An interview with
FRITZ BAUER

Conducted by Ulf Hashagen
on
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at the Technische Universität München.

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ABSTRACT:
Professor Friedrich L. Bauer describes his career in physics, computing, and numerical analysis. Professor Bauer was born in Thaldorf, Germany near Kelheim. After his schooling in Thaldorf and Pfarrkirchen, he received his baccalaureate at the Albertinium, a boarding school in Munich. He then faced the draft into the German Army, serving first in the German labor service. After training in France and a deployment to the Eastern Front in Kursk, he was sent to officer's school. His reserve unit was captured in the Ruhr Valley in 1945 during the American advance. He returned to Pfarrkirchen in September 1945 and in spring of 1946 enrolled in mathematics and physics at the Ludwig-Maximilians-Universitäat, München. He studied mathematics with Oscar Perron and Heinrich Tietze and physics with Arnold Sommerfeld and Paul August Mann. He was offered a full assistantship with Fritz Bopp and finished his Ph.D. in 1951 writing on group representations in particle physics. He then joined a group in Munich led by a professor of mathematics Robert Sauer and the electrical engineer Hans Piloty, working with a colleague Klaus Samelson on the design of the PERM, a computer based in part on the Whirlwind concept. By 1955, the PERM was complete and he continued work that he had begun in 1951 on concepts in automatic programming. This led to work with Heinz Rutishauser and eventually work on the development of ALGOL60. Professor Bauer completed his habilitation dissertation at Munich in 1954, working on quadratically convergent methods for determining the roots of polynomials, and became a lecturer at the Technical University of Munich. In 1958, he took a professorship at Mainz, working on a Z22 and a faster Siemens machine. In 1963, he returned to Munich, taking up a new full professorship, where the focus of his work turned to computer science, programming languages, in particular. Finally, Professor Bauer reviews his work in numerical linear algebra which started with his PhD advisor, Fritz Bopp, asking him to calculate eigenvalues of matrices and continued beyond his habilitation dissertation on methods for finding roots of polynomials. He worked on numerical analysis related to engineering problems such as stability of plates and power networks and continued his work on eigenvalue problems and characteristic roots. In the latter case, he had contacts with Heinz Rutishauser and Alston Householder, among others. After 1968, his research turned primarily to work on programming languages and computer science.

08.12.2005
BAUER

*This is an interview conducted on Wednesday, July 21, 2004, on Monday, July 26, 2004 and on Monday, August 9, 2004 as part of SIAM's project on Oral History in the field of Numerical Analysis. The interviewer is Ulf Hashagen of Deutsches Museum in Munich. The interviewee is Professor Friedrich L. Bauer of the Department of Computer Science [Informatics] at the Technische Universität München.*

## EARLY LIFE: 1924–1945

ULF HASHAGEN
Now the first thing I would like to do is to get you to say a little about your family background and your school career. You were born in Regensburg, Bavaria?

FRITZ BAUER
Yes, this is right. But it does not mean that I am from Regensburg, it just happens that my mother was hospitalized in Regensburg at that moment when I was about to be born. Actually, I did not stay much longer in Regensburg than was absolutely necessary.

ULF HASHAGEN
Would it be possible for you to tell us a little more about your family, about your father and about your mother?

FRITZ BAUER
Yeah. My mother, Elizabeth Hedwig, née Scheuermeyer, married my father in 1923. My father, Ludwig Bauer, was an accountant, a bookkeeper, in a little place, Thaldorf near Kelheim; that is again near Regensburg, at the Danube. I am a German citizen. I am Roman Catholic. And in this little place where we lived, I had a very good childhood in a nice romantic area. For example, the Monastery of Weltenburg was a distance of just one hour away, and I went with my parents frequently to the Sunday mass there in a famous baroque style church. So I have a very good memory of those early years. In fact, my school teacher – I was sent to a *Zwergschule* (in German), to an elementary school where all the people from the youngest to the oldest were in one room and had one teacher. And this was a very interesting teacher I had. I loved him; he fascinated me. He was a man who could do everything that I understood at that time: he could play the organ, he

could play the piano, he could sing, he could make drawings, he could make a nice little gift. He was a man to look up because he was a sorcerer. I have not very often found a man who has made such a big impression on me since this schoolteacher.

ULF HASHAGEN
Do you remember his name?

FRITZ BAUER
His name is Hans Gumberger.

ULF HASHAGEN
For how many years were you his schoolboy?

FRITZ BAUER
I was born in June 10, 1924. I came to school in April 1930. And I stayed in this place – that is my parents stayed in this place – for about two and a half years after I came to school; that means my parents left this place in fall 1932. I was at the third class of elementary school, and in the middle of the school year my parents left. My father got unemployed due to the economic situation that in 1932 happened to be around in Germany, and he decided that he would become independent, that he would build up his own business as a book revisor and as a tax counselor –

ULF HASHAGEN
That means a guy who advises people about –

FRITZ BAUER
About taxes.

ULF HASHAGEN
About taxes, yes.

FRITZ BAUER
My parents went to a little town in lower Bavaria, Pfarrkirchen, not far from Passau at the Danube. My father finally succeeded to make his living there and everything developed as far as I could see it, fine. We arrived just at the time when Hitler was about to come to power in Germany. Some of the first impressions I had when I was now in a town, and no longer in a rural area, were the particular activities of the Brownshirts, which were not always to my taste. On the other hand, I could not get away without joining some of the Hitler youth activities,

although I never thought of making a career in the Hitlerjugend. In 1934 I switched from elementary school to a *Realschule*[1] . I could go to *Realschule* in Pfarrkirchen, at the same place. I stayed at the *Realschule* for five years. After these five years I had to continue the next three years at the higher level of an *Oberrealschule* (grammar school). Since my parents both came from Munich, it was a natural consequence that they wanted to send me to Munich, and so I was sent to a boarding school in Munich: the Albertinum. That was in spring 1939, and in spring 1942 I earned my baccalaureate, in German my *abitur*, the grammar school degree.

ULF HASHAGEN
Have you been a good scholar at school? Did you like to go to school the whole time?

FRITZ BAUER
I was not always pleased with my teachers, and not all of my teachers were pleased with me.

ULF HASHAGEN
For what reason?

FRITZ BAUER
Well, probably I was not always attentive enough. And maybe I was also unquiet and not listening devotedly to the teacher. I had a few teachers whom I liked very much. One was my mathematics teacher in Pfarrkirchen; it just happened that later on he became my father-in-law. And one was the music teacher. My teacher at elementary school, Gumberger, had already instructed me in playing the piano and I was later continuing at Pfarrkirchen in playing the piano, moreover I had to learn to play the organ. My music teacher there was Bruno Törkell and again this was a man who fascinated me. I had to learn to play the organ because I was supposed to play the organ at the school service. There always had to be one of the pupils who played the organ, and I was selected by my teacher to be the one who had to learn to play the organ. So I did, so I became acquainted with the organ. And my music background, of course, was improving under the heavy duty that I had.

ULF HASHAGEN
Did you ever think about making a career in music?

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[1] *Realschule* for students aged 10 to 16 is comparable to an American junior high school.

FRITZ BAUER
Not really, no. In the first line, if I would have thought of what I would like to do later on I would have said mathematics and/or physics, and that would have been much closer to my interests than music. Music I consider something that is nice to do – something you do for recreation – but to make it a living … Fortunately, I didn't develop such a taste that I would want to be a conductor or want to be a composer. I wouldn't have been a good one.

ULF HASHAGEN
Okay. However, did you show up to be very talented schoolboy in mathematics and in the sciences?

FRITZ BAUER
Yes.

ULF HASHAGEN
Strong interests?

FRITZ BAUER
Yes, to the extent that you can. What can you do to show that you are talented in elementary school? Nothing. In the *Realschule* only in the third year do things come up that you could become interested in from the point of view of mathematics. My mathematics teacher, Karl Vogg, gave a very interesting class in geometry to me, although not always to all the people in the class. So if this means that I showed talent, it could also mean that my class comrades didn't show the same kind of enthusiasm that I did. [laughter]

ULF HASHAGEN
Did this influence your career in mathematics?

FRITZ BAUER
I think it did.

ULF HASHAGEN
In which way?

FRITZ BAUER
Later, when I came to Munich, to the *Oberrealschule*, I had a number of mathematics teachers and again, the only one that I remember – even this one was not a good one, and the others I prefer not to remember

even – they didn't do anything to further me. They left me aside. They were quite happy if I didn't disturb their –

ULF HASHAGEN
Their hours of –

FRITZ BAUER
Their work, yeah.

ULF HASHAGEN
Did you learn mathematics or physics besides the school education?

FRITZ BAUER
Yeah, I did. In fact, it really started only in Munich because in Pfarrkirchen there was not the right atmosphere for this. But in Munich I pretty soon discovered there the Deutsches Museum. At first, of course, I wanted to go regularly to the museum because there were so many interesting things. And then I found out, when I had seen all the museum sections that they also have a library, and in the library I discovered that they have mathematics books. It actually was more the kind of literature you have for engineers, for the first two years or three years of engineering classes, but that was –

ULF HASHAGEN
So that means you learned calculus –

FRITZ BAUER
Elementary calculus and geometry. This was something that fascinated me and I absorbed it. So in fact I sometimes had the feeling that I'm now well educated in mathematics because I have read all the books that I could get hold of in the museum library.

ULF HASHAGEN
Does this include applied mathematics at this time?

FRITZ BAUER
I wouldn't even have seen a difference between pure and applied mathematics. It was all the same to me.

FRITZ BAUER
Besides, I became very much interested in physics by a book by Paul Karlson, a man who wrote a best seller in the middle of the 1930s,

under the title *You and Physics*.[2] And I remember how I was impressed by Karlson's explications on relativity or on the atomic theory, everything that was new to me. Even the museum books hadn't shown it in such a lively way; they were usually more dry. And so for a while I was quite thinking of physics to be the most interesting thing, but I always came back to mathematics again. I was also interested in chemistry. That was due to a teacher I had who was a good teacher and therefore could impress me. So mathematics, physics and chemistry was kind of my hobby at school time and it looked like I would go into teaching and become a grammar school teacher. My mother would have loved for me to go this way. But before this could work out – it was in 1942, when I made my grammar school degree and when I was not quite eighteen – I was just facing to join the Army, or to be drafted for the Army. I did not want to go voluntarily, and therefore I was first sent to the *Reichsarbeitsdienst*[3] . That meant that it took about three quarters of a year more before I entered the Army services. On January 2, 1943, I started my Army career in the infantry.

