeller : Einfuhrung in die Vierpoltheorie der elektrischen Nachrichtentechnik, S. Hirzel, Leipzig 1934; 142 pp.

[13] The institute was called "Zentralversuchsstelle fur Hochfrequenzforschung". From Ulm-Dornstadt it was moved the Beyharting near Rosenheim in Upper Bavaria around 20 April 1945.

where I was between April and June. In early May, the first days of May, the American troops covered practically all Bavaria. There was no resistance any more. One day we had the Allied Technical Secret Service visit the institute. As an Austrian, I was immediately appointed translator. I moved around with the officers, and helped with my English. They were satisfied of what we had to tell and disappeared. Now the problem was, I had no orderly papers of dismissal from the army. Actually, I was put out of the army by the director of the institute, who certainly was not entitled to do so. The Americans in that area released the Italian-German army. So I was constantly in the danger that somebody would come and say, "Where are your papers and are you legally out?" So we tried to get out of there. But I didn't dare to return to Austria because, while there was no mail service and there was no telephone news did anyway spread, as it is in such conditions. I knew that the Russians had taken away from Vienna two of my colleagues who knew much less than I knew about the German radar technology. I was afraid if I returned to Vienna, the Russians would get hold of me and invite me to Moscow, which I didn't like at all. So I moved back in the Ulm area, opened a little shop[14] for electrical repairs and radio repairs and waited there until February, 1946, when I had news that these two gentlemen were back from Moscow.

ASPRAY: Before you go on, two questions about the war. What were your duties in the radar area, and what did you learn -- practical experience, theory or whatever -- that you hadn't learned in the university?

ZEMANEK: At that time, at the university, information was essentially on the analog side. In other words, everything would be treated by sinusoidal functions, although there was Laplace transform and the theory of complex functions, but no practical application. Pulse technology, at that time, was not a practical matter. All of that I learned only when I worked there. The whole thing started in the following way. I was relatively good in Laplace transform. I was interested in it. So I handled Fourier series, Fourier integrals, and Laplace transform better than the average of my colleagues. And when one of my colleagues had to do with some pulse questions, not yet radar, I don't know what it was, he mentioned something to me and made a nonsense statement about the spectrum. So I said, "This is different, I know." He said, "Oh, you know. Then you could help us." I got some kind of job, not

[14] in Bernstadt near Ulm

really a contract, to show a couple of equations concerning Fourier transform, something like that. That remained in my interest. Then, of course, when I joined the Radar people, I was already knowledgeable about it. Now the following happened. I was called back from the army, appeared in the Ernst-Lecher Institute[15], and there was a German engineering officer who said, "You will be a part of my project, which is the following: I have invented a tank-to-tank communication system. High frequency of course. I will use pulses so short that the enemy cannot hear the communication." I said, "To my knowledge, he will hear it if he has enough amplification, simply because of the inadequacy of his receiving equipment. Because if you cut off the spectrum, what remains of your short pulse is exactly the low-frequency signal." He didn't believe me. But he was fair enough to charge me to give him a mathematical proof. Then he also charged somebody else to produce short pulses, not so short ones as I did later, but millisecond pulses. Then he wanted to check on the loudspeaker that one cannot hear them. But one could, of course. I brought the mathematical proof to him.

They had all kinds of other ideas. For instance, to remove antennas in case of a thunderstorm caused by a signal from another antenna. I was laughing about them. I said, "You need the antenna to find out whether you should remove it or not remove it, which looks like the antenna you want to remove." There were all kinds of silly ideas around. I'll give you one example. Once we kept a sergeant from being called back into the army by the following giant invention. At that time, there were deep-flying enemy aircraft.

ASPRAY: Low?

ZEMANEK: Low. Very Low. In order to defend the country against this kind of enemy, the idea was to send a relatively small rocket up which carries a wire, a rope kind of wire. This stays during the time the rocket goes up, more or less like a string in the air. It would cut off the wings. So with that simple idea, we said, "He has to work on this project, whether we can show it's feasible or not." So we kept him from being called back.

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[15] "Ernst Lecher Institute" an Airforce Research Institute, in cooperation with the University of Technology Vienna, located in Reichenau am Semmering, about 100km south of Vienna

I learned a lot in all directions, in theory, in practical circuitry. I remember, for instance, doing in Germany in this institute, (rather, we were already in a school to work), multivibrations with time constants of an hour. We used a clock that would ring an alarm a little before. Then we would all look on the oscilloscope: it would make one impulse and then was an intersection for an hour. I just wanted to show that it works also for large capacitors and for long times. I simply learned as much as possible about pulse technology. That was preparation for what I did after the war when I went back to the university. Computers, I couldn't do. I was already fascinated at the time. We heard a little about it and then even more, of course, before and after 1946.

ASPRAY: Do you remember how you heard about it first?

ZEMANEK: Oh, there were rumors, for instance, in the context of the missiles V1 and V2. We understood that there was lot of problems of computations. We heard that the Americans have computers, and we heard about Zuse. Maybe, I cannot recollect it in detail.

ASPRAY: Were you familiar with any kind of differential machines at the time?

ZEMANEK: I was familiar with the harmonic analyzer. There was an inventor in Vienna who once wanted some kind of mathematical engineering judgement and came to see me. Yes, I was. I knew about that.

ASPRAY: I see. Was this an electronic one or a mechanical one?

ZEMANEK: Mechanical. Yes, rotating cams; things like that.

ASPRAY: I assume you also knew about punched-card machines.

ZEMANEK: Oh, sure. I mean, that was in principle known, but I had never seen one and I had no context of them until very late.

ASPRAY: I see, so you weren't really familiar with punched-card machines at the time? Though one could have been at the time.

ZEMANEK: No, but one could have been. On the business side, punched cards were very normal. I certainly had seen one or the other. I don't really recall it. They had no importance for me. My entry was pulse technology and the bridge from these early attempts at radar technology to the computer was digital telecommunications.

ASPRAY: Yes.

ZEMANEK: Now there is another story which I have to tell you. Let us make it a little shorter. I mean, after returning to Vienna, I first started with a friend of mine, Ernst Steinbrecher, to write a book about our knowledge in radar technology.

ASPRAY: Yes.

ZEMANEK: At that time, of course, we didn't know yet about the Lincoln Laboratory Series. Moreover, there was almost nothing in German. We had a list of contents together. But then we found out we should make our living and the book would not yield this. So we opened a little shop for electronics and developed all kinds of electronic devices, mainly with a lot of material that remained from the German army. This also led to my first little brochure, a book together with this friend, which was to show how with a very known tube, exactly the same as I started to use to do my pulses,[16] one can do every kind of tube application -- essentially for building radio receivers, with one kind of tube only. We explained how to use it, even as a rectifier tube.

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[16] RV 12 P 2000. The more powerful one was RV 12 P 3000.

We did radio receivers, a tube-test set, and things like that. But after a year, I found out I was on the wrong track. Moreover, my friend offered me not so good a contract, and at the same time the university had offered me the opportunity to return and become a university assistant.

ASPRAY: At a beginning position at the university?

ZEMANEK: That's right, yes. So in the fall, by October 1 of 1947, I came back to my university as Wissenschaftliche Hilfskraft, which is the lowest grade of an assistant professor. They had established the former owner of the chair, who had been prosecuted by the Nazis and been expelled from the institute. He was in Holland and had worked there. He had returned. He was an old gentleman. He did very little. He left everything to the assistants, and you know what this means: you learn infinitely much. We did everything. We did the lecturing. We did the laboratory exercises for the students, and we supervised the diploma works. Suddenly I had a kind of staff, namely all the students that wanted to complete their diploma examination by the diploma work. They all did some kind of projects. It's there where I tumed into digital technology.

I had to do several steps to prepare the computer. One was going over from the sinusoidal thinking to the pulse thinking. The second was to increase the number of elements involved in a diploma work. Normally, a diploma work would have one or two tubes, and the complicated ones had four. Now I started with subjects needing a hundred tubes. That was absolutely new. Also, I started teamwork. On this I had legal arguments with the university who said diploma work must be the work of one person because it's his examination. I had to argue back, "What you really considered is not the technical work. It's the written version of what they have done. It's very easy to have four people working on one project and each of them producing one document." I won. So I was able, for instance, to develop a PCM (pulse code modulation) system done by four of the students, using many tubes. And there was lots of possibility of applying pulse technology. In this way, I prepared everything for going into the computing field.

ASPRAY: Now you weren't thinking about going into the computer field at this time though, were you? You were working on digital communications technology, or am I wrong?

ZEMANEK: You are wrong in the following sense. Of course, I was quite aware that communication technology would teach me a lot and was necessary as an intermediate step before I could really go into computing. By the way, I was absolutely aware that I would go down with any tube enterprise. If you think merely on the power requirements for a system with thousands of tubes (and anything else would not have made up a real full computer at the time). I was quite clear that that could not be done in this place and at this point of time. But I went into relay machines. I built myself (I had one diploma work going, but otherwise I did it myself) a parallel relay computer, the URR1 (see picture) with a word length of 18 bits, with 16 storage places; multiplication, division done by a rotating switch. It never really worked, but I learned all of the functions of a computer.

ASPRAY: When was this?

ZEMANEK: After 1950; between 1950 and 1954. I can show you the famous photograph,[17] which still carries me around in Vienna, because it was done by a renowned photographer. It appeared in the Magnum Journal[18], and it's one where you see the relay computer. I look through a window, so to say.

Another thing I did was the following idea: a computer[19] which was in essence an analog computer, but the tension

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[17] The photograph is attached in copy. The computer is the URR1, the Relay Computer, see H. Zemanek -- list of works of Heinz Zemanek in "Formal Models in Programming" (E.J. Neuhold, G. Chroust, Eds.), Amsterdam 1985, [42] (1955).
The photographer was Franz Hubmann. This picture was often reproduced. Recently, in an exhibition called "The Wild Fifties" it was shown in life-size.

18 MAGNUM, a high level illustrated journal by the publishing house "Austria International". The picture appeared in issue No. 5 of MAGNUM, page 55 - see
H. Zemanek - list of works of Heinz Zemanek in "Formal Models in Programming" (E.J. Neuhold, G. Chroust, Eds.), Amsterdam 1985 [50] and [51] (1952)

[19] J. Horner: Eine Analogie-Rechenmaschine mit Schrittschaltantrieb zur Losung von Algebraischen Gleichungen

was applied in digital steps. You would, for instance, by an analog principle build up, say, a power series. Then you step through the different values and when it reaches zero, it stops. You have found the root of the equation.

ASPRAY: The purpose was for solving equations in this case?

ZEMANEK: Yes. Another field of application was telecommunication by pulse technology as such. So we started pulse code modulation and pulse delta modulation. You know what delta modulation is?

ASPRAY: No, I don't.

ZEMANEK: Suppose you have some kind of telephone current curve. You switch around this curve by doing the following. When the tension goes up, you make a step upwards. When the tension goes down, you make a step downward. What you transmit is only the pulses which indicate whether up or down, and even then you can remove the negative ones, because if you don't transmit, it means a step downward. If you transmit one, it means a step upwards. That is called delta modulation. You have to make short the timing intervals,[20] but you have only one amplitude; so its a very interesting form of mo dulation.

Now here I should tell you about an interruption I had in my position in Vienna. There was an offer of the French government for scholarships. And my boss, without thinking it through, encouraged me to apply. When I applied, I was accepted, maybe because I spoke French. So the university had to keep me free for a year, and I spent the year

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bis zum 6. Grad. OTF 8 (1954), p. 153-158.

[20] The system covered 'medium fidelity' audio signals, i.e. up to 3400 cps by a pulse frequency of 100 kcps for 32 amplitude steps see

H. Zemanek - list of works of Heinz Zemanek in "Formal Models in Programming" (E.J. Neuhold, G. Chroust, Eds.), Amsterdam 1985 [12] (1952) and

L.J. Libois: Un nouveau procede de modulation codee, Onde Electrique 32 (1951) 26

R. Stampfl: Bemerkungen zur Impuls-Delta-Modulation, Osterr, Zeitschrift fur Telegraphie, Telephonie, Funk - und Fernsehtechnik (OTF) 8 (1954) 58-63, 92-97

R. Stampfl, Impuls-Delta-Modulation, Dissertation, H Wien 1954

Pulse Delta Modulation has come of fashion in the Seventies.

in France. That was the scholar year 1948-49. Very interesting; already at the beginning, I assisted to a television conference where for the first time in my life, I saw a 1000 line picture. So there was a lot to be seen in Paris. Only, with my main subject I didn't make any progress for months. The reason is the following. At that time the "pope" of vibrations in any form was the French professor to whom I was attached. When I told him I wanted to work in the field of computing, he said nothing. "Well," I said, "At least in pulse technology." He said, "Everything is already done. I wish you would go into 26 centimeter high frequency waves."

ASPRAY: What was his name?

ZEMANEK: His name was Rocard. Yves Rocard. His son has been a politician over the last decade or so, or 20 years. At that time, Yves Rocard was at the Ecole Normale Superieure in Paris, the big man.

ASPRAY: Yes. Were you at other institutions in Paris while you were there?

ZEMANEK: Yes. I heard lectures at the Sorbonne. I even assisted to a lecture by Prince Louis de Broglie, the famous physicist. I could not understand a word. He was not a gifted teacher, I would say. It was the subject too, but certainly he was un-understandable.

I was also following courses and work then at the Ecole Superieure des Telecommunications which is the PTT University in Paris.

ASPRAY: Yes.

ZEMANEK: There I found the first traces of pulse technology again. A gentleman by the name of Naslin was one of the lecturers in that field.

I met Mr. Libois,[21] who is a very high officer in the French PTT, who supported me. I finally joined, then, a PTT project on pulse code modulation. This is where I have learned pulse code modulation. My contribution was a circuit to divide a sequence of pulses by seven. I did it in the following way. The sequence is amplified. The first pulse is sent into a delay line with a corresponding interval, you send back to the grid of the line the blocking version of the pulse. Suppose it's a sequence of positive ones. So after amplification the sequence consists of negative pulses. The first pulse taken from the delay line, blocks the grid and the next one cannot pass. So, depending on the length of your line and the number of taps, one can divide. If there are 6 taps, only every 7th pulse parses. You could look up the early publications in America (which were read by the French), of course, in order to see why you have to divide by seven: because it was a seven-bit system. On the receiving side, you have to find out the appropriate reference point in time. In order to do that, you afterwards have to divide by seven. That was the idea. The French did them in a different way. I not only learned a lot again about new technology. This yielded my first publication in English in the British journal, *Wireless Engineer*.[22]

I also tried in Paris to get contact with the French computer development, which was not easy. It took me from the fall down to March or so...

ASPRAY: Couffignal?

ZEMANEK: Yes. Until I got the name and finally the address of Monsieur Louis Couffignal. I visited him and he explained to me everything. He was very positive that he would very soon beat the Americans. I didn't believe this

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[21] L.J. Libois, later one of the directors of CNET (centre national des etude de telecommunication) see
L.J. Libois: Un nouveau procede de modulation codee. - Onde Electrique 32 (1951)
for P. Naslin see
P. Naslin: Circuits logiques et automatismes a sequences. - Dunod, Paris 1965; 470 pp..
It is interesting to note that the French, reknowned as a nation of logic, have not much of original computer logic publications.

[22] H. Zemanek - list of works of Heinz Zemanek in "Formal Models in Programming" (E.J. Neuhold, G. Chroust, Eds.), Amsterdam 1985. [22] (1954).

story. He said, "I cannot show you anything at the moment. Come back in six weeks." I came back after more than six weeks and he told me exactly the same story. What he changed was only the parameter to say, "Come again in a quarter of a year." So I concluded he was on a diverging ground, and I was right. As you may know, it ended up by his buying an IBM 350. He never got the Calculatrice Institut Blaise Pascal going. But you find the publication in one of the Aiken volumes.[23]

ASPRAY: Right.

ZEMANEK: So, this was my experience and I knew, for another time, to be very careful before you enter a computer enterprise. I had all the necessary warnings to keep back until the right time was coming. And the right time was coming when the transistor was around.

