Hermann Maurer

Professor Maurer was born in 1941 in Vienna, Austria. He has got a Ph.D. in Mathematics from the University of Vienna 1965. Assistant and Associate Professor for Computer Science at the University of Calgary and Professor for Applied Computer Science at the University of Karlsruhe, West Germany. Since 1978 Full Professor at the Graz University of Technology. Honorary Adjunct Professor at the University of Auckland, New Zealand since October 1993. Honorary Doctorate Polytechnical University of St. Petersburg (1992), Foreign Member of the Finnish Academy of Sciences (1996). Author of thirteen books, over 400 scientific contributions, and dozens of multimedia products. Editor-in-Chief of the journals *J.UCS* and *J.NCA*. Chairperson of steering committee of WebNet and ED-MEDIA Conference series. Project manager of a number of multimillion-dollar undertakings including the development of a colour-graphic microcomputer, a distributed CAI-system, multi-media projects such as "Images of Austria" (Expo'92 and Expo'93), responsible for the development of the first second generation Web system Hyper-G, now HyperWave, and various electronic publishing projects such as the "PC Library", "Geothek" and "J.UCS" and participation in a number of EU projects (e.g., LIBERATION). Professor Maurer research and project areas include: networked multimedia/hypermedia systems (HyperWave); electronic publishing and applications to university life, exhibitions and museums, Web based learning environments; languages and their applications, data structures and their efficient use, telematic services, computer networks, computer assisted instruction, computer supported new media, and social implications of computers.

Not only Theory

When trying to summarize my over 35 years of computer science research I end up with one major very personal lesson that I have learnt, and that might be helpful to some younger researchers: it is such a rewarding and productive

experience to work together with other researchers, be it students or top-notch experts that one must not miss it. Of two persons no one is ever "better" than the other. Rather, each one has weaknesses and strengths: the fun and challenge is to determine the right symbiosis. It took me a long time to find this out. Once I had realised the full importance of this for me, my life changed: research turned from hard work to hard fun. There are certainly scientists who have obtained greater insights working on their own than my brain would ever have allowed me to achieve, and I admire them. However, for me and I believe for many researchers the key for success is collaboration. Research can often be compared to solving puzzles. On your own, you may easily get stuck; it is my firm believe that n persons together ($2 \leq n \leq 5$) can solve a problem more than n times faster: research results are "super-linear" in the number of researchers. Thus, this paper focuses more on persons than on results. It is a thank you to all who have worked with me and who have become friends one way or another. I have made an attempt to mention all those I have ever co-authored something with. I apologize to others that I have met, learnt to appreciate and who have helped in different ways: there have been many, and there would not be enough room to do justice to all. Before going on, let me mention one further point: the book containing this paper is dedicated to the theory of computer science. However, I have spent much time also in other areas. For completeness' sake, and since drawing borderlines is difficult I will also report on non-theory stuff, albeit shorter.

The early years (59-71)

When entering university in Vienna in 1959 I was set on studying applied physics. However, the first mathematics class I attended was taught by Professor Edmund Hlawka - a superb teacher and researcher. He turned me around 180 degrees: mathematics it was to be, henceforth. And "clearly" the most beautiful areas of all, the theory of numbers. I took the only two computer science (= programming) courses available in those days in Vienna and progressed rapidly with my mathematics coursework. When I happened to meet Professor John Peck (who later became famous for e.g., his work on Algol 68) from the University of Calgary at the 2nd IFIP congress in Munich in 62 I was ready to accept his offer to go to Canada as graduate assistant for a year or two. While at Calgary, I fell in love with Canada, learnt more about computers and computer science and continued my work in number theory on diophantine equations, i.e. equations where one is interested in integer solutions, only. One of those equations $u^4 + v^4 = x^4 + y^4$ is called "Euler's equation" (like a lot of other equations) and the smallest non-trivial solution known in 62 was $133^4 + 134^4 = 158^4 + 59^4$. It was open whether smaller solutions exist. This seemed like an obvious application for computers. In a four-fold loop one would check for all quadruples of values (u, v, x, y) with $1 \leq u \leq v \leq 158, 1 \leq x \leq y \leq 158$ and $u < x$ whether $u^4 + v^4 = x^4 + y^4$. This brute-force approach (for the four-fold loop) takes over $100^4 = 10^8$ computational steps, too many for the computer in use at Calgary in

1962 (an IBM 1620). It was then that I discovered the power of sorting! Rather than examining 10^8 quadruples I would calculate some $n = 10^4$ values u^4+v^4 for $1 \leq u \leq v \leq 158$ and sort them in ascending order as $z_1, z_2, z_3, \ldots$. This requires an effort of $n \log n$, i.e. roughly 10^5 steps. A smaller solution for $u^4+v^4 = x^4+y^4$ would exist clearly if and only if $z_i = z_{i+1}$ for some i, a test that can be carried out in about 10^4 steps. Thus, using sorting, I could cut down the computational effort from some 10^8 to some 10^5 steps, quite feasible on a 1962 computer. The result (sigh) was negative: no non-trivial solution smaller than the one known to Euler exists. Nothing to publish, but a first lesson for me: computers can help in number theory, and sorting is a surprisingly powerful tool in many application areas. This realisation would come in handy years later in the study of data structures and geometric algorithms ...