ULF HASHAGEN
What did you do during your time at the *Reichsarbeitsdienst*?

FRITZ BAUER
In the *Reichsarbeitsdienst*? There wasn't much I could do. That was a very sad thing. We had to work; we had very hard work. We were building runways –

ULF HASHAGEN
For airplanes –

FRITZ BAUER
Runways for airplanes. And also hangars for airplanes; in Russia, near Dnepropetrowsk, in the Eastern Ukraine. And it looked for a while like we wouldn't be sent back home to go into the Army, but that they would have redressed us as infantry and put us directly into the battle, to the war that had just culminated at that moment in the Kuban river area. But fortunately that didn't happen. So we were sent back home and I almost immediately began military service in Munich infantry regiment No. 7.

[2]Actually, the name of the book was *Du und die Natur: Eine moderne Physik für Jedermann*; 1934, Ullstein, Berlin.
[3]German labour service

ULF HASHAGEN
Here in Munich?

FRITZ BAUER
In Munich. That was a Munich regiment and I was starting in Munich. But I wasn't long in Munich. We were sent to France and –

ULF HASHAGEN
And what did you do in France then?

FRITZ BAUER
Training, for training and as an auxiliary occupation force. It was training for about three-months' time in France. Again there is not much I can say, at least from my interests. There would be nothing worth mentioning.

ULF HASHAGEN
Did you have time to follow up your interests in mathematics or physics during your Army career?

FRITZ BAUER
Not in my *Reichsarbeitsdienst*'s time and not in this part of my military service time. Also, again, the training period was very hard, so we didn't have much time. And normally we were quite fed up in the evening.

ULF HASHAGEN
What did happen when you finished with your military education, or your military training, in France?

FRITZ BAUER
After about two or three months of this basic training I was sunmmoned to the head of this training camp and he told me, "I have decided to send you to an officer's school." I wasn't asked whether I would like, he simply had decided to send me to an officer's school.

ULF HASHAGEN
Was this an unusual thing?

FRITZ BAUER
It was not unusual because he was ordered to send from time to time people to the school, but among the choices he had, I was one of the victims of this. I think I knew the reason. One of the officers that gave

us the training, on one day, during night, wanted to explain how you find where North is situated, and tried to explain it to us, but in the middle of his explanation he stopped and looked around and saw my face and said, “You explain it.” [laughter]

ULF HASHAGEN
And you did it.

FRITZ BAUER
And I did it just by telling him the story of the moon, and how the moon looks, and it depends on whether it’s a decreasing or increasing moon, and where you could think that even at night the sun is situated, although you cannot see it; and then, provided you have a watch, you know where North is.

ULF HASHAGEN
So in some way, your hobby for physics has influenced your military career. [laughter]

FRITZ BAUER
My career, in quotation marks. So the effect was that after the three months of basic training, I was not sent back to Munich and from Munich to the regiment that was fighting in Russia in the Army group “Mitte”, but I was sent to a preparation course for “applicants” for officers. And I got a lot of solid military training there.

ULF HASHAGEN
How long did this happen?

FRITZ BAUER
It was for four-months’ time, plus two months back at the training camp, but in a platoon-leader function, to get a little bit into the habit of how to command people.

ULF HASHAGEN
Was there, by the way, a strong influence by the Nazi party to make a political influence on the officers in the Army during that time?

FRITZ BAUER
Not in this area, not in this unit.

ULF HASHAGEN
For what reason?

FRITZ BAUER

I don't know. It was not the case in this regiment; I had not seen Nazi people there. So there was nothing like this. But I had met at that second training period an officer, Captain Siegfried Graber, who impressed me again. He came from the Russian front. He was wounded and couldn't do active duty, but he still could tell us what we had to know about the real war. And he gave us a hard but very useful training, a kind of survival training, and I had later discovered that he had saved my life because he had prepared me for situations which otherwise I wouldn't have been able to consume. So, again, a man who impressed me – that didn't happen very frequently with Army officers – this man whom I liked very much. I still know him; I still meet him from time to time. Yeah. We were located first in Lyon, in France, later on near Belfort, and then again sent to the south when Italy went its own way; when Italy tried to surrender, we came to Grenoble as a kind of occupation group for that part of France because before it was occupied by Italian troops. We had to disarm the Italian troops; it wasn't very difficult: they wanted to throw away their weapons anyhow. And then this all was over, and I did come to the *Frontbewährung*, that is I –

ULF HASHAGEN

What does this mean: *Frontbewährung*?

FRITZ BAUER

I was expected to serve for three or four months' time in the function of a corporal or non-commissioned officer in a fighting infantry unit. This fighting unit was in Russia, in the middle section of Russia.

ULF HASHAGEN

When was this?

FRITZ BAUER

I arrived there – it was quite a way to get to the front – but I finally arrived there in the middle of October 1943. I came into a very desperate situation: the Russian attack had started at Kursk in the middle of the year, had progressed and had reached the Pripyat swamps, a place that became famous later because of that atomic power accident.[4] The troops were decimated, so that for the very first

[4]This is a reference to the Chernobyl nuclear power plant accident that occurred on 25-26 April 1986.

weeks it was a rather critical situation, but step-by-step the situation improved. In the wintertime the Russians didn't attack any longer, so it practically was a stable situation. The only thing that was not so nice was that we were living in the water, in the Pripyat area, which when spring came, got higher and higher and we could jump from island to island. That was actually not a very good time, but still better than to be heavily involved in fighting every day. And when this three or four month period was over, it was in the spring, nothing happened. So I waited just for something to happen because I was supposed to be sent back to officers' school or to be told that I have failed. And nothing happened: not the one and not the other. And so I waited, I simply waited until June. And then some surprise arrived. The regiment commander had seen me before, one or two times, and for quite some time – since as a boy I had lost a front tooth – I was just running around with a big, big gap. So when he came again and looked at the troops, he discovered me and said, "I have seen you already before with your missing tooth. How long are you here now? " I said "Since October". "Oh, and they didn't send you to officer's school? " No. And he told the captain, "Send him immediately to the officer's school." [laughter] And that meant that I was sent to officer's school just a few days before the big Russian attack started – that started in about June 10, 1944 and ended at the German border – and I escaped this disaster. I came to officer's school in Milowitz, near Praha, in Bohemia, and there is little, I think, that is worthwhile to tell. Again, we had to do our work and I didn't have much time left. So what I could expect is that after I have finished, or while I was finishing, I would be sent back again to the troops and I would become a second lieutenant and probably have to lead a platoon or a company. All these prospects were not looking so good, in particular since, in July 1944, the plot that was directed against Hitler failed, and so one could expect that the war would go on for quite a while. It actually developed exactly this way. I was sent home after I had passed the officer's school in the end of October 1944 and was immediately ordered to a *Führerreserve*, to a …no I can't really find the translation for it.[5] And I didn't know what it was good for, but it turned out it was a preparation for the Ardennes offensive.[6] So when I could see what would happen, that it would go in this direction, then the prospects were not too good. But, strangely, I was sent to the 15*th* Army, which was in the north and didn't attack; it was staying back home. So I was staying with the 15*th* Army and again had to do training

[5]A *Führerreserve* was formed, when impending military actions made large casualties among officers likely

[6]The Ardennes Offensive is better known in the U.S. as the Battle of the Bulge.

young soldiers. In the meantime, this training of young soldiers meant that we had people aged seventeen, sometimes even only sixteen, and that was really something very depressing, that these children would pretty soon be sent into combat. It actually went on until the middle of February 1945, and then the American attack that was directed against the Ruhr valley and against Cologne was immediately, immediately –

ULF HASHAGEN
Successful?

FRITZ BAUER
To be expected. And our training unit was sent, around February 24, to a place where American tanks had crushed the German front. This infantry of seventeen-year-old people without heavy weapons were supposed to stop it. That, of course, was not possible and as a side effect I was severely wounded and fortunately I was picked up by American soldiers that were progressing. And they even brought me back to a front hospital. In the end, I was sent to a prisoners' hospital in Normandy.

ULF HASHAGEN
So this was the first time you had been in contact with Americans?

FRITZ BAUER
Yes, and with American language. Really in contact, because pretty soon I developed into an interpreter of the *Stars and Stripes*, the US soldiers' newspaper. So I had to explain to my comrades what was happening in the war – that was what *Stars and Stripes* was mainly good for – and that fortunately the Americans had picked me up, otherwise I would have lost my leg. The medical treatment was excellent, at least in the circumstances. I couldn't say that they did less for us than the surgeons would do for their own soldiers, the Americans. I was lucky that I didn't come into a Russian prisoners' hospital.

The war ended and after I had recovered I was sent to a prisoners' camp. I had a little bit of a difficulty in showing my rank. They first wanted to give me the rank of a common soldier, since I had no papers left that could show my rank. But thanks to the good advice of a Catholic priest whom I contacted – he told me, "By God, just try to get your rank back, otherwise they will send you to France to the coal mines." – I had good reasons to try to get my rank back. And then, of course, I was investigated and there were all kinds of tricks: Whether I was just an SS man under a wrong flag. Since this was not the case and

I could show proof, then I was finally admitted into an officers' camp, again in Normandy. In the beginning of September 1945 I was sent back home, back home to my parents, who at that time still lived in Pfarrkirchen, in lower Bavaria.

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BAUER

## BACK FROM THE WAR: 1945–1955

ULF HASHAGEN
Professor Bauer, what did you do when you arrived, after the Second World War, in Germany again? What plans did you have? How was the situation in Germany when you arrived in Germany again in 1945?