ASPRAY: Now, when did transistors become well-known in Austria?

ZEMANEK: Well, to repeat it, in order to get going in Austria, my first problem as a young assistant was to get hold of the modern literature. The wartime had the consequence that practically no English or American literature was coming in. We had a hole between the years of 1939 and 1947. Well, the American literature, say the *Bell System Technical Journal*, had come in down to the entry of America into the war. But then it was at an end, so the most interesting papers we didn't have at hand. I had to convince all my friends at the different libraries to subscribe again. I emphasized in each library a particular direction so that on the whole I had the kind of completeness I was aiming at. Step by step, I built that up. Then, of course, once you have the *Bell Laboratory Technical Journal*, you have the transistor.

Also, we had here the scientific offices of the four occupation forces, of the four armies. The American and the

[23] Louis Couffignal: Traits caracteristiques de la calculatrice de la machine a calculer universelle de l'Institut Blaise Pascal. In: Proceedings of a Second Symposium on Large-Scale Digital Calculating Machinery, Harvard Computation Laboratory, Sept. 1949, Harvard University Press, Cambridge MA 1951, p. 374-386.

English were interested in what I was doing and I soon had very good contacts. When I started talking about computers, I got here and there a piece of information. In the long run I could build up quite a good picture. This describes the situation down to about 1954, which is another key year in my development. What else should I tell you?

In 1952, my boss handed over Norbert Wiener's *Cybernetics*. I still have the book I could show to you with his signature. I tried to read the book. I didn't understand much. The mathematics Norbert Wiener was using was too high for me, and the technology was too poor. So I looked in my other sources and I soon discovered what Norbert Wiener describes is one story and the basic models of cybernetics is the other. I decided I would copy all the cybernetic models with my students, which as you know and have seen in the museum I have done. So I am probably the only place on earth where all the three were running: the artificial turtle, in other words the model for the conditioned reflex behavior.

ASPRAY: Modeled after?...

ZEMANEK: After Grey Walters' two artificial turtles; then Shannon's 'Mouse in a maze', the model for orientation; and Ashby's 'Homeostat'. By the way, I also built, with some students, playing machines; I had a tic-tac-toe kind of machine built. This was another stream of development. The third stream was the Vocoder. I got hold in a British journal[24] of their vocoder development. Then, of course, I discovered that the Bell Laboratories had had a much earlier vocoder and also the Voder. Do you remember the Voder? That was a piano kind of device, not for

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[24] VOCODER means "Voice Coder" - CODER means "VOICE OPERATION DEMONSTRATOR". The main American pioneer was Homer Dudley. see:

H. Dudley: Remaking Speech. J Acoust Soc Am 11 (1939) 169 - 177.
H. Dudley: The Vocoder. Bell Lab Rec 18 (1939) 122 - 126.
H. Dudley: Pedro the Voder - A machine That Talks. Bell Lab Rec 17 (1939) 170 - 171.
H. Dudley: The Carrier Nature of Speech. Bell System Techn J 19 (1940) 495 - 515.
H. Dudley, R.R. Riesz, S.S.A. Watkins: A Synthetic Speaker. J Franklin Inst 227 (1939) 729 -769.
H. Dudley: The Automatic Synthesis of Speech. Proc of the Nat Acad of Science 25 (1939) 377 -383.
R.J. Halsey, J. Swaffield: Analysis-Synthesis Telephony with special reference to the Vocoder. J.I.E.E. (England) 95 (1948) Part III, Nr. 37, 391 - 411.

transmission, but for production of speech in the style of Mr. Kempelen's speaking machine. The Voder had been shown at the world exhibition in San Francisco.

ASPRAY: When?

ZEMANEK: Well, 1944, 1942. You would have to find out. I have the paper somewhere.[2]4 So that soon became another line of my interests. I had a student, a doctorate thesis, building the first vocoder here in Vienna.[25] It was a tube vocoder that really worked fine. At that time I had an interesting friend, Mr Eberstark. He was one of those refugees from Vienna who went to Shanghai. Jewish people, they lived in Shanghai. He was highly gifted in languages. I wanted to have him as a co-operator, but he wanted to make money so he became a translator in the International Labor Office in Geneva. But before he went away from Vienna, he did a number of very nice, interesting experiments -- of which I still have the tapes. He could do the following. We would produce, on a tape recorder, a text in German, in English, it doesn't matter, but very flatly spoken, the minimum of characteristics you can make. Then he would touch the turning knobs, the potentiometers on the vocoder, which control loudness and pitch. He could then produce, say, an English sentence with German pitch and Chinese melody, or any combination you can dream of. He was so good that he really could do it. When you hear those tapes, you hear the three languages at once.

I was dreaming at the time to do something which I never have really done. Namely, since you have in the vocoder a separation between the sounding and the pitch, you could have musical instruments speaking. You use, so to say, the consonant part of the language, and instead of the normal pitch, you use the sound of an instrument. If you recombine them, you would have a trumpet or something, any instrument, talking. I don't know whether it would have worked out. I have so many ideas that we could not carry out everything we had in mind.

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[25] Dissertation Chr. Schwiedernoch, see:
Chr. Schwiedernoch: Entwicklung eines Vocoders. Dissertation TH Wien 1955; 29pp.
E. Rothauser: Ein Impulsverfahren zur Sprachubertragung nach dem Vocoder-Prinzip. Dissertation TH Wien 1960; 63pp.

I had, at that time, a young man[26] who came to produce stochastic music. That's a fascinating story in itself. He was not a real student. He was a self-made man who really had started with a device to control the working of the cylinders of a car or of a motor bicycle. With the same principle of mixing regularity and irregularity, he proposed to make music. I gave him the necessary parts and equipment and he did. It sounded like music, like modern music. It was modern music.

TAPE 2/SIDE 1

ZEMANEK: I had a regular student to restrict his very free system by the rules of the choir for voices. In other words, we had harmonies that moved from one to the next, restricted by the rule of proper composition. Unfortunately, I have no tape of this. I have the written form of the thesis,[27] but I have no tapes. So you see, there was a lot of things going in parallel.

You were asking how I knew about the transistors. I think I have answered that. Single pieces, I got as presents from my friends in industry. I had, of course, built up lots of good relationships to the different companies here in Vienna: Philips, Siemens, ITT, and so on; also the smaller ones. So, once in a while people were coming. We had American visitors. A certain Mr. Brustman, a graduate of the Vienna University of Technology who was with RCA, came to see my boss. He brought a glow lamp with 30 positions.[28] You know what I mean? A polycathode tube with the usual red light, but one that could be switched around. Thirty are necessary for decadic operations. Basically, one needs ten positions; but in order to control the direction in which the light point turns, you have to make one step in three.

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[26] Franz Wagner from Schwaz, Tirol. See:
H. Zemanek - list of works of Heinz Zemanek in "Formal Models in Programming" (E.J. Neuhold, G. Chroust, Eds.), Amsterdam 1985 [83] (1956/59)

[27] Rudolf Leitner, Diploma work, see:
R. Leitner: Logische Programme fur automatische Musik. Staatsprufungsarbeit TH Wien 1957; 60 pp.

[28] The decimal glow lamps were from Remington and called REMTRONS - see:
J.J. Lamb, J.A. Brustmann: Polycathode Glow Tube for Counters and Calculators. Electronics. November 1949.

If you interchange them, it goes the other direction, so you can add and subtract. I had a student, first of all, to construct, to produce this tube in Vienna.

ASPRAY: Excuse me. You'd seen these first in what year?

ZEMANEK: Well, the year would probably be 1948, but we better check and make sure that this is the correct answer.[29] First of all I got a student to build this tube with the aid of some small Vienna company. I cannot describe the exact sequences; there were several interconnections. Up came the idea of producing a statistical computer based on the Galton [Pascal] triangle -- a statistical device. Now imagine instead of using sand for the purpose, you use pulses. Each nail is replaced by one of those tubes; not by one, but by a set depending on how many decimals you want. By the way, it is possible to send a piece of grain into two directions, because what the particle cannot do, the pulse can do -- going two directions. So, the sum of the two probabilities need not be one. It can be higher than one, okay? But this is only a side effect.

Now, there is a statistical checking procedure named after the Austrian statistician, Wald,[30] a scientist who was killed in a flight accident at the end of the second World War over the Indian Ocean, in American services. Now, he proposed the following. A test can be a simple decision, whether a piece is okay or not. But you also can make the statistical tests as sequences. Imagine a Pascal triangle extended from left to right. After a test, if the answer is yes, you make one step upwards. If the answer is no, you go horizontally. And you put constraints. You say if the path sinks under one line, it's rejected. If it gets out above another line, it's accepted. As long as it is in between the two lines, it is still under test. This is a mathematical statistical principle about which I understand very little. But it's very obvious how you can project with a very simple program such tests and the necessary theories on a device realized by those tubes. So that was a concept of a machine. There have been arguments who really invented that

[29] Since the first diploma work is dated 1951, 1949 or 1950 is more probably than 1948.

[30] Harmuth does not quote Wald -- he quotes H.C. Hamaker in the Philips Technical Review December 1949, March 1950, June 1950 -- but the mathematicians -- the assistants of Professor Funk -- knew A. Wald's work and the used principle quite well.

structure... I'm not mixing into this. The student, whose name (see next paragraph) which you may even know, because later on he did other things in America, after having produced the table, decided he would build this machine as a thesis, as doctorate. I gave him my warnings that he should start the building of the machine before he was sure that the serial production of such tubes is constant enough that one circuitry can serve for all the counters, because one cannot equip each point with all the steering circuits: one circuit would be switched from one storage element to the next. The student, however, started the construction of the machine without being assured that the window of operation covers all polycathode tubes. Well, I am not going into details. The whole project failed. They had built up the machine but didn't get the constant tubes. They never got it.

ASPRAY: The student's name?

ZEMANEK: Harmuth. Henning Harmuth. He was, afterwards, working in the field of Walsh functions (which do something like Fourier composition by pulses or step functions). So he was something like a leading man in the United States in the area, but the area to my knowledge was not really a successful one. Now he has disappeared. I haven't talked to him for a long time.

So, that was another experience with a lot of warning character to remain on safe ground if one is seriously going into computing. The decisive point for starting my computer was that one day my boss came and said, "I have got a grant for you of 30,000 schillings. Now 26,000 schillings at that time were slightly more than $1,000. So, my reaction immediately was, "That's fine, but it's the wrong order of magnitude. I need ten times as much; I need 30 times as much if you take everything." But in money, I needed ten times as much. The boss said, "Start. You have all my support. I would you like you to build a computer and you will get whatever I can do for you."

ASPRAY: Why was he supportive of this kind of project? What did he see as valuable about it?

ZEMANEK: His primary field was in acoustics. He wrote a handbook which is still valuable today. His name is

Eugen Skudrzyk.[31] It's a Polish name, but he was Austrian. He left Austria shortly after assuring me that he would give me every support. He moved for one year to the State University of Pennsylvania, came back, and after a short period he went forever. Still today he is in State College, Pennsylvania. So for several years I was on my own. Having this money in my hand, I decided, now is the time to go into computer development because the transistors are close enough to start. The period of the point contact transistors had already been over (it was clear that they were not stable enough). The idea was to build a computer, using semi-conductor technology, (which will allow me to cope with the power supply, which is the main problem for any kind of tube computer). I had already started work on the drum storage, magnetic drum. But now came the question, what can I do in order to get a full computer together? I need, obviously, the support of all industrial sources I can mobilize. But in particular, I need two things: an assurance from a big company that I get enough transistors, and the assurance of somebody to provide me with the money to pay the collaborators. Because I was clear that the computer development could not be done with students. The plan was different. I gave out a series of about ten diploma works of preparatory character. I selected, afterwards, five of them to be the team developing the computer. Each had the specialty he had developed already as a diploma work. So there was guy number one with the magnetic drum, that was the earliest. There was a technical physicist to look into semiconductor circuitry. There was one to deal with the core store. There was one to deal with what we would call today the architecture; we said simply "layout" at the time. This guy, by the way, was financed by Zuse with the understanding that as soon as he had completed his doctor's thesis, he would join Zuse's company, which he did. (He became there the chief engineer and is today one of the technical directors of Siemens because Zuse finally was bought by Siemens.) His name is R. Bodo. Well, the name of the team members[32] you better collect

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[31] He was appointed professor of low frequency techniques in Vienna. His primary field was acoustic, see:
E. Skudrzyk: Die Grundlagen der Akustik. Springer Verlag, Wien 1954; 1084pp.
E. Skudrzyk: The Foundations of Acoustics. Springer Verlag, Wien 1971; 790 pp.

[32] The list of the MAILUFTERL TEAM:
K. Bandat -- Core Store
R. Bodo -- Layout/architecture
V. Kudielka -- Drum store
K. Walk -- Transistor Physics
P. Lucas -- Programming (joined later)
E. Muhldorf -- Tape reader (part time)
The attached fields were main activities -- every one worked on everything when it was necessary.

from the documents. Then you have them in written form.

ASPRAY: Yes. Before you go on, can you tell me about what you knew from other centers about things like their drums and cores and such?

ZEMANEK: Yes. I was clear that I had to use all the sources of information, and that literature alone was not sufficient. So in that period around 1954 I gave up my activities with the Boy Scouts. I started my travelling for electronics, or computing, whatever you want to call it. My first trip was in the fall of 1954 to Olivetti, and the second, in the same month only a fortnight later, to Siemens in Munich. And of course, I also saw the PERM development, the Munich computer. I knew about Gottingen. I knew about Zuse, probably had met him already. I had a lot of American literature. I knew about the Dutch developments. I knew a little bit about the Danish developments. That, I would say, was about the context in which I operated.

ASPRAY: Did you know, in particular, about the Engineering Research Associates work on drums?

ZEMANEK: Not in particular, but from the literature I was familiar with that enterprise, with that group of people.

ASPRAY: Because you'd said that your first student who knew one of these areas was in the drum area.

ZEMANEK: Oh, we simply conceived a magnetic drum. You see, magnetic tapes were available. And we found out that you could get from BASF or other chemical enterprises in Germany, the tape substance as a liquid.

ASPRAY: To spray on?

ZEMANEK: To spray on the drum. We didn't need any ERA development. We simply conceived a rotating cylinder of metal to spray it on and we tried to develop a head to write, to read the circuitry to select the address. We used a step-wise approach. We started by merely having a round element of metal and sprayed the magnetic substance on it, and tried a first kind of head.

ASPRAY: You received that spray from one of the companies in Germany?

ZEMANEK: They were commercially available, maybe not directly from the shop. You had to ask and they would send it. For the university, they had an open ear. I think we got it either from BASF in Germany or from a similar company. Whoever was ready to deliver, we knocked on the doors and asked questions, and one or the other was ready to deliver. (The same was true, by the way, for the magnetic cores.) We had to develop a particular plastic matter to seal in the heads of the drums. We made experiments in that direction. We were successful. The next problem there was to find a refrigerator to maintain the liquid so that it didn't get hard before the core was finished. The company of my father gave us that refrigerator. Altogether, I needed somebody with the mechanical experience to construct the drum. I found a company in Vienna that specialized in gliding bearings. The hope was that we would need no oiling, no liquids to run it. It turned out that we needed one. They were all underestimating the accuracy of .05 millimeters for the heads. But anyway, produced a very nice design. I got ELIN, one of the big local Austrian electrical companies to produce them. So they were made in Weit (Styria) and the general manager of ELIN brought it in his Mercedes to the Institute. We got two, by the way, because if something happened to the one, we needed a replacement. So I said immediately, "Please make me two." I got them, and we got them going. In order to get the drum operational, we had to bring the pulses on one track without an unequal time distance at the completing point. Therefore, we had to regulate our frequency generator in the right way and to watch on the oscilloscope. When the whole picture stands, you know you have got it. Then you derive for the other tracks. So one track always was the basic clock for the whole machine. That was one of the ideas. We were not going to stabilize the drum from some fantastic source, but we were going to regulate the whole operation of the engine from the drum, from that clock track on the drum. We made a subdivision, so that we got the beginnings of the words, and then even other subdivisions. That's no problem. Once you have the main signal subdivision and marking in pulses, that's no problem. That's all there. Is that enough as far as the drum is concerned?