While continuing research in number theory and obtaining a few new results on the Pellian equation (integer solutions for $x^2 - dy^2 = 1$) I joined the computer centre of the government of Saskatchewan as "system analyst" (May 62 - December 62). Those short eight months of really down-to-earth computing work would prove invaluable later for my understanding of applied computer science. Although not at all related to theory I feel the urge to report two anecdotes. As first job, I was given a huge assembly language program for an IBM 1401/1410 without further explanations and the request "read it so you understand what it does". After two days I was totally frustrated: after reading pages of code I thought I knew what the initial segment of the program would do: nothing but print two columns of asterisks indefinitely, until someone would physically stop the printer. After a restart, 132 dashes would be printed over and over in the same line until again the printer would be physically stopped! When I reported this obviously wrong conclusion (but I had checked it three times!) to my supervisor he was delighted: "Yes, this is what it is supposed to do. This is used to align the forms in the printer appropriately. And the repeated printing of dashes creates a perforation so the forms can be torn off easily." As it turned out, I was thrown into the middle of the first world-wide project to computerize health care. After gruelling months of work, when the team I felt I belonged to by then had finally completed its job one day at 4 a.m., we drove out into the prairies. Someone in our group knew enough about farming, saw a harvesting machine and a ripe wheat field and before we knew it we released our bent-up tension by harvesting that field. A surprised but pleased farmer invited us for a hearty breakfast 3 hours later ...

Back in Vienna, Austria, I continued my Ph.D. thesis work but also needed a job. Werner Kuich, a friend from my freshman years (who later became one of the first computer science professors at the Technical University of Vienna) helped me to get a job with Professor Heinz Zemanek's IBM funded research group whose roots go back to Mailüfterl, the first European transistorised computer built by Zemanek in the late fifties. In this research group I started to learn about compilers (and wrote my first one [2]) and got interested in formal languages and formal description methods. I also made my first scientific contribution by noticing that one (apparently) could improve on the $O(\log n)$ performance of

binary searching by making use of the structure of the data by interpolating rather than brute-force halving. After all, if you look in a phone book for, say, "Beran" you don't open it in the middle but closer to its beginning! I was able to prove the superiority of this technique in a paper [1] only for special cases. Ten years later this "interpolation search" was shown to require $O(\log\log n)$ steps even under rather weak assumptions ...

I completed my Ph.D. thesis on "Rational approximations of irrational numbers" [4] under the expert guidance of Hlawka (based on Baker's hypergeometric series that became quite famous later on) in 65, and was starting to work more and more on the formal definition of programming languages in the IBM Lab. Indeed, I am one of the co-authors (together with Kurt Bandat, Peter Lucas and Kurt Walk) of an early version of the formal definition of PL/1 in 65 [3]. However, I found the work at the IBM Lab very frustrating: how can one describe a mess such as PL/1 in a neat, formal fashion? I quit my job and accepted an Assistant Professor position in Calgary. I must say that later I was very impressed when the Formal Definition of PL/1 was indeed successfully completed, and the technique became well-established as Vienna Definition Method: over thirty years later it is still one of the corner stones of formal definition methods. This incredible achievement is due to the genius and quiet persistence of Peter Lucas (now Professor at the Graz University of Technology), and due to the team-guiding skills of Kurt Walk (who, now retired, also teaches at Graz). When leaving IBM I remember one piece of advice from Zemanek that I did not appreciate then, but I do now: Zemanek, who knew that I was still torn between number theory and computer science, told me: "Make up your mind. You can only successfully serve one master, you can only successfully pursue one line of research." Of course, Zemanek was right.

I started to concentrate on formal languages. I compiled a large annotated bibliography and used it to write a German book on formal languages ("Theoretische Grundlagen der Programmiersprachen" [6]) that appeared in 1969. It became a German best-seller. Without even realising this, the book made me well-known in Germany, while I was just being promoted to Associate Professor in Calgary and was working on ambiguity problems in context-free languages. I believe that I gave the first simple proof that $\{a^n b^n c^m \mid n, m \geq 1\} \cup \{a^n b^m c^m \mid n, m \geq 1\}$ is inherently ambiguous [5] and that there are context-free languages that are inherently ambiguous of arbitrary degree. From Werner Kuich I learnt how to see such issues in a more general light using formal power series: we have co-authored a number of papers (e.g. [7]) and I was and I am impressed by Kuich's systematic and careful mathematical approach that often reduced "pages of handwaving" to a few lines of precise proof. Those of you who have ever looked at the lengthy "proofs" of equivalence between PDA's and CF languages in most books (including my own, Hopcroft and Ullman's classical one, or the one by Harrison) and compared it with the one-paragraph proof in the EATCS Monograph in the "Semirings, Automata, Languages" book by Kuich and Salomaa will have felt the same awe that I have felt! I feel almost embarrassed to report that in the late sixties I was convinced that if L_1 and L_2 are CF (like e.g. $L_1 = \{a^n b^n c^m \mid n, m \geq 1\}$

and $L_2 = \{a^n b^m c^m | \, n, m \geq 1\}$ and $L_1 \cap L_2$ is not (like $\{a^n b^n c^n | \, n \geq 1\}$ for L_1 and L_2 mentioned before), then $L_1 \cup L_2$ is inherently ambiguous. Indeed I spent weeks trying to prove this conjecture. When I mentioned it to Professor Seymour Ginsburg a few years later he almost had a laughing fit: he immediately wrote down a trivial counterexample! The book "A Collection of Programming Problems and Techniques" [8] that I co-authored with my friend Mike Williams (who later became famous for his work in the history of computing and his supervision of computer exhibits e.g., in the Smithsonian in Washington) appeared in 72, already after I had accepted the position of professor for computer science at the University of Karlsruhe.