FRITZ BAUER
The situation was a very sad one, as is well known today. But people of my age, if they did come back at all, looked forward. It could not become worse; it could only become better. In the first instance I had to work in my father's office because otherwise I would have been asked to do hard labor – that means labor for unskilled people, which normally I would have had to do. Then I hoped that the Munich University would open pretty soon, and, in fact, in spring 1946 Munich University opened and I enrolled in mathematics and physics.[7] That meant that I could hope to pass one day a state teacher examination and, if necessary, I could also earn a doctorate. My professors in the first year – in fact we had at that time trimesters, that means we had three trimesters in the first year – my main professors were Oskar Perron and Heinrich Tietze, these two had started courses for beginners who were all young people who had come back from the war. Perron was a very impressive man. He lectured on analysis, on classical analysis, the usual beginner's lectures, which I had thought I would know because of my prewar museum studies, and I found out that I had not known all of it. In fact, from time to time I was quite surprised what I had missed. There was also Tietze, who was a little bit funny sometimes, a little bit whimsical, but otherwise he was definitely a very good mathematician who simply couldn't convince people so much as Perron could do. Constantin Carathéodory I could not see because he was ill and he didn't come to lecture. After a while Robert Schmidt came back, he also had escaped from the war. Robert Schmidt was given work lecturing on subjects that the others wouldn't want to do, so he was first a kind of a *Mädchen für alles* (maid-of-all-work) for the others. In fact, Robert Schmidt was the most modern one at that moment. Perron was not so much interested in the most recent developments in algebra. In fact, I had been influenced much by Helmut Hasse for the following reason. During the war the *Truppenbetreuung* (army special services) sent to me on the Russian front a little booklet by Hasse, exercises for his books on algebra. I tried to understand it but it is difficult to learn algebra from a book that

[7]Munich University refers to the Ludwig-Maximilians-Universität München.

only has the problems, sometimes only hints a little bit at the answer, and doesn't even give an introduction to the subject. [laughter]

ULF HASHAGEN
Do you mean the algebra books of Hasse published in the 1920s by the Goeschen series?

FRITZ BAUER
Yes. The book I had received was the book of exercises for the two Goeschen volumes. I tried to get the original books, the proper books by Hasse. I managed to get them from the university library in Erlangen, but I could get them for just fourteen days. So within fourteen days I tried to read and understand Hasse's algebra. I made notes and I had a feeling I had done the best I could do. In fact, it turned out that I had understood almost all of it, and that had suddenly sparked my interests for algebra, at that early moment. Robert Schmidt was also lecturing on, for example, projective geometry and similar things. Tietze also lectured on differential geometry, but this was just a kind of a normal program, nothing particular.

ULF HASHAGEN
Did Perron or Tietze run a seminar for advanced students during this time?

FRITZ BAUER
Not really. In fact, in the very beginning the two men had the full load of the work with the bunch of students who came back from the war and didn't know much, had little preparation. So I think they would have had great problems in running a seminar for this many people. It actually didn't really work out this way. It developed later on, step-by-step, but in the beginning such a thing couldn't be done.

ULF HASHAGEN
How about physics during your study at the University of Munich, from 1946 to 1949? Who were the teachers? Who were the professors you learned physics with?

FRITZ BAUER
There was, of course, Walther Gerlach, but he was in the very beginning not yet "cleaned."

ULF HASHAGEN
What does "cleaned" there mean?

FRITZ BAUER
Gerlach was a little bit involved in some research that was important for the war.

ULF HASHAGEN
He was a member of the *Reichsforschungsrat*[8] during the Second World War? [9]

FRITZ BAUER
Yes. This is one thing that made him for the moment not qualified or not allowed. There was not really a lot of impression I got from physics, from experimental physics, but that would be what one begins with. And Arnold Sommerfeld did come back too, he started then theoretical physics, starting with mechanics and then went into electricity –

ULF HASHAGEN
Electrodynamics.

FRITZ BAUER
Electrodynamics, and so on. And I listened to Sommerfeld, who frequently was represented by Paul August Mann, who knew him for a long time, he was formerly his assistant, and so most of the theoretical physics work of Sommerfeld I learned through the transmission by Paul August Mann.

ULF HASHAGEN
By the way, did you learn numerical mathematics during your study? Was a course in numerical mathematics, or numerical problems –

FRITZ BAUER
No, numerical mathematics in the sense we understand it today was not done. But Robert Schmidt was one who mentioned from time to time the practical aspects that means the numerical aspects, of certain algebraic or geometric problems, and even gave sometimes hints how to do it in practice. But that was not really numerical analysis in the proper sense that you carry on a big computation. I still came into contact with numerical analysis a little bit later, when I already worked

[8]The *Reichsforschungsrat* (German Research Council), founded by the Nazis, coordinated research during the war.
[9]Walther Gerlach was the titular head of the German effort to develop an atomic weapon during the war.

under Fritz Bopp, but maybe I take this up later at the right moment.

Another thing I would like to mention is music. I had kept in contact with my music teacher Dr. Alfred Zehelein in Munich – of course mainly because I had wanted to continue my piano playing a little bit – but one day he said that he would very much prefer that we do theoretical work, that means *Harmonielehre* [harmonics] and *Kontrapunkt* [counterpoint]. I had great fun in doing this and worked with him, but it was a side issue; it wasn't my main study, it was not taking up my time, it was just a kind of a –

ULF HASHAGEN
Recreation –

FRITZ BAUER
Recreation, yeah. And, in fact, we even came this far and I started to do composition with him. He taught me composition and I even wrote some few examples, tiny exercises in counterpoint and in composition. In summer 1947, I even wrote a piece "Variations on a Theme of Bartok" for piano, performed on Dec. 10, 1947 by Albertine Schropp. But I gave it up completely when I had finished my state examination because I found out I couldn't do both. I had to leave either music or mathematics, and so I left music.

The scientific state teacher examinations came in May 1949. My orientation at that moment was very much impressed by a newcomer in Munich, Fritz Bopp. Bopp took over the Sommerfeld chair, I think it was in about 1947, and started lecturing on very advanced topics, on quantum theory, on the very theoretical aspects of quantum theory. I was very fascinated by Bopp, so I listened to his lectures from then on, got more and more acquainted with him, and had discussions with him. And he also found out that I had interests in mathematics, he had mathematical problems and so he liked to have people to do them. One example was a certain calculation that ended with determining the eigenvalues of a four by four matrix. So I said, "Okay I will try to do it," and I then sat down and with hand calculation, not with a machine, I tried to solve this. He gave me support by giving me an article by Ewald Bodewig, and Bodewig's article was at the moment the best –

ULF HASHAGEN
Overview?

FRITZ BAUER
The best complete collection of numerical mathematics methods. So there was, among other things, the eigenvalue determination. This was

my first contact with numerical mathematics, but it pretty soon turned out that I would come into a much deeper contact with numerical mathematics. But with Bopp, it developed in the following way. Bopp was interested in group representations. This was a subject at that time that had to do with elementary particle theory. He hoped that by studying the group representations – the finite representations of classical groups – one would gain inside into the elementary particles that existed, that one could group them, could classify them, and could maybe discover new ones. And he gave me a theme for my paper, for the state teacher examination, which I finished in Spring 1949. And from then on, I was in his inner circle. He gave me a half-time position as *Hilfsassistent* (auxiliary assistant) while I was attending as *Referendar* (teacher on probation) a state seminar for practical teaching under the direction of XXX Apfelbacher. In June 1950 I passed the practical part of the state teacher examination and became in September 1950 *Studienassessor*, a beginner's position as a grammar school teacher, at the *Gisela Oberrealschule* in Munich.

But fortunately, after a half year Bopp approached me again and said that he had a full assistant position, and of course when he offered it me, I accepted it. That was around Easter 1951.

ULF HASHAGEN
When did you do your Ph.D. and what was the topic of the Ph.D.?

FRITZ BAUER
After I had been offered a full assistantship, I tried to do a Ph.D. on a subject closely connected to Bopp's work. This turned out to be group representations for elementary particle theory and this I finished in the end of 1951. In January 1952, I had my examinations for the Ph.D., for the doctor degree. Elementary particle theory was at that time just in development and the group representations I had found were considered by Bopp to be a hint for possible classification of the elementary particles and even for finding new ones. It actually turned out that Bopp had forgotten one thing. He simply reminded me always to look for real representations and not to study the complex representations, which turned out that it left just the most important part that was then found years later by Murray Gell-Mann, and a bit later quarks and the modern theory. This I couldn't find because I was sort of misled –

ULF HASHAGEN
The wrong direction –

FRITZ BAUER
In the wrong direction. And in the time at Bopp's institute I also met every day Paul August Mann, I mentioned him before, who had been at Telefunken during the war in radar, and he discussed with me, from time to time, things from physics and from his experience in electrical engineering. He found out that I was still interested a little bit in physics, in real physics. Sometimes he gave me articles he found, or he would have them sent from his American colleagues, among others, articles by Richard Hamming, by Claude Shannon, and by John von Neumann. And this had brought me by mid-1950 into the field of computing, in the direct sense by the articles from von Neumann, but also Shannon's articles were very much connected and directed to computing. And Hamming's article was on coding, on error –

ULF HASHAGEN
Error correcting –

FRITZ BAUER
Error correcting and error detecting codes. I was very interested in this article by Hamming and I found out that it would be possible – if one would use an extra one of the checking characters – to do it with many fewer relays. This, of course, seemed to be an interesting solution under practical aspects. So, Paul August Mann recommended to me, I should see to make it a patent. And he also helped me a little bit in getting introduced into the patent field. I finally sent in the patent and, to my surprise, I got the patent. Siemens-Halske in Munich, again by contacts Paul August Mann had to Herbert Wüsteney, was very interested in this patent and so I sold it to Siemens-Halske. This not only gave me some money, but it also gave me an additional income by a consultant contract with the Siemens company for many years to come. It had the advantage that I was in contact now with an industrial company working in all kinds of electrical engineering, and for my coming work with Sauer and Piloty it was a nice supplementary education. It supported me in this engineering thinking that I would need when we started to build computers.

ULF HASHAGEN
As far as I know, Professor Bauer, you changed your job at the University of Munich to a job with Professor Robert Sauer, professor of mathematics at the Technical University of Munich, shortly after your Ph.D. in mathematics. Is this true?

FRITZ BAUER

Yes, that's correct. And this came about in the following way. Paul August Mann had already made me aware of the developments that John von Neumann had started in the United States. And a young colleague, assistant professor of Sauer with the name Hermann Jordan, told me one day, "Sauer together with professor Piloty, professor of electrical engineering, is now about to start building a computer." I showed my interest and asked whether the two professors would allow me to listen to these seminars they were arranging. Jordan said yes, he thought it could be managed. So I got contact with Sauer and Piloty, and from this contact developed a possibility to attain an assistantship with Sauer.