ASPRAY: Yes, I think so.

ZEMANEK: The second question was a core store. It is a fact that although the drum looks fast enough, it delays operation. The layout of the drum was such that we had 200 tracks, each track 50 words, and each word 48 active bits, and 4 bits intersection so that one can do switching in between. In such a system, if one looks for certain information and it just passed the reading head, one has to wait until it comes next, which means 20 milliseconds. If you take an average value, it's half of it. It's anyway, 10 milliseconds: a lot of time for a computer that would run about 100 kilocycles. Therefore, we immediately realized we need a small but fast memory too. We aimed at a core store, a core matrix, which would have the same capacity as one track, in other words, 50 times 48 bits which makes 2400 core store elements. Again, we got them as a present; if I recollect properly, from Philips. My joke, in this context, is: they were selected, but unfortunately, they were selected by the others and we got the remainder, the tails of the distribution. So we had to make an invention. The invention for which we got a patent (and this patent was also used by others, namely, by the Zuse company, who even paid patent royalties to my inventor) was the following. Normally, the general principle, as developed by Forrester and Rajchman, was to set all the ones from zero to one and when reading, to let the ones jump back from one to zero. We made, when reading, jump the remaining cores (those storing zeros) from zero to one, yielding a negated sequence -- easy to negate again. It turned out that the signal-noise ratio was much better in this case. That was the content of the patent. Do you understand? You write in like always, but reading out, you don't throw them back, you throw the remainder, those which have not yet been switched, also to one, which gives you a reading signal which is the inverse, the negation of the content, which is no problem for the electronic circuitry to restore the correct form. That works fine. Now, for the logical circuits, it was clear that are realized by diodes, of course semi-conductor diodes: the Ands and the Ors. The negation must be resolved actively because it is necessary to replace a zero by a one from the general pulse source.

The capital question was how to get the necessary number of transistors? It was clear at the time that no other company than Phillips could be the source for the transistors. It was, furthermore, clear that Philips was not yet in the production of fast transistors. What they could offer were relatively slow transistors intended for hearing aids, where no high cutoff frequency is necessary. This fact explains the name of my computer, MAILUFTERL. It comes from a joke which is documented, so you can read the original wording.[33] I was saying at the time, "We are going to build a transistorized computer in Vienna. But since I am going to have to use hearing aids because I depend on the gifts, it will be a slow one. In other words, it will not be a typhoon, whirlwind, or hurricane, but a nice Viennese spring breeze would be sufficient for our purposes." I said "MAILUFTERL" in German. And then the German colleague said, "That's a fantastic name. You should keep it." Against the resistance of many very serious people (my prime minister included, who has a negative remark about my naming it in his memoirs)[34] I kept the name and, meanwhile, it's tradition. By the way, the other day I discovered a letter which I had to answer where somebody complained with The Rector of the Vienna University about this naming. He thought I had not considered it and I could not give a solid defense of my naming. If you're reading today, where even in America it is known MAILUFTERL, the whole thing is only funny.

Well, actually, it was not so slow. It was not really a MAILUFTERL because we made the following development, an idea[35] of Dr. Walk, my successor in the management of the Vienna Laboratory. The deal was the following. You know that each transistor causes a certain delay; it does not react immediately. It needs some time to react to the input. Now we turned it around. We used the delay for storage purposes. So a pulse would go into one transistor. That transistor would take a certain time to react, which is enough to hand it over to the next. So we subdivided a normal basic time unit into 4 parts. Four transistors would play like with a golf ball, you know, left and right, like a tennis ball. By this, we got a very fast and reliable working of the circuits. Most of the circuits we developed would have gone up to 250 kilocycles. But not all of them, and generally for security, for reliability of the operation, we agreed to stay with 133,000 steps per second -- four times as much, if you count this "ball game", but this is not the real frequency because each transistor acts also 133,000 times a second. Did you hear about this?

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[33] H. Zemanek [list of works of Heinz Zemanek in "Formal Models in Programming" (E.J. Neuhold, G. Chroust, Eds.), Amsterdam 1985] [59] (1956)

[34] Josef Klaus: Macht und Ohnmacht in Osterreich. Konfrontationen und Versuche. Molden, Wien 1971, p. 195-196

[35] K. Walk: Die Ausnutzung spezifischer Halbleitereigenschaften beim Entworf der Volltransistor-Rechenmaschine "Mailufterl". Solid State Physics in Electronics and communications. Academic Press, London 1960, p. 924-938.

ASPRAY: Yes.

ZEMANEK: Also there we got a patent, but that one was only used in the MAILUFTERL.[36] We never got royalties for this one.

For input and output, it was very clear that -- having no money, having no connection to any punch card or similar equipment -- the natural solution for us was teletype. So we started by attaching a teletypewriter to the computer. The basic reading programs which enabled the MAILUFTERL to accept any input had to be written bit by bit, so to say. The bit chains were transformed into the teletype alphabet. Then the stochastic text that came out had to be punched on paper tape. Then we fed this program into the storage of the computer. From then on, it could understand. Mr. Muhldorf of our group developed a very fast tape reader, also fully transistorized. We removed the cover of some of the transistors, and transistors are light sensitive. Every one is. By the means of glass rods we sent strong light. Then the transistors would discover whether there was a hole or not in the paper tape. Unfortunately, the construction was done a little hastily and it never got stable. So Oktavio, as this device was called, never really got used on the computer. But we had relatively good and fast teletype equipment, punchers and readers and we never had very big problems with I/O. Moreover, the machine was never intended to be I/O intensive. So a slow I/O would not be of big harm for our project. I think I have covered all of it.

We finally added, also, fully transistorized, really fully semiconductor power units. The problem was the high current which was needed. Even if the energy consumption is low (the MAILUFTERL consumed about 400 watts) if one divides by 6 or 12 volts, one still get considerable current. Therefore, we needed power units that handle 10 or 15 amps, which requires a regulating transistor of that current. What I found out was that General Motors was aiming at electronic ignition already at that time. I discovered that I could get such power transistors, but only in Paris. After

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[36] K. Bandat: Zur Storreduktion in Ferritkern-Matrizen-Speichern. eR 2 (1960), p. 177-182.
K. Bandat, H. Zemanek: Anordnung zur Speicherung digitaler Informationen. Patentschrift 222 921, K1. 42 m6/10, Wien, 27. August 1962.

telephone calls and correspondence, I went to Paris, visited the representative, paid cash for those transistors, put them in my pocket, and smuggled them into Austria.

ASPRAY: I see.

ZEMANEK: We ended up with a fully transistorized power unit set.

ASPRAY: No vacuum tubes whatsoever?

ZEMANEK: No, not even stabilizing or anything. You remember what a Zener diode is? It's a stabilizing unit which helps to retain current or tension. The MAILUFTERL: if I say it's one of the earliest fully transistorized computers, I am certainly right in the sense that it was purely and completely semiconductorized.

ASPRAY: Yes. Now this was completed in what year?

ZEMANEK: The construction work went on from 1956 to 1958, and being the MAILUFTERL, it had to be in both cases May 1956 and May 1958; but it is almost correct.

ASPRAY: Could you make a few remarks about the applications of this machine? You said that it was not supposed to be I/O intensive, what you had in mind for it. What did it get used for?

ZEMANEK: We wanted to prove, generally, that we had built a computer, so it would have to be able to compute. That, of course, brought us into programming languages and compilers. Originally, I wanted to go the FORTRAN way, because...

TAPE 2/SIDE 2

ASPRAY: You wanted to go the FORTRAN way because...

ZEMANEK: I wanted to go the FORTRAN way because I realized that in practical applications, in industry, this was to be the language. COBOL, as you know, was not yet around, so that was not a question. It would be too far away. I didn't want to go a bookkeeping way. But unfortunately, IBM was very silent about helping me with the structure of the compiler,[37] while Bauer in Munich said, "You get ALGOL, that's no problem." So first of all, let me explain to you the direction we wanted to go in the writing of an ALGOL compiler. We joined the ALGOL enterprise at a time before ALGOL 60 was finished. Actually, the first German publication on ALGOL, on ALGOL 58 as it was called afterwards, is written by me.[38] I knew already the from my visits the different people important in Germany in the field of programming languages, and we discussed what German expressions would have to stay for the English terms in German papers. I got a consensus from the German-speaking community, and I published this in the form of a paper.[38] Bauer had promised us that we'd get documents which make it trivial to write a compiler -- which, of course, was a wrong assumption. And fortunate, I would say, it was too, because by this way, my next guy, the programmer of the team, Mr. Lucas, really had to dig deep into the art of programming languages and compiler writing. He developed special principles and we ended up by having not only an ALGOL compiler, but also a (smaller) LOGALGOL compiler.[39] Because of the reason I will explain immediately, we wanted to have the MAILUFTERL working on logical problems. So we designed a kind of extension of ALGOL for bit chains, for sequences of yes-no decisions as such. We called it LOGALGOL. The key issue is the following. In normal ALGOL operation, in order to store a yes-no decision, say a condition or something like that, any logical value, one has to use a full storage cell for one bit -- no other way. It's treated like the numerical values, zero and one.

[37] Actually, IBM Austria never answered any letter I sent during the MAILUFTERL development time; we received no support what so ever. But domestic IBM more than compensated for it when the transition to the laboratory was implemented -- in first line, by the way, in which the MAILUFTERL was bought.

[38] H. Zemanek [list of works of Heinz Zemanek in "Formal Models in Programming" (E.J. Neuhold, G. Chroust, Eds.), Amsterdam 1985] [89] (1959)

[39] Peter Lucas: Requirements on a Language for Logical Data Processing. In: Proc. IFIP Congress Munich 1962 (C.M. Popplewell, Ed.) North Holland, Amsterdam 1963, p. 556-560.

ASPRAY: Yes.

ZEMANEK: Working on switching circuitry minimization, one can't afford to do this, because then a chain of 16 bits requires 16 addresses. What is wanted is to make use of the full word capacity for bits. So we decided to have a language with a transition operator which transforms a bit sequence into an entire decimal number and stores it that way. So one can go either way. make all your logical decisions in the processes bit by bit. But in the storage, you have already transformed it, say over the core store, in fully used storage values. And therefore, you can house all the bits you require, which is about, I forget now, but it should be some 500,000 bits, except the program, of course, which you have also to maintain. You can store in all the remainder your operational bits for the switching problem, which we did. Minimization of Boolean functions was, in fact, an important field of application of the computer. Mr. Kudielka has written a doctoral thesis applying a particular algorithm twice on minimization. He had been the guy working on the drum, but then he went into switching. The thesis is based on the Quine principle,[40] but it goes quite a step further. That is detail I'm not going into at this time.

We also programmed all the cybernetic models in order to show what switching really means in the sense of other application on the computer.[40] But the first giant application program was in the field of musical theory. I had the visit of a Viennese twelve sound composer who had a theoretical problem, rather for knowledge purposes than for real composing. A twelve sound series can either be constructed by using each *key* of an octave once, or else one

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[40] Quine -- see:
V.W. Quine: The Problem of Simplifying Truth functions. Am. Math. Monthly 59 (1952) 521-531.
V.W. Quine: A Way to Simplify Truth Functions. Am. Math. Monthly 62 (1955) 627-631.
Dissertation Kudielka -- see:
V. Kudielka: Ein Verfahren zur Ermittlung aller nicht redundanten zweistufigen Darstellungen einer logischen Funktion. eR 5(1963), p. 11-21.
Learning Programs -- see:
V. Kudielka: Selforganizing Grouping -- A Learning Structure. In: Proc. IFIP Congress Munich 1962 (C.M. Popplewell, Ed.) North Holland, Amsterdam 1963, p. 419-422.
R. Eier, H. Zemanek: Automatische Orientierung im Labyrinth. eR 2 (1960), p. 23-31.
A. Angyan et al.: A Model for Neurophysical Functions. In: Fourth London Symposium on Information Theory (C. Cherry, Ed.) Butterworth, London 1961, p. 270-284.
H. Zemanek: Lernende Automaten. In: Taschenbuch der Nachrichtenverarbeitung (K. Steinbuch Hrsg.) Springer Verlag. Berlin 1962, p. 1418-1480.

can do the same by using each *interval* once. It can be shown that both construction principles make sense. His question was, "How many series exist which fulfill *both* conditions?" In other words, what was to be computed were all sequences of the twelve sounds where each interval occurs only once. The question is, how many are there and what do they look like? He came already with a kind of program, not an elaborated program, but a logical idea for it. He had also computed one block. We transformed his idea into a real Mailufterl program, and we started by doing the first block. To our satisfaction, there were, like on his sheet, thirteen results. Only, it turned out after the first glory that there were mistakes in it. Of course, it was not machine mistakes. Doing it by hand, he was bound to make mistakes: he had one series twice and didn't see it. And he lacked one and he didn't see it. So, in a run of 60 hours, the machine established the number of such series as two times 1928. That was published[41] and since then it belongs to the body of knowledge of the twelve sound composers. The second practical project was a real computational one. My friends of the high frequency department had accepted a project with the question, "How do you feed high frequency energy into a power transmission line, (in a frequency range which happened to be the same as the one which the commercial air traffic is using for the board-to-board communication so that a minimum of the energy sent radiates away?)" They had, in other words, an interest that the power line would not work too much as an antenna. The question was, "Can we adapt the input into the line in such a way that the minimum radiation goes off the line?" They had hoped they would come out with a formula describing the solution, but as it was the trend of the time it turned out only numerical calculation could give the answer. So they were very, very happy that the computer was around. We delivered all the tables they needed, by the computer in a thirty hours program. This is the bulk of the early application. We then went deeply into languages. We moved the computer into the IBM Laboratory. We did numbers of computations for IBM and studies of compiling and of looking into languages, and so on.

ASPRAY: The machine was active for what period of time? That is, how long was it in operation?

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[41] The composer was Hanns Jelinek (twelve letters!) -- see:

H. Zemanek: die Arbeiten an elektronischen Rechenmaschinen und Informationsbearbeitungsmaschinen am Institut fur Niederfrequenztechnik der Technischen Hochschule Wien. Nachrichtentechnische Fachberichte 4 (1956), p. 56-59.

ZEMANEK: From 1958 into the 1960s. I would have to look up what the number days of application was. The number of operating hours, if I am right, is somewhere between 10,000 and 20,000.[42] We gave it up, not because the transistorized part was making problems. It was the drum that had some weakness, which made it, after a couple of thousand hours, to go into vibrations, that killed the surface. Now that was possible to correct because we took the pistol and made another layer, got the clock track on and started again. It was not a comfortable type of work and one day we said, "IBM doesn't like this off-the-track engine anyhow. Why should we do it again? And we stopped the operation.

Let me make one final remark about the layout of the machine. First of all, it was my aim to produce a machine which would work in the decimal system, and not merely binary. Not because I was insisting in decimal: I knew the value of the binary system. But we wanted to make a non-trivial step, and we found building a decimal machine was of interest. Then we discovered that by altering only minor details of the construction, we could go either way. We established a flag bit which would tell whether the number or the instruction was meant decimally or binary. The MAILUFTERL operated both ways. Where we made a mistake was that we organized the *drum* decimally. In particular for the switching problems I mentioned, where we used 2^n bit sequences, it would have been much better to have a binary organized memory. Because otherwise very clumsy address computations are necessary and the storage is used inefficiently. That's number one: a decimal machine.