Learning to cooperate (71 -76)

My peaceful pace of life changed as soon as I moved to Germany. I was suddenly surrounded by a host of ambitious colleagues and in charge of supervising half a dozen bright Ph.D.'s. Research emphasis was on formal language. My first joint publications in Karlsruhe were with my colleague Otto Mayer [9] (who later became a very successful professor and dean at the University of Kaiserslautern) and his excellent student Armin Cremers (who, after at stint in the USA is now one of the most influential professors at the University of Bonn) and with my Ph.D. student Hans-Peter Kriegel (e.g. [11]) (now professor at Munich). Both Mayer and Cremers have remained life-long friends, and we have co-operated on a number of completely different projects over the years. With Professor Klaus Neumann (who still is professor at Karlsruhe) I wrote my first "non-scientific" paper on computers and the like, and we both enjoyed it. The year 1974 turned into a decisive year for me. I had corresponded with Seymour Ginsburg, one of the "gurus" in formal language theory for a long time, and I had met him a few times at major conferences. When I invited him for two weeks to Karlsruhe for co-operation he accepted to my delight ... and I was in for a new experience. It was Seymour who introduced me to the work style "spend as much time together as you can for 1-3 weeks ... by that time your are bound to have enough results for a nice paper." After working with Seymour for a week I felt like I was married to him: from breakfast to after dinner we would be together and "talk shop". I could not believe how successful this mode of operation can be, nor could I stand it after 6 times 16 hours daily any longer. I decided we (mainly I) needed a break. I took my family and Seymour for a hike to a small lake in the Black Forest. The weather was lousy but anything was OK to get away from formal languages for a few hours. However, no sooner had we got out of the car when Seymour pulled out a piece of paper and we continued working as we walked (me holding the umbrella for both of us) to the lake and back. (I am deeply grateful that my wife Ursula and the kids put up with me in situations like this. If they ever read this: thanks!) Anyway, Seymour and I wrote a number of papers on "grammar forms" (e.g., [12]) on this and later occasions and I am indebted to him for teaching me a kind of co-operation which would be my main mode of operation for many years. Let me tell a little bit more about Seymour

Ginsburg: he does not need an introduction to my generation . He was one of the giants in formal language theory in the US; his books on Automata Theory and the Theory of Context-Free Languages remained classics for a long time, and his numerous papers and his notions of AFL's, AFA's and grammar forms shaped formal language for over a decade. Grammar forms were one of the latest features blooming in formal languages in the US (till about the late seventies): by defining certain morphisms on the production rules of grammars, each grammar (form) G gives rise to a family of related grammars $\mathcal{G}(G)$, and thus to a family of languages $\mathcal{L}(G)$ by defining $\mathcal{L}(G) = \{L(G') | \, G' \in \mathcal{G}(G)\}$. Thus, grammar forms are a good tool for studying families of related languages. The "fate" of formal language theory in the US is typical for developments there that one can either smile about or disapprove (I do the latter): a group of researchers comes up with some new concepts; they produce good results; they oversell their applicability to get large research funds; many other researchers join the band-wagon; for a while all research outside this area is considered "obsolete" or "irrelevant"; at some stage disillusion sets in; the topic stops to be fashionable; another area starts to be THE in-thing to do; an analogous cycle starts all over again. I do not disapprove of researchers overselling the applicability or importance of their results: this has always been done. Have you e.g., seen the proposal of Leibnitz for a new language as cure-for-all as recounted in Umberto Ecco's book "The universal language"? What I do not like is the strong "fashion trends" that dictate what is "in" and "out" in North America. And if you are not working in an "in" area your chances for research funds, recognition or good job offers are close to nil. I am happy that Europe is less radical, this way.

It was around 1973 when I first met Derick Wood (then at McMasters in Canada, now prestigious professor at the top university in Hong-Kong): we started to co-operate both in formal languages (e.g., [13]) and algorithms for data structures. I will have to say more about Derick later, but let me mention that at this time Thomas Ottmann (now professor at Freiburg) and Hans-Werner Six (still a good friend and now professor at Hagen) joined my group as assistants in Karlsruhe. In a joint paper [15] we introduced a class of search trees that Ottmann later improved to the now classical brother-trees, one of the most elegant search structures ever invented. Indeed, I have always admired, and certainly still do Ottmann's sharp brain, endurance when working on whatever project, and his reliability that is hard to describe if you have not experienced it: if he says "yes, I will do this by date x" you can be sure to have it by date x-2, the latest. During my 5 years as professor in Karlsruhe the other full professor appointed in my institute was Wolffried Stucky: we published material on using syntax-directed techniques for programming purposes [14]. However, what has impressed me most about Stucky was his quiet humour, systematic organisational work, his ability for co-operation and for putting up with me when I had a flare-up of bad temper or some crazy idea. If I have to ever choose someone to run a group together with, Stucky will always be my first choice: the fact that he continued successfully building up the group in Karlsruhe after I left and times where not always that good, and the fact that he later obtained many honours

and became president of the GI (the German Computer Society) says also a lot about his friendly yet in the end decisive personality.

During 75 I got to know one other very fascinating person, Harald Hule. He is an Austrian who had discovered his calling for mathematics only at the age of 28, but then studied and completed his Ph.D. in 4 years (!). He went to Mexico and Brazil for some years thereafter and it was in Brazil where I met him: he helped me understand a bit of Brazilian culture and life-style (thanks Harald!). I invited him to Karlsruhe as visiting professor. And although he had never ever worked in formal languages before, within three months he was the main author of a paper on OL forms [22]!

However, the most important thing that happened to me in the period under discussion occurred at Oberwolfach in 1972, this beautiful retreat where mathematicians (and then also theoretical computer scientists) meet in the idyllic setting of a Black Forest village to work on and discuss some special topic for a week. My long-term friend Werner Kuich who I mentioned before, introduced me to Arto Salomaa from Turku, Finland. Salomaa has been shaping theoretical computer science in Europe since around 1960 and, if this is possible, his scientific output and influence continues to grow even more every year. But this is besides the point: the main aspect for me is that we became close personal friends, not just working together but also in many other ways. The last 20 years would not have been the same without my friendship with Salomaa that continues although from a research point of view I have started to follow other paths over the last years. I think this is the right time to say it: I love my profession for the chance it provides to meet, work with and make friends with not just people, but personalities. Some persons you start to respect or to admire. Others you like for their humour or their idiosyncrasies. And with some you develop a bond akin to members of your core family. Arto Salomaa, or "Tarzan" as some of us call him, belongs to the last category: working with him never felt like work but was a pleasure. I love to think back to the beautiful times we had working and relaxing at his kind late sister Sirkka's place Lauttakylä or at Arto's Rauhala (both an hour's drive from Turku), or in the Black Forest, the mountains of Austria, or the old city of Graz in Austria.