ULF HASHAGEN
Could you please talk a little bit more about Sauer; what type of mathematician was he? What did he do and did he influence you very much?

FRITZ BAUER
I had seen Sauer already when I took my state teacher examination, but I had only that few minutes of contact during the examination with him. But, through Jordan, I had been informed that Sauer, an applied mathematician of the geometric side, had done work during the war with analog computers and now was interested in doing it better with electronic computers, as they were now obviously built in the United States. Piloty was the engineering colleague who wanted, for good reasons, also to enter this field. So the two gentlemen joined and made a common project under the auspices of the *Forschungsgemeinschaft*.[10] And again, Jordan told me in summer 1952, that the *Forschungsgemeinschaft* now had agreed to support this project and that they were now looking (that meant Sauer was now looking) for a man to work on it. And I recommended my friend Klaus Samelson, who had been with me in the last years of my studying, who had been with me in the *referendar* time before we both took together the state teacher examination and who, in the meantime, was grammar school teacher at another Munich grammar school. I recommended to Sauer to ask Samelson whether he would like to work for him and I approached Samelson telling him that this would be a much better career than to do work at the grammar school. He finally agreed, and so Samelson was the first mathematician to join the Piloty-Sauer group in summer 1952.

[10]The *Deutsche Forschungsgemeinschaft* (DFG) is translated as the German Research Foundation. It is the central organization for publicly financed research institutions in Germany.

When this had worked out this way, I was thinking whether I could also work for Sauer. And after some time it just happened that Sauer offered me an assistantship in his institute, with the normal duties of a teaching assistant, but with particular orientation to the coming design of an electronic computer.

ULF HASHAGEN
For what reason, Professor Bauer, were you so interested to change fields from theoretical physics to applied mathematics; to the field of computing, which was not sure to become a field?

FRITZ BAUER
I was pretty sure that this computing field would be growing, everything looked like this. And I was in a sense disappointed with the kind of work Bopp did, in the sense that although I liked him very much as a person, I sometimes was not convinced that his approach to elementary particle theory would be the most fruitful one. So I simply tried to go on to a more reliable career.

ULF HASHAGEN
Was Sauer an influential professor in the mathematics field in Germany?

FRITZ BAUER
He was quite influential in mathematics, in particular in applied mathematics, because of his work during the war. But he was also a good pure mathematician, particularly in geometry, particularly in differential geometry. So to work with Sauer was certainly something one would like; it was a good climate, it was a good atmosphere. And personally I also started to learn Sauer better and better. And I finally agreed with Jordan, when Jordan asked me whether I would come to Sauer because he had agreed to go to Meixner in Aachen, who had offered him a position in theoretical physics. Then when Jordan tried to convince me that I should replace him at Sauer's institute, he simply said "Sauer's, this is the position for you, exactly the way you should have it." And this turned out to be true.

ULF HASHAGEN
Okay. Soon after you published your first article, together with Klaus Samelson. This article was on optimal accuracy for floating point

computers.[11] How did this close collaboration with Klaus Samelson start? What was the reason for this close collaboration?

FRITZ BAUER
We had met in the second year of our studying, when both of us were asked to do some grading work, grading exercises. And we were sitting together and we had made jokes sometimes and so one of these jokes was a little bit aggressive on my side. And Samelson reacted also a little bit aggressively on his side, and it turned out that we had the feeling we could very well build up a friendship from the start.

ULF HASHAGEN
Could you please describe Klaus Samelson a little bit more for us? What was his background, how was he brought up?

FRITZ BAUER
Samelson was half Jewish, and his father had committed suicide in the Nazi times because of being persecuted. And Samelson had great difficulties to come through during the war. In fact during the last few months of the war he went underground in Breslau [Wroclaw]. So he had a much harder way of life during the Hitler times than I had. On the other hand, he was not in the regular Army. Sometimes he was considered not to be good enough as a German and sometimes he was considered to be just good for hard labor. So we had two completely different backgrounds. But my upbringing had brought me in contact with Jewish people and so I had no problem with him. And he had enough tact that he didn't mind that I had been in the Army, which was not his Army. So that was the background of the two remarks I mentioned, and we found out that we could quite well get along with different orientations, with different histories, but going to the same goal.

So, finally Samelson was at Sauer's group, directly responsible for the building of the PERM. I was supposed to do work as much as I could in the same direction. To do numerical analysis was a side issue, which I could follow. That was all I wanted to do at that moment. I wanted to come into computing, and it was certainly a good way of approaching it. Samelson was the same. He could also now have a good academic position; he was no longer only a teacher in a grammar school. So we had – apart from our personal contact – a common goal,

[11]Friedrich L. Bauer, Klaus Samelson, "*Optimale Rechengenauigkeit bei Rechenanlagen mit gleitendem Komma*", *Zeitschrift für Angewandte Mathematik und Physik*, 1953, vol.4, pp.312–316.

and we worked together then for several decades.

ULF HASHAGEN
What were Samelson's special qualities? Was he a very good mathematician?

FRITZ BAUER
Certainly he was a very good mathematician. From time to time he showed me that he was much better than I was; sometimes I could convince him that I was better than he was. So again this was a fruitful competition. He had a very sharp way of thinking, and he had a very good feeling for studying complicated situations. Frequently I was inclined to give up in a very messy situation where one would find it hard to see what belongs to what; he would be the one who would sit down and work and come out with a solution. So, again, this was something where I was always glad I had him when I was giving up. On the other hand, I developed from time to time more pressure. I wanted to bring something forward and I could influence him to join me and to do something in the same direction. Again, this was good for him, so we were kind of an ideal pair.

ULF HASHAGEN
Okay, let me come back to the work you did during the first years. Could you please tell us a little bit more about the PERM project, what kind of a computer was this, and what did you do?

FRITZ BAUER
The PERM was supposed to be a computer up to the most recent development in ambitious computing machines. That meant it was following the Whirlwind idea of doing parallel addition, using a parallel adder – that was one thing. It was following extensions that had been described by some Swedish people concerning wired-in floating-point. That was particularly interesting for Sauer when he did computations in fluid dynamics and in gas dynamics where the order of magnitude of, say, intermediate results was very difficult to control. This floating-point representation was a subject that we just had approached in the first paper we wrote together. There were a few more works of this sort[12] where our mathematical group – Samelson and I – tried to influence the engineering group to do something more modern,

[12]Friedrich L. Bauer, Klaus Samelson, "*Maßnahmen zur Erzielung kurzer und übersichtlicher Programme für Rechenautomatzen*", *Zeitschrift für Angewandte Mathematik und Mechanik*, 1954, vol.34, pp.262–272.

to bring some new ideas into the design. That happened also later, when Heinz Schecher had joined the mathematics group, with the so-called indirect addressing, which was an important step into modern techniques of linkage. So in the first we were supposed to influence the design in bringing it up to the most recent, the most modern, aspects. On the other hand we had to prepare library routines for the PERM. At that time, already under the influence of Wilkes, a computer was no better than a library of programs that had been provided for it. And so we tried to make for the PERM, within the structures that were already outlined, a library that would be able to provide numerical mathematics – that would be special functions (starting with trigonometric functions and so on), that would be linear algebra problems (the solution of linear equations, of eigenvalue problems), and so on. This was our main business during the next few years until the computer was finished. When the computer was finished – when it was working in 1955 for the first time, and then permanently in 1956 – then, of course, it was all then coming into practice: we could see how it worked, we could see whether we had solved the task properly, and we could correct it where it was necessary. That was our second duty; the next one then was to see that the computer came into practical work, to teach users how to use it, and to give them instruments like a program library and service routines to do this in the best way.

ULF HASHAGEN
Professor Bauer, one thing you are famous for from the early periods of your career is the STANISLAUS machine. Could you please tell us a little bit more about this story?

FRITZ BAUER
Yeah. The STANISLAUS machine was in the first instance only an experimental model for solving a particular problem in mathematic logic. It was initiated by Professor Wilhelm Britzlmayr. He was a logic professor at Ludwig-Maximilians University, an honorary professor, and he was lecturing, among other things, about a syntactical problem in propositional logic: the so-called Polish notation. And I should also mention Konrad Zuse, who had one day visited Britzlmayr. Britzlmayr had invited him, and Zuse wanted to explain his *Plankalkül* programming language, and for this reason he took an example that was well known to Britzlmayr, the well-formedness problem of the Polish notation. So that was already during my time with Bopp, and I had developed a practical machine, a relay machine, for doing this test on well-formedness by a relay circuitry in a way that could not only test whether a propositional formula was correct or not, but could also

evaluate it at the same moment. And that was a pilot example, I could say. I started it at the turn 1950/1951 when I was visiting my wife in a Davos convalescent home and it was resting for quite a while because there seemed to be no further need for finishing it. But in our work on bringing the PERM into applications, we always had the problem of what one later called automatic programming. One wanted to use mathematical notation, so that from that notation the program would be developed automatically, and so that one wouldn't have to write by hand a very complicated program which was almost unintelligible for outsiders. And knowing the solution I had made years ago for the propositional calculus, I mentioned one day to Samelson this possible approach to automatic programming. There was also the work by Heinz Rutishauser in 1951, in which he had already given an algorithm for doing it. But if we did this in the way I had designed it for the propositional formula – I had called my little machine STANISLAUS – if we did it the STANISLAUS way it would be much simpler and much faster than Rutishauser's solution. And Samelson said yes and had already some ideas about how to do more: not only testing the well-formedness of a mathematical formula including executing it, but doing more of what had to be done in the practical sense of programming, for example, storage allocation, which was outside the field STANISLAUS was concentrating upon. And so we again supplemented each other on that idea of doing automatic computation, doing automatic programming (in the sense that the word was used at that time) by a very effective, by a very efficient algorithm.