Number two is we made the instruction code extremely flexible. It is called minima principle or Freiburg code. (It has a number of names.) The idea is the following. First of all, we made use of flag bits. Secondly, each instruction could be made, depending on one of the conditions. Since there is a four-bit group, fifteen different conditions could be built in. Then there were fifteen main instructions and seven auxiliary or secondary instructions which could be combined (fifteen and seven because one is always the zero case so that all the needed freedoms remain). Then there were nine bits, each of which had a function, called, therefore, functional bits. That's an idea parallel to microprogramming. I think it comes from van der Poel in Holland when he designed his ZEBRA machine and the

[42] For the Operating hours see HZ "Zeittafel" in the MAILUFTERL retrospective paper. At the university, it completed 6100 hours, until it was stored (1966) to become a museum exhibit (1975), it may have come to 12,000... 15,000 hours.

minimum principle. The point is that a bit is set or not, depending on whether a certain function shall be carried out or not. For instance, one bit was the decision whether the address was meant on the drum or elsewhere; "elsewhere" meaning we had fifty addresses over the machine, so that most of the points in the machine were accessible by an address. Then we had a relay input-output because we also worked on relay circuitry directly. The machine would analyze what happened in the relay circuit. That meant another set of fifty addresses. And for the remainder, we could have extended the core-store from the fifty addresses to 10,000 minus the ones used in the machine. Other functional bits concerned, for instance, subtracting one when multiplying or adding one when dividing (in order to work off the decimal places); when it becomes zero, the shift is triggered. This could be done simply by a functional bit. Multiplication, for instance, was programmed in the machine; but it was only a five instruction program to carry out a full decimal multiplication. There were many other very powerful and very smart and intelligent ways to program. Only: one day I realized that the philosophy was not as strong as I had thought, because if you consider a machine that runs five times as fast with the standard code, you have, of course, the possibility of combining five standard code instructions, and that is probably more powerful than all the very intelligent MAILUFTERL code. So everything is relative. But it was a big experience.

ASPRAY: What was the typical mean-time between errors, between machine failures?

ZEMANEK: There were none.

ASPRAY: There were none?

ZEMANEK: No.

ASPRAY: So you didn't have problems with long programs running and that sort of thing?

ZEMANEK: Well, yes. There were hundreds of reasons why something might go wrong. In the beginning, maybe we had problems with the power supply and so on. But it was not so often that I could establish the statistics. But in the completed machine, no hardware failures occurred.

ASPRAY: I see.

ZEMANEK: For the long programs, we had a means of attaching the machine to the telephone. So in the night, say at ten, before they go to sleep, my collaborators could call and listen to whether the machine was operational or whether there was a hang-up. The hang-up normally came from programming errors.

ASPRAY: Yes. I understand that.

ZEMANEK: At least, it was not necessarily a hardware mistake. When he heard the sound, he could sometimes guess from the melody in which part of the program the machine was just running. Or if it was a constant sound, he knew there was a hangup. He would take a taxi, go to the Institute, start the machine again, and go home and sleep.

We tried also to make a chess program, but the girl was too keen to do too much so she achieved nothing. But you see at the museum, there is a chess board and in principle one could write the necessary programs. We learned a lot. As you see, my group and I went from the hardware construction to general programming and to programming languages. From there, it was only a short step to go to the next section of my life. That was formal definition.

ASPRAY: Let's take a break for a minute.

[INTERRUPTION]

ZEMANEK: Now the formal definition was, of course, a real enterprise within the framework of IBM. At that time, we had already moved to IBM. Let me make a step backward and tell you how the interconnection to IBM was established. That goes back to my assisting to congresses. I met Professor Ganzhorn, the latest... maybe before...

but the latest in 1955 at the famous Darmstadt conference[43] which was the first one, outside England (and maybe there was one in France), with international participation. The Dutch were coming. The first Russians came to give talk, but they came too late: they had to extend the event for a day in order to house the Russians because they came when the meeting was closed. You'd have to look up the participants.[44] It's really a set of famous names. Then, there was an important conference in Madrid in 1958. It was on automation, but half of the papers actually are European computing papers.[45] The key event was, of course, 1959, the first really international conference, the forerunner of the IFIP Congresses. We actually count it as IFIP Congress number one although IFIP was founded a year later, but essentially by the same people who organized the Congress. The patron, or the honorary president, was Aiken. Ike Auerbach was the driving motor in all the stories you find: in Auerbach's paper,[46] in my book, as well as somewhere in AFIPS environments; that's all known. At this conference, I was also presented by Ganzhorn the first time to Dr. E. Piore, the chief scientist of IBM. He was not visible for my laboratory, he never came to Vienna, but he was my protector as long as he was in office. Another protector of the Vienna Laboratory was Dr. Marvin Kelly, former director of the Bell Laboratories and advisor to Mr. Watson in IBM. He was of big help. We had a good understanding. He was also a telecommunication engineer so there was a resonance in this sense. He held his hands over me for many, many years.

I had connections to IBM. I came to realize that the MAILUFTERL team could not be kept together on university grounds, simply because the University would not offer enough positions for all my assistants. Moreover, the whole

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[43] GAMM/NTG Conference in October 1955, see:
H. Zemanek: Die Arbeiten an elektronischen Rechenmaschinen und Informationsbearbeitungsmaschinen am Institut fur Niederfrequenztechnik der Technischen Hochschule Wien. Nachrichtentechnische Fachberichte 4 (1956), p. 56-59.

[44] at the GAMM/NTG Conference 1955 there were speakers from Germany, Austria, Belgium, DDR, England, Netherlands, Sweden, Switzerland, USA and USSR.
Moreover, there were participants from many more countries, e.g. CSSR, Denmark, Finland, France, Hungary, Italy, Norway, Poland, Spain, Yugoslavia and Canada.

[45] Congreso Internacional de Automatica, Madrid, 13 - 18 October 1958

[46] see H. Zemanek [list of works of Heinz Zemanek in "Formal Models in Programming" (E.J. Neuhold, G. Chroust, Eds.), Amsterdam 1985] [254] (1986) pp.41 - 94.

scale of my operation was slowly growing out of university dimensions; at least at that time. Thirdly, in the last years the new boss was appointed, so there was again a professor. I was not any more my own boss. I had again a boss and he felt uncomfortable with that giant enterprise under his hands. There are other stories in between. Once, the official curator of the empty chair tried to stop the MAILUFTERL operation, and I had to make it clear that he would have to write the stopping letters to all the people who had given me support. He didn't feel he was ready to write those letters and, therefore, the project went on. That reminds me of a point that I haven't made, which I should have; namely, from where did I get the money? Over my Boy Scouts relationships, I got a way to the most important banker[47] in Vienna of that time, presented him my project, and asked for support. Now the bank itself did not do it, but there is a banker's association in Vienna. They gave me, first, 100,000 schillings and I think altogether 250,000 schillings so that I could pay my people, except Dr. Bodo, who was paid by Zuse and went there. So much to the monies I have spent. Returning now to IBM. So Dr. Piore had seen me and when...

ASPRAY: So the upshot of this was that instead of... You were uncomfortable at the University so you moved to IBM. At what time did this happen?

ZEMANEK: Well, as I said, in 1959 I was introduced to Dr. Piore, IBM's Chief Scientist. I could save my group over two years (1959 to 1961) by contracts with the European Research Office of the American Army. I got something in the neighborhood of $8,000 per year. That saved us for these years. But it was clear that the situation was not stable. So I started to investigate which companies could house my group. At the same time IBM had gotten interested in my group and, therefore, the interconnection grew into two directions. If you listen to IBM people, they would say they called me. If I describe the history, I would say I selected IBM. To a certain degree both descriptions are true. I had a chance of going into Siemens, and in some ways I would have preferred Siemens because of the character of the company and of the German speaking possibilities. On the other hand, not only that IBM is bigger and richer (that did not count so much for me), but the fact that connection to IBM would mean a final insertion into the American stream of knowledge and development I think was the essential reason to favor the IBM connection.

[47] Director General Josef Joham

Moreover, Siemens had reacted finally by saying, "In Munich, yes; in Vienna, no." IBM was willing to accept us as a group, to open a laboratory that did not exist before, just for me and my people. They gave me a contract for the year 1960-61. It was finally agreed that the move date would be the first of September 1961. So actually, we transported the MAILUFTERL from the university to the IBM Laboratory, to the new premises, over the weekend, around the first of September 1961. And practically without interruption, work continued on an IBM basis. I could hire a number of people in addition. Altogether, the laboratory during my times went up from the five people I had on the MAILUFTERL crew to fifty people. Today they are rather approaching 100. We started as the Science Group Vienna, a subdivision of the German laboratory until... Well, the date is not a "constant of nature". We had arguments and there are different answers. But around 1964 it became certainly an independent European laboratory of IBM. The first product which went into fabrication was the vocoder (the vocoder group -- with Dr. Rothauser[23] -- I also took with me into the IBM Laboratory). We developed a feasibility model. Then it was further developed in Germany, and later on in France. It was produced in Kingston (NY). That was the voice answer system 7772, within the system 360. One of the weak points the final product had was it required a special mode of the operating system, which is of course wrong for a voice answer system. There should have been three plugs. One for the power supply, one to the machine, and one to the telephone. IBM is very special in this respect. I had no access to the product development. When they removed the vocoder from Vienna, it was out of my grip. Moreover, for instance, Dr. Tauthauser was even not allowed to continue his work. He finally moved to Zurich Laboratory and the whole acoustic development was stopped. So that was the first end of the operations. We, having recognized the importance of programming languages, were used as experts in ALGOL because we knew so much about it. On the other hand, we learned everything necessary about FORTRAN. Then in 1964, for the first time, we got the description of the proposed new programming language, NPL, subsequently PL/1. We immediately produced a critique of the language, a document which I could even show you; I still have a copy of it. This made us go to the management and explain to them that in our opinion, this new language required formal definition. Let me give two sentences of the philosophical and technical background to this. The difference between FORTRAN and ALGOL can be described in many different dimensions. One is, I like to say, if you send ten industrial programmers into a summer resort and request them to design a language, the result would be something like FORTRAN. If you send ten university professors with the same goal into the same place, the result would be very ALGOL-like. But the main point I have to make in this context is that FORTRAN was compiler-defined while ALGOL was document-defined. In other words, when you had a problem, in the case of ALGOL, you went to the document and looked at what the definition says, and that you have to fulfill with the compiler. In the case of FORTRAN, when you had an argument, you would have to look at what the compiler answered and the compiler was right. Everything you wrote down had to conform with what the compiler did. Now with the event of the System/360 this principle didn't work anymore because although compatibility was granted or was on the paper, practically you would get very different answers from different-sized installations and configurations. Therefore, we convinced management that a formal definition was the only way to survive. They really gave us the instruction, "Yes, go ahead. Produce a formal definition of PL/I." Our proposition in addition was not only (as in the case of the ALGOL documents) for the syntax of the language, but we propose to define the semantics, which would be the much bigger work. The second part of the agreement was we take no responsibility whatever for the shape, for the form, for the content of PL/I. We would formally define what 'language authority' defines to be PL/I. Now this includes contradictions because a prose, the living language of English, description of a language cannot be logically precise. It cannot be expected to be contradiction-free. So the mechanism agreed upon was the following. We will develop the method (and I will come back to this). The expected discrepancies that will develop will be organized in the following way. When we discover something that we cannot formally define because it's incomplete, contradictory, or whatever, we will write a document called a 'language point' describing the difficulty we have found, sending it back to the 'language authority', let them decide what they want, and then we continue the formal definition.

TAPE 3/SIDE 1

ZEMANEK: Now to the methodology for the Formal Definition. I was puzzled by this question already earlier, much before we made this proposition in 1964. But we could make the proposition in 1964 because meanwhile, in the framework of IFIP, I had proposed the very first IFIP working conference, which got the title "Formal Language Definition Languages".[48] This working conference was the key tool to entering the field. Not only did we get twenty

[48] T.B. Steel Jr. (Ed.): Formal Language Definition Languages for Computer Programming. Proc. of the (very) First

really pertinent papers, we got almost all the invited people working in the field as participants to the conference. I used my laboratory people as staff for the organization. For instance, my people were running around with the portable tape recorder in the discussions saying the name of the discussant before he could start his sentence into the microphone. So there was never a question of whether somebody had mentioned his name or not, which forced my people to know everyone. Also, they were always present and altogether they knew, really, every paper of the conference. Tom Steel, at that time my vice chairman and later on my successor at the chairmanship of TC2, got a composite tape, namely we copied all the movable tapes on the master tape. Then the Rand Corporation transcribed all discussions. For the papers, that was not necessary; the papers we accepted as the author had redefined them. Discussions, however, were reproduced from the tapes.

We had essentially the following authors' fundamental ideas for the formal definition: there was John McCarthy; I would also mention Peter Landin, who had pertinent ideas; and C.C. Elgot of IBM Research in Yorktown with his model for the Random Access Machine... RASP, I think it was called. You find all of that in the different documentations; as well in this conference volume as in Walk and Lucas's paper on the formal definition,[49] or in Wegner's[49] book. Wherever you look you will find it, because we always very clearly said where from we had gained the ideas. All the other contributors were of importance, whether you think of van Wijngaarden or Nat Rochester or whoever was around. Then we sat down and really tried to do it. We developed what is called the 'theory of abstract objects'. An abstract object is something that you can, for instance, show as a tree. But you could also do it as rectangles within rectangles, and so on. The essential point is that each interconnecting line from one load to the next has a name. That's the selector, and the way that you come to a point in a tree is a sequence of selectors. It's the combination of object and selecting mechanism that makes up the mechanism by which we designed the formal definition. The idea is an abstract machine of the language you define. The meaning of the program is defined by the

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IFIP Working Conference in Baden near Vienna 1964. North Holland, Amsterdam 1966; 330 pp.

[49] See K. Walk and P. Lucas. See also:
D. Bjorner, C.B. Jones (Eds.): The Vienna Definition Method -- The Metalanguage. Lecture Notes in Computer Sciences Volume 61 Springer, Berlin 1978; 382 pp.
P. Wegner: The Vienna Definition Language. ACM Computing Surveys 4 (March 1972)
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behavior of this machine. Mind you, you don't have the meaning as a text. The meaning appears to the human mind by looking at what happens when a text is applied to the structure of the machine. It's one step more complicated than you think on the first moment. IBM management was deeply disappointed because they wanted the following: they wanted you to come in with a piece of text and the Vienna Laboratory to produce an engine, meaning a program, so that you insert the text into the machine and on the output you get the meaning. That does not work. They were deeply disappointed. They thought you had the meaning machine, which produces the meaning in a written form. This is impossible. Meaning is much more on the mental side and meaning actually makes sense only together with the human mind's reaction. It's depicted here. The problems are very close to Goedel's or Turing's 'undecidability'; all language work and logic considerations from Pierce, Carnap to Chomsky. I had to study all of this. This is how I came into the philosophy of Wittgenstein, in order to make sure that what we are doing was also academically and intellectually on safe grounds. Probably my collaborators underestimated a little my contribution, which was not technical. I left all the definition work to them. But as far as the lines are concerned, I have done much more than they even realize today, I think, immodest as I am. But I think I am right. It was to our astonishment that we could furnish practically a full definition of PL/I with only one method. What we had to do was three different things at once: namely design the method, in other words, invent the abstract objects with all the side conditions. You know that the difficulty with defining a language sometimes is not with what you define, but what you do not define. Because a language never defines everything. Start from the simple concept of addition. You do not formally define what the plus sign means, and you can't. You have to leave free a lot to the compiler designer, how he decides. So a formal definition is not allowed to include an algorithm that selects an arbitrary definition which is not within the wording of the language document. It turns out that in some cases you rather have to build walls around an area than define the area. Do you see the idea of my picture?

ASPRAY: Yes. I do indeed.