One of the highlights has always been visiting Rauhala - Arto Salomaa's country home - with a rustic sauna that may well be the best in the world. I have very very fond memories of my stays, usually also recorded in a "Sauna poem" in Arto's book of visitors (see the contribution "Events and Languages" by A. Salomaa in this book for further examples). My last entry there (April 1997) goes like this:

In Salosauna, once again
I sat; and I saw plain
that life is more than Hyperwave[1]
that everything and all, I have

[1] For the meaning of Hyperwave see the section "Applications ... and some theory (1981-1997)".

accomplished doesn't mean that much
compared to friends, and love, and such.

In Rauhala, the days are sweet
when Tarzan, Jane and peace I meet
so let me sing as praise this song:
I missed this place and you for long
and thanks for sharing once again
some thoughts of joy, of fun and pain.

Work with Arto Salomaa and Derick Wood who was in our group from the beginning was quite productive: we co-operated as a team on over 30 publications between about 1977 and 1983. This period was probably my most interesting time as theoretical computer scientist, so I spend a separate section on it.

Before turning to the next section let me mention one item that I was able to do for the Theoretical Computer Science community that I am proud of and that is almost forgotten: I was EATCS Bulletin Editor from 1977 to 1981, from no. 3 to no. 14. The first two numbers were few-page leaflets, and even no. 3 was still small enough to be stapled. The current "book-like" look for which the Bulletin is now famous for and that helped to grow membership quite a bit started with no. 4 and was perfected later by Rozenberg. Even the circular EATCS symbol dates back to those days when I was Bulletin editor!

The MSW years (1975-1981)

Although I co-authored quite a few results outside formal language theory with then (or later) very prominent researchers, and I also started to dabble in more applied areas, the most decisive influence in those years was the work with Salomaa and Wood (and we were proud to be called the MSW-team after our first successes). Our work started by combining two areas that were "red hot" in those days: grammar forms (as mentioned earlier) and L-systems. L-systems are the invention of the famous late theoretical biologist Aristid Lindenmayer who noted that growth-processes can be described in a natural way by applying production rules much like in ordinary Chomsky-type grammars, but applying the rules simultaneously to all symbols of an intermediate string (representing a linear arrangement of cells of some organism), rather than to just a symbol at-a-time as is done in the usual Chomsky-type derivation process. It is curious that this idea that gave so much impetus to the study of formal languages came from biology rather than computer science. But far-sighted computer scientists such as Grzegorz Rozenberg, Arto Salomaa, Gabor Herman and others soon recognized its importance to language and automata theory; thus "L-theory" started to explode as synthesis between biology and computer science.

The first MSW paper on EOL Forms [17] combined ideas from grammar forms with EOL systems, the "L equivalent of CF grammars", as one might say. The paper laid a solid foundation for what we thought could turn into a major

field of study. Two further papers followed. All three received good reviews and were accepted for publication immediately. Then, when working on MSW paper number four [23] we ran into a crisis that I will never forget.

We had, by that time, established our MSW routine: two of us would meet, work on a topic all of us had agreed on, write up a sketch of the paper including all proofs, etc. The third one would critically read and amend the results, and do a first draft of the final version of the paper. This time, it was Arto and myself doing the groundwork on a new paper at Turku. On the second day, our work started to bog down: when Arto came up with a suggestion, I would find a counter-example; when I proposed a possible result he would show that it could not be true. We continued this Ping-Pong "game" for hours, frustration growing. Suddenly we stared open-jawed at a counterexample to the first lemma in our first paper [17], a lemma all our work (and three already accepted papers) had relied on: We checked the proof of that first lemma. It said laconically "Trivial". Intuition had badly tricked us and all referees! I am sure you can imagine how we felt: all previous work going down the drain; three papers based on a wrong result about to appear in print! I was at a complete loss. At this point Arto said something I will never forget: "I think it is time for a long sitting in sauna."

This is what we did. And the Finnish proverb that "Sauna opens your brain" proved correct: after two sittings with excellent löyly it was clear that (a) the critical lemma was indeed wrong and (b) a weaker version of it could be proven and was enough for all our purposes.

We were able to correct the first three papers before they were printed with a sigh of relief. And indeed, the fact that the original lemma 1 was incorrect established that L-forms were not just a variant of grammar forms, not just a cute little new island of knowledge to explore, but a new continent with entirely new phenomena, as striking to us as Africa with its elephants and giraffes must have been to early explorers (if you permit me to stay with my geographical metaphor).

MSW work flourished, and L-forms have become part of today's classical formal language theory.

It is with deep gratitude to both Arto Salomaa and Derick Wood for the wonderful co-operation, never marred by any rivalry, envy or what have you. Wood turned out to be the master of looking at obstacles from so many different directions, chipping away at the problem until it dissolved into nothing or until a real hard core would remain. And then Salomaa would take over, sit and think for a long time and finally say: "Maybe we should try the following: ...". And then it was already clear that he had an outline for a new route that would eventually succeed.

It was during this time that I also got to know Karel Culik II better, and we started to co-operate. Karel is one of the persons I know with a terrible sharp (but sometimes impatient) brain ("come on, we fill in the details later"), who has an infinite amount of energy, always willing to prove that he is better than you are (in research, tennis, chess, ... you name it) ... and usually he is. Before we did our first joint paper I had learnt to admire him for showing that the DOL

sequence equivalence is decidable. If the following report on how this happened is not correct, forgive me, it really does not matter: it is certainly typical for Karel.