We even told Piloty about this possibility. He said yes, and said that in the next PERM – the successor of the PERM that was just under construction – he would like to have this all wired in, so that this automatic programming would also be a sort of normal running of the machine. That meant that we had to see whether we could have it patented. So we sat down and I wrote the patent, submitted it, and finally, after some time, we even got the patent. It was also filed in the United States, and the United States patent was granted. In fact, the United States patent, although filed later, came much earlier; the German patent took quite a time before it went through all the procedures. But when we were this far, when the United States patent was about to be published, Piloty had found out that the *Forschungsgemeinschaft*, the research council, would no longer support building of machines at institutes. There were by then machines being commercially manufactured in Germany, and therefore this whole thing – to make by wiring-in the next PERM an automatic programming device – would be obsolete. Both Samelson and I said, "Okay, if we cannot wire it in, then we programm it," and we started

into programming it for the PERM that was coming into use. That was the beginning of our sort of automatic programming; Rutishauser called it Automatische Rechenplanfertigung and we called it the Cellar Principle. The idea behind is the use of correct bracketing under all circumstances.

ULF HASHAGEN
This is the very famous Cellar Principle –

FRITZ BAUER
And that was the origin, the birth of the Cellar Principle idea. That was probably the most important result of Samelson's and my work for Sauer and Piloty, to make this step forward in programming technique. The Cellar Principle turned out to be, in great many applications, visibly or invisibly involved. In most of the programming techniques people were using at that time, it was invisibly existing and in fact it influenced the programming language. It turned out that the best programming languages for practical use are those that make the Cellar Principle running smoothly. So it had a strong impact on the development of programming languages. One of these impacts was ALGOL 60, which was built strictly according the Cellar Principle's doctrine.

ULF HASHAGEN
And ALGOL 60 was a project you were heavily involved in?

FRITZ BAUER
ALGOL 60 was a project I was also heavily involved in, again together with Samelson, and with Rutishauser in Zürich.

ULF HASHAGEN
What was the aim of this project? Could you please tell us a little bit more about it?

FRITZ BAUER
There was a very practical motivation. One day one of the advisors of the *Springer-Verlag* (Springer publisher) approached Samelson and me saying "Now you wrote many of these routines libraries, program libraries, couldn't they be published? And wouldn't it be a good idea for Springer to publish such a handbook for all of these? " And we said okay. But we said, "It makes sense only if we use a common language"–because at that time every company that built computers had its own programming language–"we have to have a common

language." There was already Fortran, but we didn't like Fortran to the extent that we would base our work on Fortran which did not obey the cellar principle. So we did something that fits our way of thinking, meaning that we followed the Cellar Principle for the development of a language for this handbook. That was the start of it.

And of course this had to be brought into an organization, so we started, informally at first, to approach American colleagues. We formed a very informal group, four Americans and four Europeans for its first draft in 1958, so-called ALGOL 58. There was American competition and this all ran together into ALGOL 60, which was developed under the IFIP umbrella. IFIP, the International Federation for Information Processing, was formed in 1959; it was just formed in time. So under the IFIP umbrella, ALGOL 60 was developed in January 1960 in Paris, in an International Conference by a carefully selected group of people, of specialists in this field. This is how ALGOL 60 did finally come along.

## FIRST STEPS OF AN ACADEMIC CAREER: 1955–1962

ULF HASHAGEN
Now the first thing that I would like to do is to get you to say something about your academic career after your *habilitation*, your qualification as a university lecturer at the Technical University of Munich in 1954.

FRITZ BAUER
My *habilitation* was in the middle of 1954. That means that after less than two years of working at Sauer's Institute, I had already decided that I would try to stay at the University, to go on a university career. The *habilitation* was just a necessary first step. The dissertation was not directly in the Sauer line, it was on a problem that I had become interested in mainly by the work of Piloty, who in working on electrical filters always had to deal with the determination of all the roots of a polynomial. There was known the Bernoulli-Jacobi method for the determination of roots of polynomials and it would seem to be interesting to do it in a quadratic convergent form. At that time, many people worked on linear-convergent approximations that were too slow. Even on fast machines one just had to do too many steps.

ULF HASHAGEN
Did the PERM computer in Munich already work at that time?

FRITZ BAUER
At that time it did not yet work, so I had to make my test computations by hand. But of course as soon as the PERM would be in working condition I was trying to use the method also on the machine.

ULF HASHAGEN
After this, as far as I know, you became a lecturer, a German privatdozent, at the Technical University of Munich?

FRITZ BAUER
Yeah, after a few months' time, Sauer addressed the University people to give me an appointment as a lecturer. That happened in 1955, it worked out in the same year, and from then on I was entitled to offer lectures, and at the same time I was obliged to present lectures from time to time. I cannot really remember what the first one was. It was certainly in my scientific field, but whether it was more on the mathematical side or whether it was more on the machine side, this I cannot really remember anymore. I worked as a lecturer for the next

two and a half years, until I was informed one day by Sauer that there were a few positions open in Germany where a professor was wanted. My first answer to Sauer was, "I don't want to go away, I would like to work here and I enjoy working here." But Sauer said, "That doesn't help, you have to go away sooner or later if you want to make a career, but maybe you can come back one day." This last part I knew would be difficult, and so I didn't take it very seriously. I only hoped that I would have a choice that I wouldn't just have to accept the first call, a *Ruf*, as one would say in German. It was normally expected that one would accept the first call to a university; if one would not accept the first call, it might be that it was the last call.

ULF HASHAGEN
For what reason did you want to stay in Munich, and were not open to go to other parts of Germany?

FRITZ BAUER
On the one side, in late 1957 when this happened, the PERM was working and I enjoyed working with the PERM. I had many interesting things that I wanted to continue on the PERM; in particular, this part of programming theory that then later became part of my life. And so I had good reasons for trying to use this nice instrument. Moreover I liked the environment: I liked Sauer, I was in a group that was working well, and so I had no reason for wanting to leave. But Sauer after a while was very nice and said "There is one possibility in Aachen, there is one possibility in Cologne", and I think he also mentioned Saarbrücken, "and there is also one in Mainz". And we discussed a little bit what the situations were, and it looked to me that Mainz would be preferable. One reason was that it is closer to Munich, where my father still lived, and I wanted to see him from time to time, and also to be more in the south seemed to be better for me. So, I may have mentioned this to Sauer, and Sauer happened to take it up, and the first call I actually got was from Mainz –

ULF HASHAGEN
This was in 1958 then, or –

FRITZ BAUER
That was in 1958. And so before long things worked out. I had to go to Cologne to give a lecture there; I think I had to go to Aachen, too. But this was just the normal kind of thing that people wanted to see the man they were supposed to hire. But actually Mainz was a little bit quicker. And so before the other things developed – maybe they wouldn't have

developed, I do not know – the procedure in Mainz had already progressed quite a lot. This was mainly due to the fact that one of the Mainz professors, Robert Furch, a geometer like Sauer, was a very kind man, and in particular he was kind to me. So I had the feeling that he was a man who would be a good colleague. He would be an elder colleague, and elder colleagues are sometimes difficult; but he seemed nothing like this to me, so I was quite pleased to go Mainz. It actually started in spring 1958, in spring 1958 I took the position in Mainz.

ULF HASHAGEN
What kind of professorship was it in Mainz?

FRITZ BAUER
It was an associate professorship and that means I was given an institute, or rather I had to build up an institute; one wouldn't need to have a full professor to begin with, and one wouldn't want to have a full professor to begin with. So the fact was that the Mainz faculty had decided that they wanted to do something in computing, and among the things they had in mind was a Zuse machine, a Zuse Z22. For the computer they had to have a professor that would bring it to the students and that would arrange some activity.

ULF HASHAGEN
This was part of the program of the *Deutsche Forschungsgemeinschaft* to give computers to the German universities.

FRITZ BAUER
And this was probably the very reason for founding the associate professorship: that they wanted to get from the research council a machine and that this was a kind of a starting point. It was meant to show that one would not only want to get a machine but would also want to do something with it. Yeah, I went to Mainz. I guessed it beforehand, but pretty soon it turned out to be the case that it wouldn't really be satisfactory to work with the slow Z22. I came from Munich, from the PERM, where I had a much larger computer at my disposition. But at that time the *Forschungsgemeinschaft* (the research council) also was looking around, or had shown interest in buying a few larger machines from the Siemens Company and from the Standard Electric Company. So I tried to be –

ULF HASHAGEN
To get one of those machines –

FRITZ BAUER
To get one of these machines. In this case I knew already that the Siemens machine would be the better one, so I didn't have to wait long to decide which one I would like to have. And it actually came through. So within a short time we had not only a Z22 but also a Siemens 2002, which was a much faster machine and was quite an interesting machine. From then on I was quite well equipped with computers. We also got an analog computer. I really didn't want to, but I just had to accept it. I got a little Telefunken analog computer, not yet a transistorized one, but an old tube model RA 463. In a sense, it was interesting because it was something different from what I had done before. But typically I gave to one of my students the problem: here you have a digital computer and for an analog computer you have to do a wiring; given a problem that means given an algorithm, can you, by a machine program, find out what wiring would be the best one for solving the problem on the analog computer. It showed my orientation to automatic computing by even using a digital computer in order to program an analog computer –

ULF HASHAGEN
Okay, I understand.

FRITZ BAUER
The next thing was that I had left Samelson back in Munich. I had arranged for Samelson to have a research position at the university with Sauer in 1952. But when I'd now left Munich and went to Mainz, I had the feeling I should at least ask him whether I should arrange something for him in Mainz. And it turned out that Professor Furch, a man I mentioned before, said he had already found out a little bit what Samelson did in Munich. And when I said we could also have him as a professor in Mainz, he said, "Yes, that would be interesting." So with the help of Furch, in three quarters of a year's time Samelson was also an associate professor in Mainz.

ULF HASHAGEN
Okay, I understand. May I ask you, what was the reaction of the pure mathematicians on this new institute of applied mathematics headed by you, and how did it work in Mainz? What did you do there, and how did you feel there as an applied mathematician?