ZEMANEK: Fine. So this was one of the problems. We had to introduce implicit definitions in some cases. That was not easy. The second thing was to really define PL/I. I remind you of the 2000 language points. We had a big battle, for instance, about recursivity in PL/I. The people who really did it had no mathematical training. But we had difficulties. We finally cleaned it up totally. We cleaned many other points. Let me describe this in pointing out the third problem we had. You have to produce everything you work out as a document. Now it's a highly critical text because it's logical text, and every bit counts. So we had to do something to avoid printing and copying errors. Therefore, we also developed an editing system, which would bring a formula automatically in a defined shape, mainly by indenting properly. That not only yields a style; it gives a number of advantages. What the system does is it accepts the text. It must know about the grammar. In other words, it must somehow store the grammar of the description language and must be able to work within the structure of the grammar so that it gets its own area of freedom, in the final printout, to accept the parameters for the printout, write the correct text in a good shape. You see the interconnection between those conditions. This was also done. It was also a very big enterprise. The description of this system is again a thick book. And the formal definition, as such, has almost 1500 pages. So it's quite a thing. What the volume offers, among other things, is a list of the identifiers being in use in the description, which has the following advantage. To each identifier, the editing systems works, underlined, the place where the concept is defined. It also lists all the occurrences. Where is a defined concept being used in the definition? So it is easy to discover what is being used and was forgotten to define. All those cases can be repaired. Then you find out you have defined several things which were never used, so you can easily strike them out. Then you discover that you have defined things twice which can mean two different things. Perhaps it comes from teamwork: two people may have had the same idea and each of them found it necessary to define the thing, so each of them wrote a definition; and of course they need not be equal. On the other hand, engineers and scientists are not necessarily very creative in naming. So it may be two different concepts but, with a lack of imagination, they choose the same name for both. So all of these shortcomings can be simply found out from the list of occurrences. This is one of the advantages of formal definition. Now, I am not to give here a publicity speech for formal definition. But I want to add in closing this whole subject, a philosophical remark. You may ask technical questions in addition.

I think that we did not find for what we have done there the recognition and the compensation that we have deserved. But this is not a complaint against IBM or certain people. I think this has been fate, and the reason is the following. Formal definition is an ideal, but as you know it is always difficult in this world to realize ideals. It would have required, first of all, the education of all (at least: of many) IBM programmers in this field, and we were much too early for this. There were not yet any graduates of informatics available, so people couldn't easily read our text. They didn't have the logical background. When we came to Poughkeepsie and discussed with the manager of education the introduction of our methodology and of the formal definition of PL/I, it turned out it was hopeless to introduce it -- already from a cost point of view. The time and the cost for the time to educate 2000 programmers in this direction would be hopeless. It's not done with it, because if you really want to apply formal definition, all the customers... all the users would have to have the same education. So in one way the whole thing is impossible, but at least we were too much ahead of time for the realization. But one may also say that the whole field of computing, for pragmatic reasons, left the well-defined grounds and decided to go on undefined grounds, of which we are going to suffer for the next 100 years. I promise you.

ASPRAY: Even if this had been adopted, there also would have been the problem of it being much more costly in programming time to use the formal definition, wouldn't it?

ZEMANEK: That is, of course, an economic consideration. If you take, on the other hand, the money spent for correcting all the mistakes which are made because the programs have not been formally defined, you might come to a break-even point in favor of the formal definition. I will tell you another story. I always was looking out for a key example to explain to management the merits and the justification for formal definition. I wanted to have a simple example which would convince them. After preaching this message for a while, I really found an example. That was military exercising. If you think it over, military exercising is a binary, digital structure. Because every step you can think of being made is made on a digital field, on a squared field. So then my request to my people was, "Show management for such a simple thing as military exercising, which is learned by the most primitive guy drafted for the army, how impressive the formal definition looks." They never did; maybe for good reasons, maybe for bad ones. I'm not judging at this point. When I became an IBM Fellow, I finally had somebody who had to do nothing but to follow my orders; only one, but to him I could really request such a thing. I said, "Sit down. Define military exercising." Which he did; he came back with one page of lines of code. I looked it through and showed him what there was missing. We ended up with, I think, with 380 or more lines of code. This is, of course, exactly the contrary to what I wanted to achieve. It shows how clumsy, how difficult [the method is], and that goes in the direction of your point. What is the disadvantage of formal definition? It's expensive; not only in dollars but whatever dimension you consider. What is the explanation of this? Well, start with a simple gesture. Suppose I'm in an auditorium. I stretch my finger and say, 'you'. Everything is clear. But look at the context. This is nothing like a formal definition. The formal definition would be, 'seat number 28 in row number 17' -- much longer, much more abstract, much more error-bound than if I stretched a finger. So formal definition, first of all, is a lot of additional effort. From the example of exercising, you learn that you cannot restrict formal definition to what the young draftee is exposed. You have to define not only his movements; you have to define the battlefield, so to say, the ground on which it happens. You have to define the commander. You have to define all the restrictions. If the commander... makes a mistake, the men get a lot of fun, they would run against the wall and make some nonsense. In the formal definition, a mistake is a mistake and it must be removed. One cannot leave any hole. Everything must be covered by lines of code. That makes it so big. Or we can then go into a question of correctness proofs. That is a diverging process, because not only every proof needs a proof that the proof is correct, the effort and the volume of proving, with each step, normally gets larger. One is really in a diverging situation, which will never converge. So my answer was then the third section in my life, the abstract architecture; because if you work on clear principles, you don't have to run so much to formal definition. Practical life is much more based on clear conditions than on formal definition.

ASPRAY: Before you go on to that, can you tell me what the significance of this work has been? What later developments by you or by others have grown out of this?

ZEMANEK: Well, one consequence was that when PL/I was standardized, ANSI produced a PL/I standard document.[49] If you look at it carefully, the Vienna Definition Method is not mentioned, but it's been used. It's essentially our language in which this has been done. So the achievement was really a clear definition. The achievement was the cleaning of PL/I, impossible by the standard methods. They wouldn't have found most of the descriptive answers we have discovered.

ASPRAY: So this could be considered, for example, a methodology for standards committees. It could be one application of this.

ZEMANEK: Well, let me first mention the second point before you accept this as a very optimistic consequence. The problem is that to define after the fact is no pleasure, is a lot of work, is costly. Moreover, in particular in the case of PL/I, the people's reaction was: PL/I cannot be as ugly as it appears in your definition. The fact is, of course, it is as ugly. Let me take default definitions and all the exceptions (they have made for 100 good reasons), as example. They make the whole language such a complicated thing. So that I used to make the joke that PL/I is the language where a description of the subset is longer than the description of the full language.

The ideal application of formal definition would be to use it in the design of the language. In other words, the concepts, everything built into the language, is built in the terms of the metalanguage. This is very difficult, because then very experienced people are needed on both sides: on the side of definition, on the side of how the language should look. My people, in the deception about the acceptance, or the lacking acceptance in IBM of our methodology, said, "The formal definition is very good to define something in existence. It doesn't tell you what to create." That is another indicator why I went to abstract architecture. We would come here in the full problematics of the definition and of the design, of a goal you want to achieve, starting with the specifications -- everything which is common today for software development. You start with specifications and you want to have as efficient algorithms as formal algorithms to make the transition from what you want to achieve to the object itself. But that is full of loopholes and traps and so on. They all reflect, of course, themselves on the language, on the methodology, and so on. We did part of it; we had a system called Vienna Development Method, which is being carried on by my Danish former collaborator, Dines Bjorner, and by my former British collaborator, Cliff Jones. They are continuing the work and they have published a lot about VDM.[50] So it is not gone. On the contrary, I think that in the future the

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[50] see the proceedings of the Brussels conference 1987 -

D. Bjorner et al. (Eds.): VDM '87: Vienna Definition Method -- A Formal Method at Work. Proc. of a Symposium in Brussels March 1987. Lecture Notes in Computer Sciences Volume 252. Springer Berlin 1987; 422 pp.

see also:

D. Bjorner, C.B. Jones (Eds.): The Vienna Definition Method -- The Metalanguage. Lecture Notes in Computer Sciences vol. 61. Springer, Berlin 1978; 382 pp.

C.B. Jones: Software Development -- A Rigorous Approach. Prentice Hall, Englewood Cliffs, NJ 1980; 400 pp.

C.B. Jones: Systematic Software Development Using VDM. Prentice Hall, Englewood Cliffs, NJ 1986; 300 pp.

P. Wegner: The Vienna Definition Language. ACM Computing Surveys 4 (March 1972).

Vienna work will be even more appreciated than in the past. Only it is now of historic character and not any more a direct applicable tool, which we never said it was. We would have expected that IBM would finance the laboratory or maintain the laboratory, first of all, to develop the methodology further; to separate it from PL/I. And to give us an opportunity of developing something using, not only the method, but all the necessary tools. What is needed for software development are, of course, the tools. But we are already discussing my next section in history and that is the theory of design.

DATE: 16 February 1987

ASPRAY: This is a continuation of the interview started two days ago. It is now the 16th of February and we'll go to the next stage in Dr. Zemanek's career.

ZEMANEK: After we had completed the formal definition work, we tried to get some kind of project within IBM where we would develop either a language or a software system applying our knowledge of formal definition and applying formal definition from the very beginning. We were only partly successful. We got one or other of those projects, but none of them really was put through to the end. We got another task. That was to look into parallelism, and my people really investigated all the conditions of parallel operation. I will only quote here the final slogan we derived out of this. When you do parallel operation, you are confronted with the problem that most of the tasks have a varying degree of parallelism in its structure. So for long times, and particularly if you think of array operations of any kind, you might have a high amount of parallelism. But there are moments where all units wait until a certain decision is completed -- normally done by a single processor, and all others are idle. Therefore, the next thing you do is you consider multiprogramming in the hope that, where the one program doesn't make full use of all the goodies you have got, the other program will do so. You end up with a terrible amount of administration. What remained, at that time and I think to a certain degree this is still true today, what you gain by parallelism, you lose by administration. This is a warning signal that doesn't exclude very successful parallel operations. When you have no

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ANSI X 3.53 Programming Language PL/I Standard.

other way to swallow big amounts of number crunching, then you simply have to go parallel. That doesn't exclude it. It was really a philosophical investigation. At the same time, we were looking for further developments. We were clear about the fact that the time of the language had come to an end. I had recognized this already back in 1970 when we prepared for the Lubiana Congress. I wanted Mr. Lucas to give a paper on the application of formal definition methods to programming systems, rather than to languages. But I was too fast. And if you look at this paper,[51] you can recognize the attempt I made through him, but it was not very successful. Actually, one of the California professors[52] came immediately to me and said, "This was the typical missed opportunity." I agreed; so it was.

IBM had lots of management changes and did not make use of the formal knowledge we had put together. We were, simply, too early. Then came a big reorganization. It has to do with an enterprise. I don't know how much there is in your history about a thing IBM internally called Future Systems.

ASPRAY: We've heard very little about it.

ZEMANEK: Well, I am certainly not the man to inform you. The point simply is, after that, they reorganized the company and Mr. Evans was put into a staff position. The Vienna Laboratory remained, more or less, on the distribution field until the end of the discussions. Then we were attached to one of the divisions who had no real interest in us. We were in a dry period and, one day, Professor Ganzhorn came to tell me, "It's the end of the Vienna Laboratory." Now, I am not going into detail. It doesn't matter what the considerations were. The fact was they appointed me IBM Fellow. The Vienna Laboratory first was handed over to IBM Austria, who could not afford a group of 40 or 50 people. I immediately made an economic calculation and said, "In my view, you have to reduce to 15." But then they discovered there was excellent programming manpower and so they moved it back, not to the

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[51] P. Lucas: Formal Definition of Programming Languages and Systems. In: Information Processing 1971. Proc. IFIP Congress 71 (C.V. Freiman, Ed.) North Holland, Amsterdam 1972; Vol. 1, 291 - 297.

[52] Professor M.A. Melkanoff, UCLA, Engrg. Dept.

American divisions, but to the administration in Paris, to the European headquarters of IBM. It ended up as a programming products development center, which it is today. It makes a lot of money for the Austrian company. It's a very important source of income. Otherwise, IBM Austria is a sales company. But here is a production place and you know how it is; the products, the income of the product is booked on the group. They have to stay in black numbers. They shouldn't come into red numbers. So there is some kind of bookkeeping on this; and they were successful. The scientific component in this kind of work is, let me say, very modest. But the group is alive and they're even increasing, as I said. They are right on the way from 50 to 100. I was moved out. Mr. Carey wanted me to be in a laboratory. Vienna had no laboratory, so it meant I had to go away from Vienna. The first goal had been Zurich. Funny enough, nobody undertook any steps to realize this; when I finally had to look into the Zurich software library, I found they had less than I had in Vienna in my own library collection. So I said, "Why don't I stay in Vienna?" We made a working proposal for my fellowship, which was totally accepted with one minor difference, very big for me. I had to go to Boblingen.

ASPRAY: In West Germany?

ZEMANEK: In West Germany, near Stuttgart. So I suddenly was in German exile[53] again. After 2 1/2 years, Mr. Evans intervened successfully, and I was permitted to return to Vienna.[54]

ASPRAY: Still as an IBM fellow?

ZEMANEK: Still as an IBM fellow. As a matter of fact, it is basically a period of 5 years. It was extended for me until retirement.

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[53] I should not, but I call the war period April 1944 to February 1946 my first, and the Fellow period the second 'exile'.

[54] appointed IBM Fellow on 5 April 1976
transferred to Boblingen by 1 November 1976
retransferred to Vienna by 1 June 1979
retired, end of fellowship by 28 February 1985

ASPRAY: Which occurred when?

ZEMANEK: In 1985. So it was nine years of fellowship rather than five. Also, after 5 years, the fellowship usually gets reduced. I lost one of my assistants; I had only one instead of two. It sounds wonderful to become an IBM fellow, and it certainly is. I have enjoyed it. Already, the possibility to be every year at the IBM Technical Recognition Event is great. Fellows are allowed to travel as they like, which I used extensively. So altogether I am very grateful. But there is nothing which wouldn't have negative sides; and I will very carefully try to explain to you what the negative side is. A fellow gets the permission to do what he likes. This means if you do so, as a rule, you jump out of the framework in which you had been. The only place where this is not severe is IBM research. So those fellows who are appointed within IBM research have relatively good positions because they may try another project, but by definition they are within the right framework. Not so me; I was suddenly alone. I could not request one of my former collaborators from the Vienna Laboratory because I immediately would have had to face the reproach that I weakened the laboratory, if I take a very good one; and if I take an average one, I don't have very much of it. Moreover, it's a question of money; an assignment at that time was just at the point of getting very expensive. So, essentially, I had to select from the German people. It's not easy to find somebody who has the right experience. If he has the wrong experience, he is like a carriage that runs in certain rails, and it's very difficult to get these people at a later age off the rail. Younger ones, IBM almost did not hire at the time. So you may have some idea that I was blocked in this direction. I did not have the proper partners. I had nobody to talk to anymore. Also, with the top of the company you have a one-way communication. Every other IBMer has the appraising and counseling talk with his immediate superior. If somebody reports to you, every year you make this consultation and to produce a document on a prescribed form. A fellow doesn't have this. He makes a yearly report, which is considered as the replacement. But it means that you concoct ten to twenty pages of information, which runs somewhere in the machinery. You never get the an echo, no answer, nothing. Nobody is responsible for you. You are on your own. This is not very good. First of all, a human being needs a bit of pressure. The only pressure a Fellow has is that he was to do what he wants to do within 5 years. That really doesn't work as a real pressure.

ASPRAY: It's too far in...