Karel had not done much on L-systems, when the wave started. However, when he attended an L-conference he heard a talk on the above mentioned problem: given two words w_1, w_2 and two homomorphisms h_1 and h_2 can you decide if ${h_1}^n(w_1) = {h_2}^n(w_2)$ for all n (where h^n means n-fold iteration of a homomophism h)? This question sounds deceptively easy, yet is quite deep. At the end of the talk, Karel got up and said he could prove decidability. To a stunned audience he gave a sketch of a proof: although intriguing, the proof contained "large holes", as Karel was ready to admit. "But these are just details that I will fill in till tomorrow". Well, the proof was refined next day by him, yet many gaps remained. From there on, Karel kept improving and detailing his "proof" many times, much to the chagrin of some colleagues who got more and more exasperated by having to wade through more and more complex arguments and - at the end - still discovering gaps filled by "handwaving". I remember a letter by Arto Salomaa that he was about to study the last version of (and for the last time) a "proof" of the decidability of DOL sequence equivalence by Karel. However, this time the proof was "water-tight". Karel had indeed solved this very hard problem ...

Thus, it was "natural" that I wanted to start with simple topics such as e.g. [18]. Soon we ended up in deeper things like [20] or [21], the latter also with co-author K. Ruohonen, then one of the many top-notch assistants of Salomaa. Working with Karel was both exhilarating and frustrating: Karel always seemed to see solutions (like in the case of the DOL sequence problem) very fast, but just ignored (or considered trivial) gaps in the proof. In working with Culik I understood the first time very clearly: one can co-operate even if one "functions" very differently. With Culik, my only function was to punch holes in his arguments; he would fix them, I would find problems, he would refine his proofs: he was the brain, I just a humble critic ...

Grzegorz Rozenberg (or Bolgani as his friends call him) whom I had the pleasure to meet first during those years is just about as much the opposite of Karel Culik II as can be. Where Karel can be abrasive, Bolgani is gentle. Where Karel stubbornly pursues one problem at a time, Bolgani sees a vast array of problems and possibilities, too large to explore, so he concentrates on new ideas and fields with a staggering amount of energy and imagination. He is the best "salesman" of scientific ideas I have ever met, presenting difficult material in such a superb way (almost like a magician) that one feels compelled to listen and to appreciate what he has to say. It has been an honour to be accepted by Bolgani as friend: I know that I will not be able to ever repay him for his generosity, and for the open and warm way we have co-operated scientifically and in other ways. It was also through Bolgani that I have co-authored a paper with the famous A. Ehrenfeucht from Boulder [32].

This period of my life has also been rewarding by being able to work with bright young assistants, first at Karlsruhe, Germany, later at Graz, Austria. I

have reported about some already above, but I must also particularly mention Jürgen Albert who wrote an excellent Ph.D. thesis, with whom it was a pleasure to co-operate (e.g., [19] and [27] who has been now professor at Würzburg, Germany, for a long time and whose quiet and gentle ways combined with excellent work continues to impress me. One of my last excellent Ph.D's in formal languages (already in Graz) was Werner Ainhirn [26], who later left for work in industry in Germany, but who has returned to Austria in the meantime. As luck has it, we are now cooperating on a substantial applied project.

During my first years in Graz (starting in 1978) I also had the chance to invite and work with many visitors, some of them already famous, some rising stars. My respect, if not awe, for e.g. Maurice Nivat from Paris had always been tremendous: to actually work together with him and publish successfully on rational transductions (e.g., [29]) has been a definite highlight. As my interest slowly extended from formal languages to the theory of algorithms (particularly concerning region location problems) I had the pleasure to get to know John Bentley (e.g.[24] and [28]) later e.g., famous for his "Programming Pearls" who was a hit with the students in Graz when he came to our offices and to stodgy, conservative professor Hermann Maurer on his skateboard; I am particularly proud of the papers [30] and [31] with Thomas Ottmann and Jan van Leeuwen, since I believe they constitute the first systematic approach to provide efficient solutions for the dynamic versions of problems for which up to then good techniques only for static cases had been known. While I also published quite a bit with members of my institute such as R. Frey, V. Haase, J. Stögerer, W. Bucher, G. Greiner, H. Mülner, I. Mischinger, F. Haselbacher, P. Lipp, H. Cheng, J. Theurl (now vice-president of Graz University of Technology), W. Jaburek (who received two Ph.D.'s in Law and in Computer Science and has been an influential force in computer science laws in Austria for now over ten years), and G. Haring (who later became professor at the University of Vienna and Head of the Austrian Computer Society) and short-term visitors such as Detlev Wotschke who later became professor at Frankfurt [33], I.H. Sudborough (professor at the University of Texas at Dallas) [35], D.G. Kirkpatrick [39] from the University of British Columbia at Vancouver, and both famous Franco Preparata and Arni Rosenberg [42], and while I also did some non-scientific publishing on my own and with others including my wife Ursula, I cannot go into detail but will just concentrate on one further aspect: two of my last theory Ph.D.'s in Graz turned out to be particularly talented:

In the process of writing their theses they started to surpass my knowledge and talent in this area and have become leaders in their field: Emo Welzl, now professor at the ETH in Zurich, Switzerland, and Herbert Edelsbrunner, who has been professor at the University of Illinois at Urbana, for some ten years. I am proud to know that I helped them a bit early in their career; it is both exhilarating and humbling to see how fast talented students develop. I got to know Herbert in a second year course on data structures where I followed my books [10], [16], presenting well-known material with well-known proofs. It happened over and over again that Herbert would ask during my classes "could one

not also see this in the following way?" ... and such question invariably meant that he had discovered some inaccuracy or mistake in a proof that had been around for years. We started to work together when Herbert was just a bit over twenty years old (e.g. [37], [38], [42]). By the time Herbert was finishing his Ph.D. visitors often came to Graz to see him, rather than me ...

Emo Welzl was initially working more on formal languages and discrete mathematics. Already his early contributions like [34], [35] or [41] showed his talent; his career went rapidly upwards: he became young professor at Berlin, obtained prestigious awards and distinctions and was soon one of the stars in German computer science before moving to the famous ETH in Switzerland.