FRITZ BAUER
The Mainz faculty was, of course, mainly oriented to pure mathematics of all sorts. I had no difficulties with the pure mathematicians. I never

had any difficulties with pure mathematics anyhow. I didn't do applied mathematics because I would not like pure mathematics, I just did it because I found it as interesting as pure mathematics, and then from time to time I did pure mathematics or any kind of mixture between applied mathematics and pure mathematics. So as far as I can remember, I had no problems with my colleagues, from the point of view that this now was a completely new field, they had wanted me, I had accepted, and I was there. They supported me to some extent, to the necessary extent – Robert Furch in particular, but also some of the other professors – and that was essentially a good cooperation. In particular, an interesting cooperation was with Rohrbach –

ULF HASHAGEN
Hans Rohrbach –

FRITZ BAUER
Hans Rohrbach. Rohrbach was a man who had a feeling for applied mathematics, in particular for numerical mathematics, although he was not a numerical man. But he had a feeling for it, he had an interest for it. I did at that time not know so much about his life. I found out much later more about it, in particular I found out much too late that he had worked in cryptology during the war. So during the time in Mainz I would have had an opportunity to ask him a few questions had I known that he knew such things. But maybe it was good that I didn't know and that I didn't bring him into a difficult position because, as I also found out later, he had not always been happy with his work during the war. So Rohrbach is a man I remember, next to Furch, quite well. As for the other colleagues, I would not have much to say.

## BACK IN MUNICH: 1963

FRITZ BAUER
That time in Mainz ended as suddenly as it had started. In fall 1962, there was an IFIP[13] congress in Munich; the congress after the first meeting in Paris was brought to Munich, Sauer and Piloty mainly had worked for this. I had already been acquainted with the IFIP machinery, so I was at that congress, but I knew already that there was something coming in Munich. That's because Sauer had phoned me in the summer, asking: "We may have a professorship, an extra professorship, an additional professorship pretty soon, would you think of coming back to Munich?" So I said to Sauer, "Of course I would like to come back to Munich, but you know that one should not go back to the same place where one was as a young man. I was your assistant and now I would be your colleague, this is normally not the way it is done." And Sauer said, "Yes, but it has advantages. I know you and you know me."

ULF HASHAGEN
I understand [laughter] –

FRITZ BAUER
So this problem was easily solved, and during the time of this congress I already went to the Ministry of Education in Munich. I was asked to discuss my coming and in fact I accepted during the time of the congress.

ULF HASHAGEN
And this was before you got an official call.

FRITZ BAUER
No, I had the official call – that all went very suddenly. I had the call, and there was this Munich Congress, and I was coming to Munich anyhow, and I had a few questions with Sauer, and I was sent to the Ministry –

ULF HASHAGEN
And this was a new full professorship –

FRITZ BAUER
A new full professorship. It was a parallel professorship, as it was

[13]IFIP: International Federation for Information Processing. Founded 1960

called, because we had so many engineering students at that time that the Ministry had decided they had to have, apart from the two regular chairs, they had to have two –

ULF HASHAGEN
Parallel –

FRITZ BAUER
Parallel chairs in order to be able to cope with the situation: we didn't have lecture halls big enough for all the engineering students. So that was one of these two parallel chairs. In the meantime I had said I would take up the position in a few months' time, but in Mainz I had difficulties with the dean of the faculty at that time. I had asked for particular support, in a particular tiny problem, and he had not shown interest in helping me, so I was a little bit angry. I told him, "Okay, Munich wants me, so I will leave Mainz at the end of the year," that means in the middle of the term. They had not expected that I would do this, but I was decided that I would want to go away, and it had the effect, which turned out to be quite useful, that I didn't have to give a class in Mainz in the winter term because I would go away in the middle of the term, and I couldn't start a class in Munich –

ULF HASHAGEN
In the winter term –

FRITZ BAUER
In the winter term. So I had a free semester, which was quite good because I could prepare myself for the life in Munich. It was of course clear: In Munich I had to work hard, I had to build up something. And this was one of the things I discussed with Sauer when I was at the computer congress in Munich. "What do you want me to do, what are your plans? " And he said, "I think you know what you want to do, and I think I have a feeling for what you want to do. That's okay and I will support you whatever you do in this framework." So he gave me more-or-less *carte blanche* that he would support me. He knew, of course, what I would be most interested in – and in fact I knew it too – so I think it was a fair agreement. I didn't cheat him and he didn't cheat me.

ULF HASHAGEN
What were you most interested in at this time?

FRITZ BAUER
Yeah, by that time I was already standing on the second leg. My first

leg was numerical mathematics, but I had already developed so much that I was deep into computer science in the proper sense, that means in programming theory. Programming languages, in particular, was the field I was working at that time. I tried then for the next few years to combine these things: to do still some work in numerical analysis, which would bring me to the computer, and to do some work in what came to be computer science, programming languages, in order to be able when I got to the computer to make the best use of it. It turned out that this was a very good combination. And this I started on January 2, 1963 – I had left Mainz at the end of the year – and I stayed on then for the rest of my life in Munich. But there was another thing that came up over the years. I had always worked on numerical problems but that new field, computer science, absorbed more of my time than I had expected. And although I had originally been on a chair that was for the teaching of engineering students, more and more I was teaching students of all sorts of mathematics and physics, and so on, not necessarily in the engineering sense. And I was still using a professorship in mathematics for doing this: so it seemed to be more –

ULF HASHAGEN
Appropriate to –

FRITZ BAUER
Appropriate to think how would I adapt myself to the changed situation.

ULF HASHAGEN
Yes, I understand.

FRITZ BAUER
So, during this time when I was more and more oriented towards computer science, computer science developed into an independent direction. That was not only so in Munich, that was so at a few other places, and one would expect that pretty soon computer science would become an academic field. So there would be, and should be, new professorships. And there was also a program of the German Federal Government to support the upbringing of computer science and founding chairs in computer science. In the line of this development there was also a possibility for Munich, an obvious possibility for Munich. So actually some four new professorships for Computer Science were coming in the years around 1971-1972. When it actually had worked out then I discussed it with my colleagues and said, “Wouldn’t it better that I change into a chair for computer science and

give away my chair for mathematics so that you can call for a mathematician in the proper sense, a mathematician in the classical sense for doing the work that you have to do." And they agreed that this was an obvious good decision. So I changed then positions in 1972, although in fact I changed only the label. I asked that I would then have a chair for mathematics *and* computer science and my colleagues were kind enough to allow me to call myself also a mathematician from then on.

ULF HASHAGEN
As far as I know, your research agenda changed during the first years in Munich after 1963 again. And after 1968 you more and more changed to research in computer science and you stopped your research in numerical analysis. Is this true?

FRITZ BAUER
Yeah, that's true. In fact, in 1973 or 1974, I practically stopped doing research at all anymore in numerical analysis. I stayed always a little bit interested, I tried to keep up with developments, but I was no longer pushing it myself. I have to mention Samelson, because most of the life I have explained so far, and also the rest of my life as long as Samelson lived, was in close contact with him. I had, in 1952, helped him to come to Sauer. Likewise in 1958, I tried to bring him to Mainz and actually after three quarters of a year it worked out. And when I went back to Munich I asked Sauer, "wouldn't you think we should have Samelson back in Munich? " And Sauer said, "Okay, fine." He agreed to it and his colleagues also were sympathetic to this. So, again, three quarters of a year after my appearance in Munich – that means in fall of 1963 – Samelson got a call on another one of these parallel professorships in Munich.

ULF HASHAGEN
This is an interesting story, which is not very usual in the academic field or … do you agree?

FRITZ BAUER
I agree, but Samelson was not a very usual case. He was a very particular case, not only for me, but quite generally. So it was the best that could happen to me to bring him back to Munich, and we could continue our joint work. We could have continued it anyhow, but it was much simpler, much nicer to do it the same place. The sad thing is that Samelson did not have a very long life: in 1980 he died.

ULF HASHAGEN
How long did you teach at the Technical University of Munich then?

FRITZ BAUER
I then went on teaching in the 1970s, when I had changed to the computer science chair, and teaching in the 1980s. In 1984 I had a heart infarction and I was for a while little bit more restricted, but it has improved again. But a new health problem came up slowly in the second half of the eighties. When I was approaching sixty-five that was in the year 1989, it was the earliest moment that I could retire. I could have stayed on three more years, but my health condition was such that it wouldn't have been wise for me not to retire. And so I asked the ministry for retirement *(Emeritierung)*, and of course I was granted it. It actually happened that two years later, in 1991, I needed a bypass operation, but from then on my health was much better. That means had I waited long enough I would have been in the very difficult position of a man of sixty-eight years who suddenly is unemployed and doesn't know what to do. So in 1989, when my health was not so well, I had very good reasons for cutting off some of the ties I had, for stopping some work that I never liked to do (although I didn't know that I didn't like it, I only found out later on that I had not liked it). So, it was the best that could happen to me, that I was sick enough when I got my retirement and my health was quite a bit better later on, for the next ten or so years after 1991.

ULF HASHAGEN
As far as I know you changed fields a little bit – fields of research – after your retirement or quite a little bit before you got into the history of computer science and the history of mathematics, and in museum business.

FRITZ BAUER
Yeah, yeah. That also is connected to my health situation after 1984, when I had the first heart problems. When they came back again I started discussions with Otto Mayr, the director of the Deutsches Museum on building up a computer science exhibit. And since our talks had started in 1984, I tried in 1985, in 1986, to get a group of colleagues that would cooperate with me. And at that same time my health didn't improve, so I found it difficult to do my university duties *and* the buildup for the museum. But the Ministry of Education was helpful, they allowed me a reduction of my teaching obligations to two hours per week. So practically I was *beurlaubt* –

ULF HASHAGEN
On vacation –

FRITZ BAUER
On vacation, although I took that vacation in the Deutsches Museum. In 1988, we opened the exhibit on computer science and it helped me at that time as far as my health was concerned that I had a much freer way of arranging my day. It also helped me to follow a little bit my interests in history, particularly in the history of mathematics, the history of computer science, the history of mathematical instruments, and the history of computing machines, which had developed over the years. So that then, of course, I continued and still do it today. Another thing happened after my retirement, that I found time and opportunities to do something in a field that I had always looked at with great interest but in which I had never published anything before – that is cryptology. It is a story I told already before, how I got acquainted with cryptology in the early time of my life by a book Britzlmayr gave me, a book on cryptology, the best book at that moment.

ULF HASHAGEN
Britzlmayr was a professor of –

FRITZ BAUER
Of logic in Munich. But now I had time. On the other hand, I had in the meantime tried to keep my eyes open, in particular my ears open, to hear what had happened. And I found out that many of my colleagues had worked in this field, not only in Germany, but in England and in the United States, and also hadn't always told me that they had experience in this field. So I had built up a kind of larger experience (although coming from other colleagues) and then I could write books. This is a field one can write books about. This is kind of my *Spätberufung* –

ULF HASHAGEN
Late call –

FRITZ BAUER
So Samelson, as I already said, died too early. And Rutishauer too. Rutishauer died in 1970, Samelson in 1980, but that's it; I am still alive.