ZEMANEK: In the beginning it's too far and at the end it's too late. It turned out that the particular subject I had chosen (and I wanted to choose it; that was a clear decision) was a very difficult one. Let me explain the start of the idea. First of all, when we had finished the formal definition work, my people felt the success was not proportional to the effort we had put into it. They said, "Yes, we made a mistake. First of all, we were doing something which is not practical. In this company you must aim at a product. Then you are successful." Secondly, philosophically seen, the formal definition tells you how to describe things. It doesn't tell you what to do. Not even how to do it. It merely says, when you do it, this is the way to describe it. This was true and gave me a lot of thinking. From another source, I had discovered the term "computer architecture". It fascinated me because I immediately understood it was more than a mere other word for the layout. It meant a philosophy; the philosophy of appropriate design. So I started asking myself, what makes a design a good design. The answer is architecture very generally. If we look on the early definitions of computer architecture, which was an IBM idea, it really appears in the first time in the book on the development of Stretch,[55] where the fathers of the later 360 system developed the philosophy of architecture. It was Fred Brooks. It was Gerrit Blauuw, who brought certain of Vitruvius's ideas from van Wijngaarden, I think. The third father of the 360 system was Gene Amdahl. On the architectural side, it was primarily Fred Brooks and secondly, Gerry Blauuw who developed the ideas. So this was one of my sources. Then I got the idea and said, "Funny, the architects are in business for at least 300 years. Why do I not look at what they can tell me?" One way to do so was to take the Encyclopedia Britannica, open to the entry on architecture, and make a second version, turning everything to the field of computers. It was interesting, but not all too successful. The suspicion that the architects had no proper theory was totally confirmed. I really studied the theoretical bulk of architecture. I went to the Harvard School of Architecture and I found out there was an Austrian professor, so I had a very good interconnection. We looked it through. I cannot say I found nothing. I found two things for my purposes. But all that remainder is really in bad shape. The one guy I found was Vitruvius, the architect of the Roman Empire, who

[55] see the book on Stretch:
W. Buchholz (Ed.): Planning a Computer System -- Project Stretch. McGraw-Hill, New York 1962; 322 pp. (notably the chapter on "Architectural Philosophy" by F.P. Brooks, Jr., pp. 6-16.).

wrote the first book on architecture. This is one that can be used for computer aspects and has been used, actually; because, as I say, I am convinced that Gerry Blaauw brought in a number of ideas from his knowledge of Vitruvius. What you have to read, if you are interested, is simply an English translation of his first book, and you will be impressed. Or you read it in my paper for Copenhagen.[56] He talks about education of the architect and you can very easily translate this into modern conditions. You see that we failed. Our philosophy of technology in general is whoever knows the necessary physics (first -- mathematics; secondly, physics; thirdly, the known technologies) is good at design. Now who says that? It was only one branch, namely building architecture or building of buildings, that made the division between studying the engineering of the house construction and the architecture. But that's the only case that the separation was made. For instance, nobody speaks of the architecture of a car. There is the term of ship architecture. At the Franklin Institute, I found a nice indication to that. But in general engineers have not recognized the difference between the art of design and the art of construction, which have two different points of view. If you look from the construction point of view, you would do what the average engineer does. You optimize piece by piece in your chain. It is the architect who steps one level up and looks on the whole and says, "It is better to improve less, but to have common properties in mind." For instance, if you improve piece by piece, you would end up with very many elements, with very many materials or even metals. You would have twenty-five metals. An architect would step up and ask, "Can we not come through with three?" This is only a general structured idea, what architecture really should do. So I decided I would really study and call the things together. I have in a paper for the Copenhagen summer school,[56] the best description of my ideas so far. I have a prepared book. But I am dissatisfied with what I have. And I am not able yet to develop it so far that I really want to present it. It sleeps at the moment. I am very unhappy, because you would think that within nine years, you should be able to produce a reasonable book. If not so, that afterwards, now in retirement, I would complete it. I don't know. I hope that God will keep my health good enough so that I can return to it. I will not make it as one of my next steps. I have other things before. So my architecture is somewhere in the waiting endlessly, in the sleep status. However, this paper, in Copenhagen, for anyone who is interested, gives the right structure. I give you only two basic ideas if you

[56] Opening paper Copenhagen Winterschool 1979. See also in the same volume the dinner talk and the dedication to H.Z.D. Bjorner (Ed): Abstract Software Specification. - Springer Lecture Notes in Computer Science Vol. 86 1980; 1-42, 554-563.

wish.

ASPRAY: Please do.

ZEMANEK: For instance, you can look on the development of any technology and you will find four phases. The first one is the natural one, where things are not yet sophisticated, particularly if you think of housing. A house would be constructed by the standard rules which are heuristic (?) or ... I don't know. They are nowhere written down. People know them, carry them out. There are standards. You don't do what you like. All the farming houses of, say, the 17th century look in a certain way similar. In another way there is space enough for individuality. So it's a kind of natural equilibrium in which even technical things are produced. Now comes science and technology and have tricky, fantastic ideas. But they disturb the equilibrium. Houses or any other technical objects are constructed by standard rules which were heuristic, developed over centuries. They were not written down. People knew them, carried them out. Think of a house typical for the outskirts of bigger cities, where the parts of the house, without much taste, are simply selected from the sales catalogs of the furnishing industry. If the builder needs a window there, he looks at which one fits best and then he puts it there; not very much taste, not necessarily agreeable to live in -- simply a technical product. If the confusion out of this principle is big enough, comes the cry for the architect. We need somebody who has a view of the whole. So architecture develops. That's the third phase. You have principles of design. You have principles of selecting the parts. You wish that each part is a function of the whole. That's the principle of architecture. Then comes the fourth stage, which better should be included in the third. Namely, if you go too much along those objectivity trends, you get a very inhuman result. And a cry comes up for rehumanization; very typical again for house construction. Now with a computer all of that is equally true. These four phases is one way of looking at architecture.

Another way of looking at it in a different universe of dimensions is the distinction between the object, the production line for the object, which influences the object, and the documentation necessary, which is particularly important for the computer because in a way you could call all the software the documentation for the hardware. But that's not the end for software itself. All of what I am saying is applicable on it too, so it gets recursive. There is the object, which is the program or the programming system. There is the production line, which is normally not considered. The tools are poor. The systematics are poor. So we have not yet reached, really, an architectural status for software production. Documentation is the next step. You have to document the document, which for many people is not as obvious as it should be. Therefore, also, the quality of documentation is terribly bad. A document in itself, again, has an architecture. So you can again turn the whole thing around. You have some idea why it is so difficult to write a book. You come to items like criticism. Criticism today, unless it's private or political criticism, is essentially understood as art criticism, where somebody goes to a performance, gets some feelings and ideas, and writes in some fashion. There is no education for the critic people. I would train them. There is logic behind critics. I have found one paper by a logician who says that critics should do the following. He should first describe the object: what was it that has been performed? Secondly, he should describe the kind of performance. And only the third one is then the critical view, the negative sides of it. Now it's a little more complicated, but as an introduction that's very fine.

TAPE 3/SIDE 2

ZEMANEK: What are the rules of criticism in our case? First of all, there should be a critical review of the specification because for very many products, it is already in the specification where later weaknesses are nailed down. So a critical review of the specification is very important. Then, you need a critical review of the architecture of the design. Then you need a critical review of the finished product because, very often, there are lots of feedback processes between the two and the architecture has to give in to practical and economical needs of production. Thirdly, I propose a fourth review after an appropriate time of use. Let us look at a building. Buildings when they are opened usually look much nicer than they look after five years, while a baroque castle you can look upon after 300 years and it's still nice. Again, there are many things that one can learn from other fields. I once wrote a paper[57] for "A hundred years of the Mercedes Benz Company". I gave slight parallels to the situation of the car, which is very

[57] H. Zemanek: Gedanken zum Systementwurf. -- In: Zeugen des Wissens. 100 J Automobil -- Daimler Benz (H. Maier - Leibnitz Hrsg) v. Hase & Koehler Verlag Mainz 1986; 99-125.

instructive too. The architecture of a car is a very interesting feature. Here I want to mention one of the principles. If you look at a car, most people are very much interested in how fast it accelerates. But this is only one side of the picture. The brakes and the stopping are as necessary as the acceleration. So, architecture will always look on the symmetries. It will ask, how does it come into existence? But it will also ask, how does it go out of existence? We very seldom look on what happens to a computer after it has been used. This is true for software as well. So there are many, many aspects. I hope I have made it clear to you what the ideas of architecture are. I think I have also made it clear why my comprehensive book has not yet been written.

Moreover, I got lately interested in history. No, I have been interested in history all the time. Let me go back to the IBM fellow once more. A man like me -- who had most of the successes in his life, by having understood very early that, in computing, one needs young people and one needs teamwork -- being suddenly transformed into an IBM fellow who is alone, has no easy way of building up a team. I probably should have done this, but I didn't recognize the problem. Being alone, being sent to Germany, made a big shock on me. It was so difficult. Well, I don't know. I can only judge five years from now or maybe later. But, I think I have made clear what the points are. What I would have needed was a team. What I would have needed were people to support and encourage me. I could have sent out a letter to all my friends and asking, "What do you understand by a system? What do you understand by architecture?" Such a questionary turn would have helped me a lot. I haven't done it. There are other things which I haven't done. To a certain degree I am a little angry to myself. On the other hand, I slowly understand what really happened. Maybe I can correct it. Maybe not. It's fate, again. I don't complain. I had a very nice time. I have continued to understand.

Well, there's another point. We have not touched here what I have done for IFIP and I invite you to get it from the written sources.[58]

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[58] H. Zemanek (Ed): A Quarter Century of IFIP - The IFIP Silver Summary. -- Proceedings of the 25th Anniversary Celebration of IFIP; Munich. North Holland Amsterdam 1986; 598 pp. H.Z.: This is, I think, a unique volume, reflecting the history and spirit of an international federation.

ASPRAY: Yes.

ZEMANEK: I think I made, at a specific point in my life (although I cannot show you the point and I certainly haven't done it as I describe it now, but I somehow did) the decision that I would less care for technical details and more for a universal overview. I decided on understanding computing, for instance, as a global activity, and IFIP gave me lots of possibilities for understanding the different views of information processing in different nations. Today, there is no country on earth important for computing where I wouldn't have friends, where I wouldn't have lots of insights. I have visited many of them; most of them, one can say. I have an idea what information processing means on the global side. On the personality side, a list of people I have met in my life, I think, could be relatively impressive. So, it was a clear decision that I would not become a specialist in any one of the narrow technologies, but rather that I would go for a large view. I think this is my strength.

ASPRAY: How did IBM support your IFIP activities? What was their interest? Was it that they were allowing one of their senior scientists to do something he thought important, or did they have a feeling that it was important also?

ZEMANEK: Here I must really praise IBM. They have done a lot for me and have never, never tried to influence in a narrow company sense what I was doing.

I had to respect IBM. That's quite clear. I had to be careful with interviews and so on. That was an unwritten law. I had one or two incidents where I was misquoted by journals and then there was a big fuss about it. But this was rather to make the situation clear. It was never any problem for me, never any deep reproach against my activity.

When I was chairman of TC2, IBM simply permitted me to do what I wanted to do. As a laboratory manager, I had a basic amount of degree of freedom, which I could use for the purpose. For instance, this first working conference, of which it was absolutely clear that it has a very positive influence on the subsequent formal definition work in my laboratory, had to be appreciated by IBM. There were a number of bosses who not only permitted me to do it. They found it was worthwhile for IBM, for me personally, and a duty of IBM to the professional community, that one or the another who is gifted and willing should do it. So this is true for the first seven years. The second seven years, the period I was in the IFIP executive body, I had particular support. When I came back with the information that there was a chance I would become vice president and nobody could exclude that they would even make me president, IBM management reacted absolutely positively. I immediately got a budget which was higher than the overall IFIP budget. Well, you know, in a laboratory you have lots of overhead. So this is not an appropriate comparison. By the way: talking about the IFIP budget is a special story in itself, which I have witnessed over twenty-five years. But you see the amount of sympathy and support I have got. I was allowed to have a special secretary for IFIP. I was allowed to hire an assistant solely for my IFIP purposes. I hired a lawyer for the purpose in the beginning. I hired an economist in the second period. Because I felt these were the fields in which I needed particular support. So the answer is, not only has IBM permitted me to do so much for IFIP; they have supported it. Clearly, you can always use all the feedbacks and all the combinations. So it was not so that my IFIP work was an orthogonal dimension of my work. It was always interwoven. I normally made a trip for academic, IFIP and IBM purposes (you should rather, turn the sequence around). I used to report to my bosses, and practically they were always satisfied with what I have done. In an overall evaluation of my life, I must really praise American generosity and IBM support in particular, for the IFIP work, which really enlarged my view and gave me possibilities which I could not have reached within the company itself. Although as you know IBM is truly international, and the advantage that wherever you are, except maybe in the worst eastern countries, you can go to IBM headquarters. You are sure of support. You are sure that you are at home, and so on. So it's a multidimensional story which you ask me here. It's a positive story. The negative aspect (there is everywhere a negative aspect too) is that IBM was not particularly interested to gain anything technically from what I had experienced. Whether you think of the first period where my suggestion had been that one should aim at a unification of languages, that the academic world and the applied world of IBM should have a common language so that you learn already at the university what you afterwards apply in industry -- got no reaction from either side. The academic people didn't really move for a cooperation, for a unification, and IBM didn't move, almost not, in the other direction. If you consider that for many years of my life I was an ideal switch-point between FORTRAN and ALGOL, or between PL/I and ALGOL, and neither side made any use of this possibility. You see a lot about how important mentalities are in technology.

ASPRAY: You have not said much about your later university career. Would you like to make a few comments?

ZEMANEK: Yes. It is not really a career because what happened was this. I started, as you have heard in my description as a university employee, as an assistant professor. When I left the university in 1961 for IBM it was a clear decision to stop the real career at the university and to turn to industry. So the consequence is that on one-hand I could have returned full-time to a job at a university, and indeed, I had two offers in my IBM time. I had one call from Munich[59] and later on I had a very honoring call, namely Unicoloco, to Vienna.[60] You know, this means that normally there is a three-fold proposition to the ministry, but if somebody is wanted exceptionally urgently, only one proposition is made. This was done for me. After careful consideration, I declined Munich. I declined again, Vienna. I think I have done right. But this leads very far. On the other hand, I have never stopped teaching; so there is no year since 1947 (well, except the year I passed in France) that I did not lecture in Vienna. So I have, really, been teaching all the years.

ASPRAY: Even the time you were in West Germany?

ZEMANEK: I flew into Vienna and gave my lectures. And also, last year when I had the guest professorship in Munich, I continued lecturing. I did some blocking. (What you are doing is you are not doing it every week. You make it fortnights or so.) But, otherwise, I have kept within the framework. I have always been teaching. But this is lecturing and supervising theses: it is not what you would call an academic career. The third dimension are the titles. Now the titles... I was a doctor and docent already before I joined IBM. It was Marvin Kelly who pushed that I should get a title of a professor, which I did; it was successful. They gave me the extraordinary professorship in 1964. Now, in the last years, they made me ordinary professor, which is nothing very conspicuous. You just remove one letter from the abbreviation, maybe. But it's a very rare case. Only very few people have gotten this distinction.

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59 call to Munich 10 October, 1964, reclined 12 December 1964.

60 call to Vienna 2 July 1969, reclined 18 June 1970.

ASPRAY: Have you been involved in the advanced training of graduate students in your later years?

ZEMANEK: You mean, whether I have, for instance, guided dissertation work?

ASPRAY: That's right.

ZEMANEK: The answer is yes. Not too much; but one here and there was always in the program. The most interesting case is the Soviet citizen, whom I brought in two scholar years to an Austrian doctor of science degree.

ASPRAY: Who was this?

ZEMANEK: This was Mr. Butko, a graduate of the Odessa Polytechnic Institute and University, who had a scholarship here in Vienna for two years, who had known me and wanted to work under my guidance. He had a heart pacemaker. Is that the term you are using?

ASPRAY: Yes.

ZEMANEK: Yes. He had one and then they suddenly got afraid it would be of damage to the heart. So he came to Vienna with the idea of writing a dissertation simulating the heart and the pacemaker, and trying out on the simulation the best fixing of the pacemaker so that it doesn't hurt. By a number of very interesting inventions and tricks, we achieved the whole thing.[61] He was an excellent student. He got the best mark on the examination he could get. He even taught one of the professors; rather than answering a question, he gave him a lecture. He was very good. Unfortunately, our connection has gone down because he's not in the academy framework. He's at the university.

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[61] A. Butko: Untersuchung eines technisch-biologischen Systems mit bio-elektrischer Steuerung. Dissertation TH Wien 1969; 116 pp.

A. Butko: Simulation eines kombinierten technisch-biologischen Systems mit Digital-Rechnern. Elektron. Rechenanlagen 11 (1969) 195 - 204.

But, we are still a little in contact.