Looking back at those days when I was mainly working in theory I must say it was a great time; the talent of some students in both Karlsruhe and Graz was indeed impressive and it was a pleasure to see them "grow".

Let me finish this section with one of the more embarrassing stories of my theory days. In [25] we tried to show how to use single public key pairs to allow various overlapping groups secure access to information. The aim (to avoid having a special set of keys for each class of information one is entitled to access) is important, and the paper got quoted a few times. However, two years after it had appeared a gap in our argument was found showing that the proposed method was not safe. Of course, we published a corrigendum (basically much weakening our earlier "results") but I can just hope that nobody has seriously used the method proposed. The fact that in electronic versions one can make an addendum in the spot where the mistake occurred is one of the reasons why I believe today in electronic (Internet) publishing (see [67] or [76]) and http://www.iicm.edu/jucs_annotations, and I am happy that Cris Calude and Arto Salomaa decided that we three together should get started in this direction in 1994. I will have to say more about this in the latter part of the next section.

Let me finish this section with a story of my early theory days that might destroy my credibility as good organiser, but it is time to tell. I was responsible for organising ICALP'79 (the sixth ICALP) in Graz. The reception was to take place in the best setting Graz has to offer: the Renaissance castle Eggenberg, lit by 3.000 candles in crystal chandeliers (electrification would destroy the murals!), local food and wine served by pretty young restaurant-trainees, sit-down dinner with the provincial governor.

As recent arrival I asked a more senior member (let me call him Mr. X) of the organising committee to make sure that we would have a reception on Monday evening. At each meeting I asked Mr. X whether everything was OK with the reception. Every time (I can prove it through the minutes of the meetings!) the answer was "yes", yet I never received a written confirmation from the government.

When the program was already printed I got nervous and phoned the secretary of the governor. "Let me check", he said. "Yes, everything is fine, a dinner reception with the governor is scheduled for your group for Tuesday evening." I was appalled: "You mean Monday evening." "No, sorry, Monday is impossible, there is a concert scheduled in the same rooms."

I was at a loss what to do: we had, of course, another event scheduled for Tuesday! I ran to the office of Mr. X and told him furiously that he bungled the program. He listened patiently, sighed, took the phone, called the ticket reservation agency. "How many tickets have you sold sofar for that concert?" "Two", was the answer. "Great, I am Mr. X., I take the remaining 198 tickets".

Thus, two surprised tourists got a a free dinner after the concert they had booked. And many colleagues complemented me and asked with surprise how I had managed to throw in a high-class concert free of charge ...

Applications ... and some theory (1981-1997)

Around 1981 I started to work more and more in applied areas of computer science. I have never quite given up theory, as recent publications like [68],[70],[71], or [88] show. Yet, my emphasis has shifted. Following the spirit of this book that is dedicated to theory, but also following my intention to mention all the many persons I have had the pleasure to co-author papers with I will give as compromise a fairly tense description of this comparatively long period.

Shortly after I moved to Graz I got involved in a study for the Austrian government whether Videotex (also called Prestel in UK, Minitel in France, Bildschirmtext in Germany, ...) – a TV/telephone/modem-based distributed information system – should be introduced also in Austria. My recommendation was yes, but the network should be seen as a network for special network PC's rather than for "dumb" videotex terminals. In particular, those network PC's should be able to handle vector based colour graphics and the execution of code just downloaded. We called our network PC's then "intelligent videotex terminals" rather than Net Computers (NC's) as they would now be called, and while we called the downloadable executable software "telesoftware" today everyone talks about JAVA (applets), really just a variant thereof. The recommendation to develop such a more modern version of Videotex was accepted by the Austrian Telecommunication authorities. Since no suitable PC's existed (this was before the time of the first IBM PC, remember!) my group got suddenly shouldered with the development of a dedicated terminal. Without any hardware knowledge to speak of I could not have done the job without my brilliant assistant Reinhard Posch who designed the hardware and supervised most of the system software of the device we called MUPID: officially this stood for "MultiPurpose Intelligent Decoder", but insiders know that it stood for "Maurer Und Posch Intelligenter Decoder". One of my best friends said at some stage that it really means "Maurer's Undertaking Puzzles IBM Directors". Anyway, MUPID was quite a hit for a few years (there is no Austrian developed computer that was ever produced in similar quantities, a total of some 35.000, almost 40% in export), but the emergence of the IBM PC's and clones and the departure from Videotex towards more open standards as we see them today in the internet was the end of MUPID as hardware product. However, MUPID team members continued their work by founding a total of 15 new IT-companies in Graz with today some 250 employees. The MUPID years and all the turmoil of going commercial would warrant

a separate chapter except that there is not so much theory involved in all this. The most powerful person, and the person who I count as one of my best friends in Graz, is Reinhard Posch. He has been now full professor at Graz University of Technology for over ten years and is internationally recognized for e.g., his work on computer security. Co-operation with him has always been a pleasure, see e.g. [44],[45]. The MUPID and Bildschirmtext efforts brought also interesting publications with other team members, such as Heidrun Bogensberger, Walter Schinnerl, Gerhard Greiner, Walter Jaburek, Helmut Mülner, Günther Soral and particularly Dieter Fellner: Dieter later went to Canada for some time (to St. John's, Newfoundland) and returned as professor to Bonn, Germany. There he has built up a sizeable and recognized group in the areas of computer graphics and electronic publishing. It is a pleasure to say that the contact with the original core "MUPID Team" is still intact, and that co-operations on many levels still continue.