ULF HASHAGEN
And you are still working.

FRITZ BAUER

I'm still working. I'm now eighty years old, and I hope I can do it for a few more years.

**NUMERICAL LINEAR ALGEBRA: 1955–1974**

ULF HASHAGEN
Professor Bauer, when and for what reason did you come in contact with numerical analysis? Was the work on your habilitation thesis your first contact with numerical analysis in 1954?

FRITZ BAUER
No, it started much earlier. As mentioned before, Bopp gave me one day a little problem saying, "Could you please calculate this." It turned out that it meant I had to determine the eigenvalues of a four by four matrix, and that was absolutely new to me. I didn't know much about how to do it or how to do it in the best way, all I had at hand was a pencil and paper. I didn't even have a machine, not even a desk calculator, for doing this. The script of Bodewig was a good, rather complete essay on classical numerical methods in linear algebra – in particular linear eigenvalue problems. But that made me aware that something like numerical mathematics exists. I didn't have many other stimulations, except that I knew that Piloty worked on filter theory, on electrical filters, and I heard from him (or from somebody else) that one of the problems in filter computations is that you have to calculate all the zeroes of certain polynomials of higher degree. This is critical because these zeroes are sometimes in clusters; it means some zeroes are very close together and it's difficult to separate them. But they have to be separated because this is just part of the problem – to determine, for example, which ones are in the right half plane, which ones are in the left half plane, as in stability problems. So I knew that these roots of polynomials and eigenvalues of matrices, would be an interesting thing. This was also, as said above, the subject of my habilitation thesis, where I invented a method of doing it with quadratic convergence – quadratic convergence because this is the kind of convergence one has for Newton iteration, and this was something one wanted at least to have. Later I was sometimes interested in cubic conversions, but sometimes cubic is even too good. So quadratic can be good enough for many problems. And from then on, of course, from time to time I was exposed to problems that led into numerical analysis, but mainly of the matrix kind, mainly of the linear algebra kind.

ULF HASHAGEN
Were there other mathematicians at the University of Munich or the Technical University of Munich who were interested in numerical analysis, or – ?

FRITZ BAUER
At that time I think I was the only one. Of course, Samelson, he was interested in everything and was interested in what I did. So he was certainly my partner in many cases. But it was not his main subject, he just simply had a very wide range of interests.

ULF HASHAGEN
On the other hand, and as far as I know, there was a group of mathematicians around Sauer who were interested in applied mathematics at this time.

FRITZ BAUER
Yeah, but this was the gas dynamic group, calculating supersonic flows. This was a subject Sauer had always been interested in, during the war he had already made computations for supersonic aircraft, solving hyperbolic differential equations..

ULF HASHAGEN
And did they use numerical methods in order to solve such problems?

FRITZ BAUER
They needed it. They couldn't do it without numerical analysis, without doing number calculations. The subjects are usually so complicated that you cannot simply do it by analytical methods.

ULF HASHAGEN
For what reason did you come to numerical linear algebra, not to the numerical solution of differential equations? Was it from your educational background –

FRITZ BAUER
My educational background was Perron, also Hasse – I had learned my algebra first from Hasse, without ever having seen him. So I was oriented towards algebra and not so much towards differential equations. But also working with Sauer – since analysis was his field, I thought it would be wise that I do something else. Sauer was also interested in this: that I would be independent, that I did not just follow his work, that I had my own field, that I develop my own group. So he was interested in it and he also let me do it this way.

ULF HASHAGEN
Was this the usual way he did work together with his assistant professorships, or with the lecturers he was leading? Were there other

mathematicians in this gas dynamic group – ?

FRITZ BAUER

There was a group of usually about half a dozen people that changed over years because some went away and some grew up and got some various professorships. He was very protective in this sense, but I had not so much contact as I could have had if I had wanted. But I also didn't want it. I didn't want to go into competition with him. Among the young people that worked with Sauer, I should mention Hans Stetter, who later went to Vienna University, and in particular Roland Bulirsch, who went first to Köln, then to San Diego and came back to Munich. He covered the field of the numerical solution of differential equations.

ULF HASHAGEN

Okay, I understand. Professor Bauer may I ask you, in which way did the environment of the Technical University influence your research in numerical analysis? Did the engineering professors show up with mathematical problems and ask you to help them?

FRITZ BAUER

Yes, engineering professors showed up from time to time with mathematical problems, but this does not necessarily mean that it influenced my own research in numerical analysis. The methods I was interested in were usually applicable in a wide range of fields, and still they were not necessarily the ones that *our* engineers would look for. But I had from time to time attempted to help somebody, to bring him closer to a solution to the extent that I knew how to help or to what person I should send him for help. So I was in contact with a number of engineering colleagues without having done research for them; I just simply had found out what they did and maybe could from time to time make a useful remark. I remember two or three cases where such things actually went a little bit deeper. One example is in civil engineering when stability problems – oscillation and resonance effects – had to be calculated. I remember another case where plates had to be calculated, the stability of plates and the forms of how they are swinging. There was of course from time to time interest from the electrical engineering side, in particular from calculations of power networks, electrical power supply. This was something that led deeply into numerical analysis of the linear algebra type. In fact, it meant that we had very big systems of linear equations, much too big one would say. Before we had a computer available, we couldn't solve them. But with the PERM then at hand, it was not too difficult to solve it. It was something of like

fifty or sixty equations for the same number of unknowns that was then solved within about an hour or two hours, which was absolutely new and was for the power engineering people of course something that they were very interested in. My old friend and colleague Richard Baumann specialized in this field.

ULF HASHAGEN
Did they, or did you, use mechanical hand calculators before the PERM computer was available in Munich?

FRITZ BAUER
Yeah, we did. First, sometimes we had to do test computations before we had the computer working, just testing a method. Then we had hand calculators, of course. And also we had a practicum –

ULF HASHAGEN
In order to learn how to –

FRITZ BAUER
In numerical analysis. In order to do this, students were given such instruments. And of course we had to prepare it, so we had to do it sometimes ourselves before we would give it to the students.

ULF HASHAGEN
Professor Bauer, during the decade after your habilitation you published about two dozen papers on numerical analysis. I would like to get you to say a little bit about the field of research, or the fields of research, you were working on in numerical analysis.

FRITZ BAUER
Yes. As you say, this numerical work started about the moment I had had my habilitation, and the first published paper with the title, "Contributions for the Development of Numerical Methods for Program-Controlled Computers: Quadratic Convergence of the Bernoulli-Jacobischen Method for the Determination of Zeroes of Polynomials."[14] This quadratic convergence, mentioned before, was a theme that I had for a few of my papers in the next years. The problem was quite simply this: quadratic convergence was known for the

[14]Bauer, F.L., "Beiträge zur Entwicklung numerischer Verfahren für programmgesteuerte Rechenanlagen. I. Quadratisch konvergente Durchführung der Bernoulli-Jacobischen Methode zur Nullstellenbestimmung von Polynomen." *Bayerische Akademie der Wissenschaften. Mathematisch-Naturwissenschaftliche Klasse. Sitzungsberichte*. (1954), pp.275-303.

Newton process and of course all the people who knew this would be very disappointed if they had to work with linear convergence, since this linear convergence can be so terribly bad that one just simply cannot wait long enough to finish the work. Quadratic convergence, once it goes, goes faster and faster. It may happen sometimes that quadratic convergence has a very slow start, that it takes a while, before it goes into running. But then if you can wait long enough you will always gain compared with linear convergence in the long run. Another paper that I wrote during this time, in 1954, was "A Method of Shortening the Iteration for Algebraic Eigenvalue Problems, in Particular for Determining the Zeroes of a Polynomial."[15] And this was, of course, closely connected to the work my boss Piloty did, who had to determine zeroes of polynomials when constructing electrical filters. So that was a good reason for investigating these things. At that time I had already contact with Rutishauser. And Rutishauser had, at that moment when I discussed this with him, not solved satisfactorily these quadratic convergence problems; he had published the LR ("left-right") transformation, which is a linearly convergent process. I said to Rutishauser, "This is very slow, couldn't you find a way of doing it with quadratic convergence?" Since these linearly convergent LR transformations had to do with matrices the idea in the back of my mind was: "If you do repeated squaring of the matrix that is in question, then you get something like quadratic convergence; but how would you combine this with Rutishauser's LR transformation, how would that go together? " And we sat together one evening and in the end of the evening we had a method and we wrote a paper together. And it was then published. That was the first paper that I had published jointly with Rutishauser, in the French Academy of Sciences.[16]

ULF HASHAGEN
Professor Bauer would you please tell us a little bit more about Professor Rutishauser?

FRITZ BAUER
Well, Rutishauser was the mathematician in the team Eduard Stiefel had built up in Zürich. The engineer was Ambros Speiser and the mathematician was Rutishauser. Everything was around Rutishauser or

[15]Bauer, Friedrich L. "Das Verfahren der abgekürzten Iteration für algebraische Eigenwertprobleme, insbesondere zur Nullstellenbestimmung eines Polynoms. Z. Angew. Math. Phys., v.7, 1954, pp.17–32.
[16]Rutishauser, H. and Bauer, F.L., "Détermination des vecteurs propres d'une matrice par une méthode iterative avec convergence quadratique," *Comptes Rendus Hebdomadaires des Séances de l'Académie des Sciences*, v.240, 1955, pp.1680-1681.

was around Speiser, since they had already been working for a number of years. So they were the central helpers of Stiefel. Of course, my contacts with Rutishauser have been much more fruitful than my contacts with Speiser, although I happened to know him well and I sometimes had discussions with him. But Speiser would have been the right man for the engineering group in the Munich Institute, and they had already good contacts anyhow. Rutishauser was a man who had written a thesis on continued fractions. Although I later became familiar with continued fractions, at that moment continued fractions didn't matter along the line of Rutishauser's LR transformation. So these were three cases where a known method was made quadratically convergent. The next thing that would be a necessary follow-up was: quadratic convergence wouldn't help if you wouldn't have a stable computation, if you would get into instabilities you would pay too much – quadratic convergence would get more and more inaccurate and it wouldn't be interesting. So the next problem was how could you avoid what had troubled Piloty when he had determined all the roots of a polynomial and then had built up the two polynomials again (the Hurwitz decomposition): that this was so terribly instable. No, the point is you have to factorize a polynomial *directly*, not by determining the roots and then going back and building up the two polynomials. But just how could you do it? So this was the problem of direct factorization, published in 1955 "A Direct Iteration Method for the Hurwitz-Decomposition of a polynomial"[17] and in 1956 "Direct Factorization of a Polynomial"[18] , the next of the "Contributions for the Development of Numerical Methods for Program-controlled Computers", published in the *Bavarian Academy of Science Proceedings*; one was presented by Robert Sauer, the other one by Josef Lense.