ASPRAY: Are there younger people who are continuing to develop some of the ideas that you've worked on for major portions of your career?

ZEMANEK: Yes. The answer is yes.

ASPRAY: You were in a mentor relationship in some way, perhaps?

ZEMANEK: I have not built up a school. This was the decisive item. I was very close to being the founder of a Viennese school of informatics. But the University did not offer me what I would have put as a condition. In a way, they should have done this before I joined IBM. Coming back, my conditions were more severe. Meanwhile, I had learned the large-scale operation, and what they had offered me always was too narrow for what I saw. Not because I felt I am so important, I need a large field of operation. Rather I am talking from the technical or from the content side of it. I saw a much larger task to fulfill, than the opportunity, the tools they were offering. So there is a long set of professors who had learned with me. Recently, when I was in Marburg, it turned out it was in the hands of a guy who had done his diploma work under my guidance, and he felt that the way how I brought him to the diploma really focused the line of his life.

ASPRAY: Could you mention, for the record, some of these people, the names of some of these people who have worked in your direction?

ZEMANEK: A 'product' of the Vienna Laboratory, for instance, is Professor Kuich, who is the Vienna theoretician for informatics. Under my hands, he made his Habilitation, and he was called from my laboratory to a chair.[62] Another

[62] Dr. phil. Werner Kuich, Professor for Theoretical Informatics

name is Mr Erich Neuhold,[63] who went to the states, was teaching at the Brooklyn Polytech, started writing books, got a call to Stuttgart, got a call to Vienna, and recently joined the German GMD to run an institute.[64] Out of the formal definition group, there are a number of professors. There is Alber in northern Germany, I think in Hanover or Braunschweig.[65] If you want, of course, a complete list, I would have to sit down and compose it.

ASPRAY: Just some samples.

ZEMANEK: Then I mentioned already, when we talked about the Vienna Definition Method that the foreigners in my laboratory, namely Dines Bjorner[66] from Denmark and Cliff Jones[67] from England, afterwards went over into an academic career and they are really continuing the work; particularly Dines Bjorner is very successful. He uses the Vienna Development Method for products which he sells for a lot of money, even to China. But the list is much longer. Let me conclude as far as this subject is concerned, with an account of my historic interests. It started, I would say very early, by looking into the different automata. You remember that the cybernetic machinery was of the automaton character. The computer is an automaton. So for me it was quite clear, looking into the history of automata, I would learn a lot. I may even insert here that I was always found looking into history very practical. When I joined IBM, the first thing I did, I looked into the history of IBM. Very soon I discovered the importance of Mr. Patterson[68] for IBM. You know of whom I am talking; I am talking about the General Manager of National Cash

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[63] Chair for Theoretical Informatics.

[64] An Institute in Darmstadt of the GMD (Society for Mathematics and Data Processing -- a governmental agency). The name of the Institute is "Institute for Integrated Publishing and Information Systems".

[65] Klaus Alber, University of Technology Braunschweig.

[66] D. Bjorner, C.B. Jones (Eds.): The Vienna Definition Method -- The Metalanguage. Lecture Notes in Computer Sciences Vol. 61. Springer, Berlin 1978; 382 pp.
P. Wegner: The Vienna Definition Language. ACM Computing Surveys 4 (March 1972).
ANSI X 3.53 Programming Language PL/I Standard.

[67] C.B. Jones: Software Development - A Rigorous Approach. Prentice Hall, Englewood Cliffs, NJ 1980; 400 pp.
C.B. Jones: Systematic Software Development Using VDM. Prentice Hall, Englewood Cliffs, NJ 1986; 300 pp.

[68] S. Crowther: John H. Patterson -- A Pioneer in Industrial Welfare. Doubleday Page, New York 1923.

Register, who at the time of Mr. Watson, was so to say the high school of management in the United States. You don't understand IBM and many of its installations, in the general sense, unless you understand the experiences which Mr. Watson had with Mr. Patterson. The quota, the 100% group, for instance, is one of the subjects. But there are many more. I gained a lot from reading the family story of the Watsons[69] and the book on Mr. Patterson.[68]

The same I did for the prehistory of automata. So you have seen those studies I have made, little papers on early automata. This certainly was one way of doing it. It was a kind of hobby. I wrote here and there. I started collecting a few automata and musical automata in particular, but not very much. The decisive step to become more of a historian was the instance or the case of Schaffler, where I had an Austrian name, and nobody knew what he really had done, what his history was. I had to reconstruct the gentleman from nothing, from a status where I like to say he had gone out of history on purpose. He left the glory to the Post people, provided he was allowed to earn the money. Very smart.

ASPRAY: Yes.

ZEMANEK: Step by step, I reconstructed the man and the result[70] was fascinating. I discovered that Mr. Schaffler had filed the very first programming patent in 1895. I got the idea of what one has to do in order to look into the life of a man and derive from it his importance for the history of technology. You always get a lot of side effects of insides of which you wouldn't think in the very beginning. A second adventure which chronologically followed this first one, was also an old question of mine: namely, where does the term algorithm come from? By asking this question repeatedly, I finally located it to the country of Khiva. Academician A.A. Dorodnicyn (so he spells himself - the correct transliteration would be Dorodnitsyn, but he uses such an older version), the Soviet delegate to IFIP, had started his career in Tashkent, which is not too far away. He liked to say, "I must invite you to Tashkent." He never

[69] T.G. Belden, M.R. Belden: The Lengthening Shadow -- The Life of Thomas J. Watson Sr. Little, Brown, Boston 1962.

[70] Heinz Zemanek: Datenverarbeitung vor 100 Jahren: Otto Schaffler (1833-1928), ein zu Unrecht vergessener osterreichischer Pionier der Nachrichten - und Lochkartentechnik. -- E&M 90 (1973) 543-550.

really did, but we had a standard remark in answer to the situation in which he said, "You must come to Tashkent." I then started to say, "Yes, I will come, provided you make it possible that I go to Khiva." This game we played for many years until in 1976 he actually invited IFIP for a general assembly to Tashkent. So I set up an admission, "Now is the point, I want to go to Khiva." His reaction was, "Mozhno" (it is possible). It's a little more. There is an interesting semantics behind this answer. To give a short account, I came to Tashkent. I tried to go to Khiva and finally they said, "Sorry, it can't be made. There is no way of flying." Then I said, "It doesn't matter. I'll go by camel, by car, by bicycle, whatever." But you know, it's one hour's flight, meaning 600 kilometers over desert, so all the other possibilities couldn't work; only the aircraft was a possibility. Practically a day before departure, Dorodnicyn came back and said, "It's possible. Do you stay till Saturday?" I said, "Yes, I stay till Saturday." It was made a "state visit", with the meaning that for the state delegation was made space for in the aircraft. We had six seats. It was my first visit to Khiva. I didn't know until very late that I had chosen without knowing it an architectural jewel. Khiva is the only city in Asia that has maintained the central Asian character. There are two walls around the city. Within the inner wall, there is no foreign building, meaning, no 19th century modern technology building. The whole city really consists of buildings erected in the historic style; most of them are younger than 200 years, but they could be 1200 years old; they wouldn't look different. So I made a very good choice and it started me to think more of Mr. Al-Khorezmi and it started the Russians to think more of him. They discovered they had a jewel in their hands, which could be used for many kinds of propaganda or publicity purposes. So, in 1979, they organized a colloquium, a symposium; Don Knuth and Andrei Ershov incorporating and organizing it and inviting me to give the historic introduction. This is the paper on Al-Khorezmi you have in your hands.[71] Then Ershov came and said, "Could you give a little talk for the unveiling of a cornerstone?" I said, "What do you mean by the unveiling of a cornerstone?" "A cornerstone of a monument." I didn't understand what he meant until I really pulled the line and the unveiling took place. Here was a large stone and the inscription said, "On this place there will be erected a monument of Al Khorezmi." So I gave a talk and I was on television and so on. Then I was curious what they would do when the final monument would be erected. Indeed, I was invited in 1983. I came there. Now with such an official event, they

[71] Heinz Zemanek: Al-Khorezmi, His Background, His Personality, His Work and His Influence. -- In: Algorithms in Modern Mathematics and Computer Science (A.P) Ershov, D. Knuth, Eds) Springer Lecture Notes in Computer Science Vol. 122 1981; 1-81

could not have a foreigner unveiling the monument. But they were very nice. What they did was the workers unveiled it and I was the foreign guest speaker. So actually, I am the only IBMer who ever unveiled a monument in the Soviet Union, and I did it twice.

This story brought me deep into the history of mathematics. It turned out that Al-Khorezmi not only gave the name to the algorithm. He really is the bridge between Greek mathematics and medieval and later mathematics in Europe. It was due to his books that mathematics spread over the Islamic empire. It was from this empire that Europe got a re-interest in mathematics via Spain, via Italy, and via Sicily in particular. The best-developed sources referred to Spain. Italy is rather characterized by Leonardo of Pisa, who collected his knowledge all around the Mediterranean, while about the Stauffer emperors in Sicily very little is worked out because of the weakness of the historians who always tried to avoid mathematical questions. But now there is a new generation of historians around, and things are improving very, very fast. In particular, in your country, Mr. King, who presently is in Frankfurt, for instance, went on a search for Al-Khorezmi manuscripts and he found some.[72] So there are still some around. I think that there is a lot of future in all of those fields. Let me make a side remark. When you treat a man like Al-Khorezmi who lived 1200 years ago, you would like to have as much biographical information as you can get, which is very natural. Only, you find little because 1200 years ago biographies were not yet written. For instance, we do not know when he was born. We can only compute from his first appearance that he must have been at least, say, 20 years old, so you can make an estimate when he was born.[73] About his death, we are also unclear. However we know that in 847, a caliph was on the point to die, so he called his scientists and wanted to have a horoscope. The scientists of the Academy of Science really produced a very fine horoscope for the caliph and said, no worry, you will live for another 10 years or something, 20 years. He died a fortnight later. The historian reporting this was smart enough to list the names of the mathematicians and Al-Khorezmi was one of them, so we know he was still alive in 847. Usually one says he died in 850, but this is absolutely arbitrary. The only story out of his life... Well, he must have traveled. He obviously was in

[72] D.A. King: Al-Chwarizmi and New Trends in Mathematical Astronomy in the Ninth Century. Hagop Kevorkian Center for near Eastern Studies; Occasional Papers on the Near East, No. 2, New York 1983; 43 pp.

[73] Birthday of al-Khorezmi: The 1200 years celebrations in the USSR in 1983 'fixed' it to 783.

Afghanistan. He was in the country of the Khazars. I'm not sure that Al-Khoremi was born in Khorezm, the region of Khiva, because at that time the Khorezmians were already a large community in Baghdad. It was the Grinzing, the restaurant quarter of Baghdad where the Khorezmians would run the restaurants. The district was called Qutrubul. (I even have a map of the old Baghdad.) Today there is nothing left of the city of the old times. In it's place, now, is the freight railway station. There is nothing left. These things fall to sand.

The only story I finally discovered was a report where the caliph sends his astronomer, Mohammed Ibn Musa (which fits to my hero), to inspect the cave of the seven sleepers. In the beginning I thought this was merely a matter of a short report. You explain the legend and you quote the story, which is there in several versions, and that's the end of it. Well, what I found out first, having a better version of this report, that they never have been in Ephesis, where they should have gone. Secondly, I started to question myself, why would a caliph send his astronomer to inspect the cave of seven Christians? It turned out the seven sleepers are as well Koranian saints, as far as such a thing is in existence. They are mentioned in the Koran. A full surah, namely number 18, is devoted to the seven sleepers. Therefore, the whole subject is also an Islamic subject. Then it started mushrooming. I visited not the Islamic places, but most of the Christian places where there are either chapels or pictures devoted to them. I have seen most of the museum pieces referring to the seven sleepers. So I've got a fascinating enterprise related a little bit to Al Khorezmi, but a subject in itself.

Another book I should write is the book on Maelzel referring to the invention of the metronome, to the first stereophonic composition for two orchestras by Beethoven, Opus 91, which originally was intended to be run on the music machine, but which was then rewritten for two orchestras. The story then turns over to the history of the wrong chess player, of the magic instrument invented by Mr. von Kempelen, which then was shown by Maelzel in the United States. There is the famous essay by Edgar Allen Poe. But the story is much more involved and has many further chapters. There is a very good book in America about the chess player,[74] but I have still more details. I even have brought back from America an original letter of Maelzel on the subject. I think that concludes my telling

[74] C.M. Carroll. The great Chess Automaton. Dover Publ., New York 1975, 116 pp.

stories. If you have any further questions, you can ask anything.

ASPRAY: One of the things that scholars would be interested in doing is looking at some of your written works. Can you give us some information about how to find access to your work, what you published, and so on.

ZEMANEK: Oh this is, fortunately, an easy question for me because I have been very careful in registering and filing all my publications. As a matter of fact, over there is a collection of all my papers. For my 65th birthday, when IBM organized a symposium in my honor, my collaborators included in the booklet a list of all my publications from the beginning to 1985. So in this book, which is called *Formal Models in Programming*.[75] The list of publications starts on page 11 and goes through to 28. It even gives a subject area overview of my publications. Moreover, all those publications are filed at the library of the Vienna University of Technology. Recently I have updated this collection, so it is practically complete. I will try, as long as I can do so, to keep it complete. I am also working on some documentation about things which are not my works, but refer to me or have otherwise to do with me. So the university library will have a complete file where any scholar could check for any of those publications from me and around me.

ASPRAY: Let us take on an orthogonal topic now. The question is technical developments within IBM in Europe as compared with technical developments in the United States, and the transfer of those technologies between the two areas.

ZEMANEK: Well, we will have to distinguish the proper IBM part of it from the remainder. As I said, they are a little orthogonal. The overlapping is not all too big. Let me start by explaining to you the operation of IBM in Europe. They started relatively early to have subsidiaries or interested people over here in Europe. If you take Germany as an example, there was a gentleman[76] who got interested in punched card equipment before IBM as such was in

[75] E.J. Neuhold, G. Chroust (Eds): Formal Models in Programming. -- North Holland Amsterdam 1985; 425 pp.

[76] DEHOMAG was founded on 30 November 1910 in Berlin by Willy Heidinger, who was also the first General

existence. He had a kind of branch enterprise which was not a part of IBM. It was then called DEHOMAG, Deutsche Hollerith Maschinen AG. It finally developed into IBM Germany. But in the beginning and during the war time it was quite independent. On the other hand, from the very beginning, it would have been very silly of IBM to develop separate equipment over here. This is the tension that exists until this day; that if you want to have world-wide products, you must have a central authority that controls the product policy, the product development, the product production. Therefore, there is always a certain tension between people who want to achieve things, who want to give more local weight to the different branch enterprises and the other ones who watch for central controls. So you have two parties who fight each other. You can imagine that immediately after World War II, when the possibilities in Europe were growing, the party that was for more freedom, for more independence, for more local ideas, were the stronger ones. So, like mushrooms, there were ideas and later on also laboratories created in Europe -- not necessarily to the pleasure of the central power. Wherever you look, you would probably find, if you could analyze it, the activists who wanted to create something in Europe and the central controllers who tried to inhibit it. Or if they couldn't inhibit it, they would try to get maximum power over the thing. This is true for research, where somebody had the idea of establishing a research laboratory in Europe and they chose Zurich, which probably was not the best choice simply because Switzerland in the German speaking area is not considered the best place to go. The French don't go to German speaking area and so on. So Zurich probably was really not a good choice; but it was chosen, and there it was, and there it is until now. I could support what I am saying here by practical experience. I have had contacts with the gentleman who was in charge of trying to build up a mathematical department and failed because the mathematicians didn't want to go to Zurich. This is just to explain to you how difficult the situation is. Another is the branch enterprise of the Systems Research Institute. They had a European systems research institute in Geneva, which subsequently was then moved into the general educational center in Brussels.