During this time I was also consultant at IIASA (International Institute for Applied Systems Analysis) for two years. This was the Austrian attempt to diffuse the then "cold war" by bringing researchers from East and West together for shorter or longer stints of co-operation in the former summer castle of empress Maria Theresia just outside Vienna. It was there that I met Istvan Sebéstyén from Hungary and Wolf Rauch. The former joined my group in Graz for some time at a later stage, the latter became professor at the University of Graz and in 1997 its president. Sebéstyén was marvelously good in digging up facts and combining them, see e.g., [36] or the paper [40] that we co-authored with J. Charles from the Institute for the Future in California. Wolf Rauch is an absolutely unique combination of researcher, philosopher, organiser and diplomat, and interested in all kinds of questions, see e.g. [43]. It is a real joy to work with him and have him as friend ... and we have done some unusual things together! Like at the Hypermedia 1991 meeting in Graz [57] when we had a public discussion "Pro and Contra Hypermedia": One of us had to take the "pro" point of view, the other the "contra". But the chairperson Jürg Nievergelt from ETH Zurich had the audience vote before we started who would be "pro", who "contra". (I ended up "contra" and it was not easy to stay my ground against an opponent as skilled in arguing as Rauch is!) [59].

My long-term friend Wilfried Brauer (the founder of computer science in Hamburg, and now professor in Munich) sent one of his students, H. Cheng, to Graz resulting in a number of papers, e.g., [46].

My interest in non-mainstream computer science also resulted in a joint paper [47] with Norbert Rozsenich, who as vice-minister for research for over 20 years has been shaping research policies in Austria more than any other individual. I have found Rozsenich's support, imagination and frank criticism always very refreshing ... and I think I learnt also a bit from him how to deal with politics (unfortunately not enough!).

Starting in 1985 my interest in using computers for teaching and learning continued to grow. After early work with H. Huemer, Peter Sammer, and Dana Kaiser, co-operation with the late John Garratt from Control Data, Germany

(a fruitful and fun co-operation made easy through John's stamina and humour) started to lead to significant projects culminating in COSTOC (Computer Supported Teaching of Computer science). This was an interesting period consisting of three aspects: (1) Implementational work with e.g., P. Lipp. J. Nagy, John Garratt, and others; (2) co-ordinational work as editor of a series of courseware modules (with prominent authors such as Arto Salomaa, Gerhard Barth, Thomas Ottmann, Peter Widmayer, Herbert Kraus, Henry Shapiro, Egon Börger, Vladimir Stepanek, and Peter Warren ... (just to mention a few), and (3) work on the boundary between applications and theory. For the purpose of this book it is appropriate to dwell a bit more on the last point.

This semi-theoretical work was carried out with e.g., my assistants Fritz Huber (see e.g. [51]), Robert Stubenrauch and Ludwig Reinsperger, but also with my colleagues Thomas Ottmann from Freiburg and Fillia Makedon, then Dallas now Dartmouth, e.g. [48], [50], or [52]. I particularly enjoyed working with brilliant, imaginative and enthusiastic Fillia Makedon: I have learnt a lot from her, particularly looking at things from a point of view as general as possible ... and she also converted me to a fan of her home country Greece (but it takes little to become a fan of Greece: if you aren't one yet, just go there!) A period as adjunct professor at the University of Denver also enabled me to become Ph.D. supervisor of sharp-minded John Buford-Koegel, now at the University of Lowell, Massachusetts [49].

Paul Gillard from St. John's, Newfoundland, and Mike Stone from Calgary, Alberta, came to Graz as visiting professors. Both are long-term friends who have shown me more beauty and serenity in Canada than I can describe in a few lines, but I cannot suppress my urge to mention one or two events. Like when Paul Gillard took me on a multiday fishing trip into real wilderness: as we were camping on an island in the stream, and evening fog was rolling up the river from the sea, the sight of a dinosaur appearing around the corner would have not much surprised me; or like when Mike Stone took me telemark-skiing in 15 foot powder snow in Western Canada and we started to get caught in a blizzard: I reciprocated by taking Mike a year later on skis up a mountain where we had to make our way down through dense, steep forest and on just patchy icy snow .... Both with Paul and Mike we worked together on teaching aspects, e.g. [53] and [56]. Short-term visits of my good friends Gordon Davies and Jenny Preece (then both at the Open University in the UK where Gordon still is) resulted in e.g. [55]. And it was the first longer meeting with Pat Carlson from Rose-Hulman Institute of Technology in the USA that crystallized my "missing organ thesis", later published in e.g. [60]. I believe this thesis is simple yet interesting enough to briefly review it here: our ears are passive instruments (they only receive sounds); we have an active counterpart (our mouth); the eyes are also passive instruments (they can just receive pictures); but we do not have an active counterpart, no "mouth for the eyes", no "picture generating organ" (= the missing organ) that allows to easily convey (mental) pictures from one person to another. Looking at this phenomenon more closely leads to two conclusions: (a) the missing organ is deeply influencing our communicative behaviour and (b)

we should try to develop a prosthesis for the missing picture generating organ much as we have done for other missing organs such as wings or gills. Some work on this is still in progress. In connection with this and computers and teaching I also have co-operated with some of my learning-theory and cognitive-psychology colleagues, particularly Ricky Goldman-Segal from Vancouver and Dave Jonassen from Pennsylvania [84].

Despite all efforts that have gone into the development of Videotex and computer assisted instruction all over the world and in Graz neither area managed to achieve a real breakthrough in the eighties. Around 1988 I started to form a group to analyse what future networked multimedia systems would have to look like, networks that would work better than Videotex and would solidly support educational aspects.

Previous work, particularly with Fillia Makedon, Reinhard Posch and the MUPID and COSTOC efforts had yielded some insights. The wave of hypermedia efforts at Brown and MIT yielded further. As luck would have it other factors in the form of three brilliant scientists helped our efforts in Graz, tentatively code-named Hyper-G: Ivan Tomek from Acadia University, a bit later Nick Sherbakov from St. Petersburg, both visiting Graz for an extended period (Nick actually deciding to stay for good) and particularly Frank Kappe, then one of my ambitious and ingenious assistants who soon became project leader. Ivan Tomek's quiet and systematic work brought the theoretical underpinnings of Hyper-G to a good start. Also, it was and is a pleasure to co-publish with Ivan: after a few hours of brainstorming we often end up with enough ideas for more papers than we can possibly handle. Using Ivan's great skills to compose excellent papers once the basic ideas are clear we managed to co-author over a dozen publications within two years (this must be close to a record!), e.g. [54], [58] or the paper co-authored with M. Nassar [63].

Nick Sherbakov brought with him deep knowledge in database theory and data modelling that resulted in a host of valuable ideas and joint papers, some co-authored with P. Srinivasan or Ann Philpott, and others such as e.g., [62], [65], or [72]. The driving force behind the modern JAVA-authoring tool HM-Card [85] is also Nick Sherbakov. It was Frank Kappe's Ph.D. thesis that gave the first fairly rigorous specification of what future networked multimedia systems (like today's Hyperwave, the successor of Hyper-G) must look like. Good introductory papers co-authored by the rapidly growing Hyper-G team around Kappe, including both capable researchers and developers such as Keith Andrews, Klaus Schmaranz, Gerald Pani, Florian Schnabel, Jörg Faschingbauer, Mansuet Gaisbauer, Michael Pichler, and Jürgen Schipflinger are [61], [62] [75] [77], and the book [86].

Parallel with above activities I had the pleasure to help establish the Interactive Information Center (IIC) in Graz in a paper co-authored with famous media "guru" Don Foresta from Paris, the well-known Styrian philosopher Johann Götschl, and Wolfgang Schinagl, the real "motor" behind IIC under whose guidance IIC has developed in four years into a top-notch IT show-case.

Another important stage in my life was my (temporary) move to Auckland, New Zealand, in 1993. With my two first Ph.D. students there, Achim Schnei-

der and Jennifer Lennon, we managed to very successfully pursue a number of topics in networked multimedia. I was particularly impressed by the impeccable work of Jennifer Lennon who has become a very good personal friend, one of the leading personalities in multimedia in New Zealand, and a prolific co-author, see e.g. [64] or [73]. Other publications from my time in Auckland are with Achim Schneider (e.g., [80]), with L. Rajasingham and John Tiffin [66] from Wellington, Julian Harris [74], Barry Fenn [78], Bill Flinn [79], Channa Jayasinha (the IT director of New Zealand's main museum in Wellington) [82], the German student Michael Klemme [89], and particularly Professor Cris Calude who rekindled my interest in theory [68]. Cris and Arto Salomaa were also "responsible" for convincing me to start an electronic journal J.UCS, mentioned earlier. See http://www.iicm.edu/jucs and [67] for more technical information. The further technical development of J.UCS is now much in the hands of one of my top Ph.D. students Klaus Schmaranz, see e.g., [76] but has also stimulated co-operation with my friend Gary Marchionini from the University of Maryland, see e.g., [81].

Arto Salomaa turned 60 in 1994. There were a number of big festivities for this occasion and I was lucky to be involved in two. I hosted an international meeting for Salomaa in Graz where the proceedings where edited with my friends Karhumäki and Rozenberg [71], and I was invited to be co-editor of the Salodays proceedings [69] with Cris Calude and Mike Lennon in New Zealand. I have to say a bit more about Mike: he took me on a number of out of the world tramps (as Kiwis usually call hikes!): two days underground; three days wading in water (in the absence of trails and with dense forests you have to walk in the river-beds); through beautiful NZ South island mountain scenery; up an active volcano with winds raging at over 100 km/h; bivouacking at the snow line just with sleeping bags with no way to get a fire going; and much more. And all this sprinkled with the occasional talk about some mathematical problems. Thank you Mike for being such a terrific guide, friend, ... and cook: even under extreme circumstances Mike manages to whip-up an incredible hot stew in a short time. Mike organised something very unusual for Arto Salomaa: not just a native Maori feast, a hangi, but a very special one where Arto, in a touching ceremony, became member of that particularly Maori tribe: I believe there are very few Europeans who have this honour. This is much deeper and much more serious than it sounds: Arto's tribe now considers Arto a member and will support him, if it came to it, from now on no matter what.

Due to a Fulbright scholarship that I managed to get for Auckland I got to know Professor Suave Lobodzinski from California. His scientific vitae had impressed me. But I had not known that Suave is also a top mountaineer (he has been on Mt. Everest without oxygen), is a dive master, and a dynamic person to a degree that is unbelievable. I am lucky that Suave has let me profit from his friendship and experience in outdoor situations that are borderline for me, yet trivial for him. We are also co-operating on medical applications of Hyperwave, see e.g. [83].

My main interests these days are in using Hyperwave: you see, Hyperwave is the first theoretically sound WWW server: it has a database, search scopes, automatic link maintenance, customisation features and much more. B.T.W. it is free for university institutes (see http://www.hyperwave.com) and ideally suited for educational applications (see http://www.iicm.edu/mankind). This is also the reason why we are co-operating with a number of educational groups, particularly with the one around Professor Manolis Skordalakis from the Greek National Technical University in Athens, see e.g., the paper also co-authored by A. Koutoumanos, N. Papaspyrou, and S. Retalis [87]. Skordalakis is a true Greek friend and gentleman. So it is typical that he proposed the acronym GENTLE (General Networked Teaching and Learning Environment) for a Web based training project and permitted me to use it in the future. Manolis has been leader of a successful European Web-based training project, EONT, for some three years. Working under his guidance has been a pleasure. Thus, we had nostalgic feelings at our last joint meeting in Athens in June 1997; and the description of the last evening is a fitting ending also to this report: here we were, in a roof-top restaurant at the foot of the Acropolis. The red sun setting, a gentle evening breeze stirring, the moon rising over Herodot's ancient theater. And many ideas for future work being discussed with growing excitement.

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Note. This is a very partial list of papers that I have co-authored with some of the persons mentioned in the body of the paper. For a full list see http://www.iicm.edu/maurer.