The development laboratories is the next question, because real product development you can do only in a laboratory of a reasonable size. Now it is clear that each national IBM company, at least, needs a repair workshop

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Manager, see:

W.E. Proebster (Hrsg.): Datentechnik im Wandel -- 75 Jahre IBM Deutschland. Springer Verlag, Berlin 1986; 266pp.

because you cannot send everything abroad. You try to do it on the premises, if possible. So there is a certain inclination to have your shop in each country and then to develop this shop into a laboratory is a very, very natural thing to try -- for many reasons. The most successful are of course the British because they have no language barriers; so its quite clear that there is a British Laboratory. The Germans, because of their mechanical strength... In the early days, this was particularly important --no problem to convince IBM to have a German laboratory. So they started Sindelfingen as both a laboratory and a production facility. The French also have several production plants and they also have a laboratory. Italy is more or less unstable. It comes and it goes. Italy had a laboratory, but I think only for a fortnight or so. This never played a big role and you would hardly find it anywhere in the lists. Sorry, I jumped a little too fast over France. You see, I make a primitive picture now of the Americans but there is some truth in it. England is not yet Europe because it's still English-speaking. So the next guess or the next decision of the Americans is always Paris. For my mind, Paris is now terribly English or Americanized, but for the Americans its a foreign-speaking country where you go. All that remainder then is, so to say, already in the forests; not to talk about the east. So it is not by accident then that the headquarters of IBM Europe have to be in Paris (as far as they are not in White Plains, NY).

So it is obvious that there had to be three big European development laboratories, the British, the German, and the French. The French, by the way, was moved one day from Paris to La Gaude near Nice, because the French authorities wanted an action to release the overburdened Paris area. The consequence was that the laboratory lost half its staff and had to find new people willing to work in the Nice area.

Later, when the big laboratories were shaped, came the boom to go to other countries. Three further laboratories of the development kind were established: one in Holland, mainly for mechanical development, moving parts and printing; then a laboratory in Sweden, mainly for questions of process control, in view of the Swedish steel and Swedish paper industries -- so they did process control computing; and the Vienna Laboratory because I did exist and the Germans pushed it. It was started as a part of the German laboratory, and it was conceived as a software laboratory. So there were three big laboratories and three small ones. In the late 1970s, the middle of the 1970s, the dying of the small ones started.

TAPE 4/SIDE 1

ZEMANEK: So we have the three big laboratories in England, in France, and in Germany. There were the small ones with the specific areas in Holland, Sweden and Vienna. Around 1975, the small ones disappeared. Today, you have only the three: the British, the German, and the English. The next question is, what is the style and the condition of development? You have the same contradiction I have already described. You have the people who have the ideas, which means decentralization, and you have the people who have to administer and watch the costs and so on. They are in favor of centralism. On the other hand, IBM has never been really centralized. There were always competing groups. IBM never saw a disadvantage in parallel development. They often had several candidates for the same purpose. The principle was, let us see which of them is the most suited. To extend this principle into Europe was nothing very special, except that there was more distance and control questions came up that the fight between the development groups in America was somehow depicted under European conditions by allying, by inviting people. You can imagine how all of this runs. One can say there are also two kinds of products under development.

One conformed to the central plan. The other one is the category where somebody has locally an idea, a bit of manpower to carry out some development. They produced something which was not centrally planned, or was a contradiction, or at least in competition to what was done in America. Then it's a question of... how do you say in English?.. of competition. Which one is going to win? Which is going to be the official product? Or will there even be two variations of the same product? So again and again, the administrators, the central management tried to make decisions -- redistribution of the development lines, redistribution of the goals. It was a constant flow. If you have that in mind, you will see that you get a very natural answer to your question of the mutual influence. While there was, and there will always be, little direct contact between a branch of IBM and any other local enterprise, it is a natural thing that the know-how as such is interchanged simply because you hire people from the outside, people leave IBM, accept a position offered by a national industry. There moves also, each time, a considerable amount of knowhow. So the mutual influence is very big and governments who inhibit IBM of operation in their countries are silly because they cut off the best lines of getting know-how into the country. Finally, my decision was, if you analyze it to the last ground, there was nothing else but the hope that by joining IBM I would pump know-how into Austria, into my personal environment, into my personal brain, but essentially that meant into Austria. What was true for me is generally true all over Europe. So a certain amount of mutual influence, of mutual furthering, of exchange of information is naturally given. What you gain in a giant company like IBM is the large scale. You don't think anymore on the millimeter, you think on the meter scale. I experienced it very much in myself in that I now have a worldwide view of information processing, certainly is coined by the possibilities of IBM. That must be seen. I'm certainly a promoter of European self-developments. But it should always be done in cooperation, in exchange with America. What IBM has done for Europe is certainly immense. That's an influence on our technological development which cannot be overestimated. It has always been underestimated because it does not sharply show up, but it's there.

ASPRAY: Do you want to give some examples of joint projects or projects that were important in Europe?

ZEMANEK: I will give one, first of all which was not so very important, but which is characteristic. When we had shown that our voice answer system was a possible product for IBM, it was moved to Germany in order to be developed into a real IBM product with all the experience of the production conditions of IBM. Then they found out they had too much in Germany, too little in France; and it was decided to move this project into France. So it was moved from Germany to France, and the production itself took place in America. This is very typical. Normally, they make some distribution. I think that for many years Germany was in charge with the middle size computers. So, in terms of IBM 360, it would be IBM 360/30 or 40. Maybe it was a 40, but I give you a general idea. It was the lower middle of the product line where the Germans were put in charge. On the other hand, there was an attempt in Germany to do something very special. The idea was to have half-size punched cards years ago, in the early 1960s. They developed the system and failed and had to give up. It is definitely much more difficult in the risky case to try something in Europe than to try it in America. For many reasons. One of the reasons is, probably, you are too far out of the control centers and you are not aware of certain developments. When you get hold of them, it is too late to correct your own direction. But it's complicated. When the system fails, it usually has many reasons. So much to the general development. There were subjects which were concentrated. For instance, for a long time (maybe it's still the case), the French laboratory was the one for communications and for telephone switching. In America, as you know, IBM didn't touch telephone switching. So naturally, this was in Europe, and in Europe they chose France for it. I once made a negative remark that the French telephone switching had not been one of the best in the past. Now all those differences have gone. But the answer, and probably the right answer, was if IBM achieves to run their systems on the French system, it will run everywhere. That's a good argument. Now, does this overview somehow answer your question?

ASPRAY: Yes, I think so.

ZEMANEK: In the case of software, things could happen like this. Are you aware of the history of the Time/Life section of IBM? IBM domestic had a floor in the Time/Life Building in New York City on the Avenue of the Americas, with a concentration of language development. So the main responsibility for the IBM side, I think, of FORTRAN IV, COBOL, and PL/I, and so on were all concentrated there. Later on, they gave it up and decentralized it. It was then moved to Boulder. The way in which in their big company functions are moved from one place to the other, you don't understand it. Maybe the general management does. Or does not. I have never found out. One day they decided to move the PL/I responsibility out of Time/Life into Winchester and into the British laboratory. That explains why we called it "Luftbrucke", the air interconnection, for our language points between the Vienna Definition work and the language authority was between Vienna and London, because in Winchester was the group of people who defined what PL/I was like. So you see that a full and important function was moved from America into England. Such things occurred again and again. On the other hand, the danger (and the central managers always watch for this) is that you spend too much money because you distribute too much. If you make parallel developments, it's in some way a waste of money. This fight between centralization and decentralization is quite natural and can be applied to many of the developments. Europe has, of course, the advantage of being further away than any place in the states. Japan is also in the same condition. It's a different branch of world trade. But, what I want to tell you now is an example of the Japanese laboratory. They are in need of handling Chinese characters. So they needed a printer that is good enough to do that. They had an excellent development piece for a printing device. It did come through. As seen from New York, such a sophisticated and detailed printing device may be, from a business point of view, not the best solution.

ASPRAY: Yes.

ZEMANEK: Germany was also very much on the printing side and also one day was simply turned down. They had a printer, the basic principle of which was originally invented here in Austria by a company called Koreska. It was the idea of working by light arc through a kind of specialized carbon paper. Where you put the light, it gets black. It keeps very sharp and very, very fine printing. It was a high quality printer they had. It was not made a general product. As you know, the engineering work on a technical product is 10%, and all the other questions make up 90%. Therefore, the judgement of an engineer like myself has always to be taken with a lot of care. The only point is, I realized relatively early in my career that this one to nine relationship was correct. Therefore, I never overestimate my own judgement. For all the other questions, I can only suggest you interview somebody in Paris who has the overview on Europe, which I, by necessity looking from outside, don't have.

ASPRAY: Why don't you mention, on tape, a few people who you think would be worthy of interviewing.

ZEMANEK: As you understand, I have to be a little careful. It's so easy to lay oneself open to reproaches when one says the wrong thing. On the other hand what should happen? F. Genuys[77] in Paris, is one key figure who has seen a lot of the development and who has had the inside experience. He was partially in the services of IBM France, partially in the services of IBM Europe. He has a very general scientific view, and I suggest that an interview with him would make sense in any case. An interesting man is the former manager of the French laboratory, who started, I think in Paris, who was for many years in La Gaude and was replaced; he had several functions, interesting scientific interconnections in Paris. His name is Maurice Papo[78]. And of course, there is Moreau.[79] I think he was never in the

[77] Francois Genuys, IBM France, 5, Place Vendome, F-75021 Paris Cedex 01

[78] Maurice Papo, IBM France, 5, Place Vendome, F-75021 Paris Cedex 01.

[79] Rene Moreau, Manager for Scientific and University Development, IBM France.

European part of IBM in France. He was always on the French side, but on the scientific center he did a lot. He did a book on history.[80]

ASPRAY: Yes.

ZEMANEK: If you could get an interview with retired Mr. Maisonrouge,[81] he would have been, years ago, the right person to ask. Finally, why not try to get K. Cassani[82] for an interview; he knows a lot. He was around for very many years before he became the general manager of IBM Europe, and he's more on the business side, clearly. That's the people I would know. Another in England is John Fairclough. He was for many, many years the manager of the British laboratory. He made a very good career. He is in a high position right now, and he's the guy to be interviewed on this subject, absolutely surely.

ASPRAY: Anybody in Zurich? At IBM Zurich?

ZEMANEK: In Zurich -- Ambros Speiser,[83] as you know, has left. He's now with BBC. He's not anymore IBM and he very much separates from his computer time. No, it is difficult to give you a name. Whoever they choose as a speaker for you will be good, but I do not know a name.

ASPRAY: All right.

ZEMANEK: Two other names. There is Pierre Bobillier[84] in IBM -- Geneva, in Switzerland, who is the IBM

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[80] R. Moreau: The Computer Comes of Age. M.I.T. Press, Cambridge, MA 1984; 227 pp.

[81] Jacques G. Maisonrouge, IBM Europe, Tour Pascal, Cedex 40, F-92075 Paris-La Defence.

[82] Kaspar V. Cassani, IBM Europe, Tour Pascal, Cedex 40, F-92075 Paris-La Defence (retired).

[83] Ambros P. Speiser, Brown, Boveri & Cie, Baden, Switzerland

[84] Pierre A. Bobillier, IBM Suisse -- Geneve 48, Av. Giuseppe Motta, CH-1211 Geneve, Switzerland

representative to CERN to the European Atomic Center near Geneva. He has a lot of knowledge concerning high-speed, and high-amount number crunching computation. He has also been IFIP president for two terms, for six years, and was secretary in my time. So he has a lot of international experience. That's the one name. A similar man with the different character is Walter Proebster[85] in the German Laboratory. He has a historic background because he started off with the German computer development at the university in Munich and had a lot of knowledge from his international activities. He is preparing congresses all around Europe.

ASPRAY: Very good. Now we could have one short minute (we have a little bit of tape but not much more time left) on the European industry.

ZEMANEK: I have mentioned Siemens and maybe Siemens is a good example for the ups and downs. A company meets when its character is communications and it moves into computers. Very generally you know from the American history that attempts of big companies, whether they are in power engineering or in telecommunications, to move into the computing field is not necessarily successful. Many of them have spent a lot of money and had to give up. In Europe this can happen too. But with a big enterprise like Siemens it wouldn't happen, because they have power enough to overcome difficulties and they would not give up. Moreover, from a national point of view, they are expected to continue. However, there is the counter example in Germany of Telefunken, who have stopped their computer activities, more or less. So it does happen in Germany too that there is a big effort and there are even products that come out, but after a certain time they don't continue. So it depends very much on the local environment. In France, for instance, you had in the very beginning a big number of promising attempts to produce computers within the framework of smaller or bigger companies. Not very many of them survived, as you know, and France was not able to maintain Bull as a French enterprise. They had to attach it to Honeywell and so on. You know this story. Bull, as you know, was a Norwegian. In Germany, it was essentially Siemens that kept the position. They were invited, to say it kindly, to save the Zuse company. At the beginning, when Zuse came into financial

[85] Professor Dr. Walter Proebster, IBM Germany POB 800880, D-7000 Stuttgart 80, Germany -- see:
W.E. Proebster (Hrsg.): Datentechnik im Wandel -- 75 Jahre IBM Deutschland. Springer Verlag, Berlin 1986; 266 pp.

difficulties, had to sell the patent rights, which is always half the end of such an enterprise. Then he sold the whole company to BBC (Brown, Boveri & Co in Switzerland) who obviously didn't know exactly what they were buying and what they were doing, what they had bought, and didn't make it properly. So they said, "No, we either close or we sell." Then the government invited Siemens to save them. So Siemens bought the Zuse company and made it a branch of Siemens. For awhile they continued with products like the Zuse graphomat, the plotter. But step by step it lost its particular character and became a normal Siemens' branch factory or development center. Siemens had built the very early transistorized machine 2002, continued, but then decided to have at least in the upper range of computers Japanese products. They had a contract with one of the big Japanese companies. Today, I don't know exactly how it is. In Munich you could interview the old people, Dr. H. Kaufmann[86], who was for quite awhile very active in the development. There is still in a position, although not anymore directly involved with computers, Heinz Gumin, who was one of the leading directors of the computer department of Siemens and still is a Siemens high executive. If you can get one day in an interview with him, he certainly can say a lot. Telefunken, as I said, left the business. One would have to look up how the whole thing came along. They had the TR-4 and later the TR-440 -- relatively successful machines. But the operation as a whole did not survive. However, a positive example is Nixdorf.[87] I have met Mr. Nixdorf back at his famous conference in 1955, and he remembered me very well. We met one day before his death one or two years ago. As you know, he was not only very successful in Germany, he also has a branch enterprise in the United States. His idea was to stay at that time in the low-end. Meanwhile, they are not anymore low-end because there are much smaller units around today. His machines were particularly tailored for the northern Germany industry. That was a good concept. It was very successful. Otherwise, the north of Germany is less computer-open than the south, or less important for computing. Everything slowly moves to the south, either directly to Munich or at least to the line between Munich, Stuttgart, Karlsruhe. The north, somehow, doesn't make it. Hamburg, for a while, had a very good developing informatic department. Meanwhile, two key professors moved to Munich. I don't know. In northern Germany, it doesn't work properly. All these small countries had difficulties. If

[86] Dr. Hans Kaufmann, Siemens AG Munchen

[87] See the paper on Heinz Nixdorf in the 25th Anniv. Issue of the Elektronische Rechenanlagen HZ [342] pp. 100-104.

you look at Denmark, for instance, Nils Bech[88] was not only nationally very active, he had his own company and he produced a famous GIER[88] machine and had a lot of customers in Poland. The machines were of such a good quality that he could afford to have servicing not done by a branch office in Warsaw, but by a telephone cord. Somebody would jump into an aircraft and fly from Copenhagen to Warsaw and get the machine going again. It happened so seldom that this worked. Still, he could not maintain his company. That is true almost all over Europe. It is obviously very difficult to get going. Well, I think we should stop.

ASPRAY: Thank you very much.

END OF INTERVIEW

[88] Niels Ivar Bech (1920-19) -- see:
P. Sveistrup et al. (Eds.): Niels Ivar Bech -- en epoke i EDB udviklingen i Danmark (in Danish). Data, Kobnhavn 1976; 266 pp.