The idea worked in a number of directions. One direction was: when you iterate a vector by repeated application of a matrix, you will necessarily get the largest eigenvalue (or the eigenvalue of largest modulus). But you may sometimes want another eigenvector. What can you do? You had to directly compute the eigenvector belonging to the second or third eigenvalue, and not by simply first computing the dominating eigenvector and then doing something to get rid of this part of the matrix that would lead to this eigenvector. So the idea I found

[17]Bauer, Friedrich L. "Ein direktes Iterationsverfahren zur Hurwitz-Zerlegung eines Polynoms." Archiv für Elektronik und Übertragungstechnik. v.9 (1955), pp.285-290.
[18]Bauer, F.L., "Beiträge zur Entwicklung numerischer Verfahren für programmgesteuerte Rechenanlagen. II. Direkte Faktorisierung eines Polynoms." *Bayerische Akademie der Wissenschaften. Mathematisch-Naturwissenschaftliche Klasse. Sitzungsberichte*. (1956), pp.163-203.

was paralyzation. You tried to paralyze the iteration steps on some system such that the second eigenvector and the third eigenvector and the next ones are *forced* to go *not* in the direction of the larger ones, that this is prevented. So whenever it will work, the eigenvectors would be computed independently. Of course this is a problem that can easily run into instabilities and so the whole thing was how to make it stable. This was done by so-called bi-iteration: you iterate from the left hand side and from the right hand side, and have two systems that mutually orth ogonalized during this process.

Another example was the Hurwitz decomposition. This is maybe the cite that I liked most in all my papers in numerical analysis: a method which *directly computes* the Hurwitz decomposition of a polynomial. Unfortunately, I didn't find a quadratically convergent method; the method I found is only linearly convergent. And all my life I was sort of disappointed that I didn't find a quadratic convergence for the Hurwitz decomposition, which otherwise was a fine thing and which I enjoyed very much.

ULF HASHAGEN
For what reason did you enjoy, or do you enjoy this paper, so much? Is it a question of mathematical style or – ?

FRITZ BAUER
Yeah, it is so absolutely surprising that you *can do it* at all. You have a polynomial and you want to decompose it into two factors: one has all its eigenvalues, all its roots, in a particular region and the other has them in the complement of that region. That you can do it, that you can separate them: it is a matter of elegance –

ULF HASHAGEN
I understand.

FRITZ BAUER
In particular, if these roots sometimes are very close. If it happens that one root is very, very close to the boundary of the region you are wanting and the other one is very close to the boundary of the complementary region, then of course it had to be difficult and it would take longer, so a linear convergence process would have more difficulty in this situation. This is what one would expect. But finally a quadratically convergent method would go faster.

ULF HASHAGEN
Okay let's follow up your research in numerical analysis a little bit

more.

FRITZ BAUER

I tried to understand quadratic convergence when I started to reverse the process that had happened to my own thinking. First, the Newton process was something with quadratic convergence and then one would look for quadratic convergence for other methods. Now I turned it around and said, "How does it come that the Newton process *is* quadratically convergent? What would you do in order to find out what one had to do in other situations? The Newton process is normally defined for polynomials, what would you do when you have to do it in quite different circumstances." This is the topic of another paper I wrote, "The Newton Process as a Quadratic-Convergent Shortening of a Linear Iteration Process."[19] The typical examples were Bernoulli-type convergence and Gräffe-type convergence[20] .

Later, in 1967 and 1968, I came back to this whole idea in a particular aspect. It was known that by a linear shift of the matrix you can sometimes improve the convergence. What shift would you do in order to make a linear convergent process quadratically convergent by the right shifting? This was something in a paper in 1967, based on the QD Algorithm, which was originally, in Rutishauser's form, linearly convergent; the challenge was to get it quadratically convergent by appropriate shifts[21] . Of course, if you do the wrong shift you wouldn't gain anything, so you have to find the appropriate shift. This is the problem that I had to solve. And a year later I did the same for the QR transformation of Rutishauser[22] . So this was something that came late; I don't know why I had not looked into it before, but it was not too late to do it at that moment. All this work was centered around linear algebra problems. The matrix was a kind of a known center of all these considerations, but it was sometimes necessary not to work with the matrix but to go the secular polynomial – to the polynomial whose zeroes are the eigenvalues – and then the question was how do you do this. The Danilewski method was known at that time; it would give in a nice way a secular polynomia; I reported in Dresden 1955 how to do

[19]Bauer, F.L., "Der Newton-Prozess als quadratische konvergente Abkürzung des allgemeinen stationären Iterationsverfahrens 1. Ordnung". *ZAMM 35* (1955), p.469-470

[20]Bauer, F.L., "On Modern Matrix Iteration Processes of Bernoulli and Gräffe Type". J. Assoc. Comp. Mach. v.5, (1958) pp.246–257.

[21]Bauer, F.L., "QD Method with Newton Shift". Stanford Comp. Sc. Dept. Techn. Rep. CS 56, March 1, 1967

[22]Bauer, F.L., C. Reinsch "Rational QR Transformation with Newton Shift for Symmetric Tridiagonal Matrices". *Numerische Mathematik*, 1968, v.11, pp.264–272

this. And I wrote then the paper with Householder, in 1959, "On Certain Methods for Expanding the Characteristic Polynomial."[23] This was my first paper I wrote together with Alston Householder, whom I had met with in the United States for the first time in 1958; I had met him three years before when he was visiting Alwin Walther in Germany. And from that time in 1958 and 1959, I visited him practically every year in the summer for a few weeks. I stayed in Oak Ridge and I enjoyed working with Alston Householder very much.

ULF HASHAGEN
Could you please tell us a little bit more about the mathematician Alston Householder? For what reason did such an intensive collaboration evolve? Did you like his style of mathematics, or for what reason was this such a good collaboration?

FRITZ BAUER
I think in the first line I liked him personally. And that means the kind of style he had in his publications; the kind of approach he had in his work was also something I liked. So it just happened that I found somebody whom it was a pleasure to work with. I could have thought of a few more colleagues in the United States: George Forsythe, Wallace Givens, Harry Huskey that would have been of this fine kind, but it just didn't always work. It is not enough for me to want to work with someone – you have to find somebody who also wants to collaborate with you. So with Householder it worked from the first day, it was just a kind of a spark that started my work with Householder.

ULF HASHAGEN
Did you have such an intense collaboration with Rutishauser too, or was the intensity of the cooperation with Householder much greater?

FRITZ BAUER
I was usually visiting Rutishauser from time to time during the year, and I was visiting Householder every year in the summer. So I sort of alternated. I couldn't always go to Oak Ridge for a few days, so when I went to Oak Ridge, I went for a few weeks. But with Rutishauser we met frequently, he came to Munich and I came to Zurich. In fact, these were the two people I worked most closely with during this time, apart from Samelson who always was around me. There were other papers I wrote in this time, one paper with the title "Moments and Characteristic

[23] Alston S. Householder and Friedrich L. Bauer, "On Certain Methods for Expanding the Characteristic Polynomial," *Numerische Mathematik*, 1959, v.1, pp.29-37.

Roots," that means I studied the connections between the so-called moments of a matrix and the characteristic roots.[24] This was something I think was also new at that time. Another paper I wrote with Alston Householder was "On Certain Iterative Methods for Solving Linear Systems."[25] This was a paper where we made extensive use of norms in order to get bounds and to get the estimations that one needed. And slightly earlier I wrote an overview, a survey paper, "Connections between Certain Iteration Methods of Linear Algebra," where I tried to connect methods that had so far been discussed independently in the literature to see to what extent one could be derived from the other, or in what extents are they different when they could not be directly derived one from another, when they are not completely equivalent.[26] Another field that became more and more important for me was algorithmic thinking. Of course, I had always constructed algorithms when we did this numerical work, but that the algorithm as such would be the kind of thing one would use in finding a name for a particular method – that was new. Rutishauser had started it with the QD algorithm. I discovered a few more algorithms. First, in the sense Rutishauser had started it, you have a certain problem and that means you have a repeated process – it is like knitting with needles – and this goes on and on. There was one algorithm I discovered at that time in connection with continued fractions, the so-called g-algorithm, and I found a few nice connections to algorithms existing in the literature.[27] One algorithm that was published at this time, when I had been in Mainz, was by Peter Wynn: He invented the epsilon algorithm (å-algorithm), which was also an algorithm that did something reasonable, something connected to determining eigenvalues, but it looked quite different from Rutishauser's QD, quotient-difference algorithm. In fact, Wynn was so proud that he had discovered something different. And I wrote a paper showing that QD and epsilon are related.

ULF HASHAGEN
In which way?

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[24]Alston S. Householder and Friedrich L. Bauer, "Moments and Characteristic Roots," *Numerische Mathematik*, 1960, v.2, pp.42-53.
[25]Alston S. Householder and Friedrich L. Bauer, "On Certain Iterative Methods for Solving Linear Sysstems," *Numerische Mathematik*, 1960, v.2, pp.55-59.
[26]Friedrich L. Bauer, "Zusammenhänge zwischen einigen Iterationsverfahren der linearen Algebra: Aktuelle Probleme der Rechentechnik," *Bericht über das Internationale Mathematiker-Kolloquium, Dresden, 22. bis 27. November 1955*, pp.99-111.
[27]Friedrich L. Bauer, "The g-Algorithm," *Journal of the Society for Industrial and Applied Mathematics*, v.8, 1960, pp.1-17.

FRITZ BAUER

In this way: that the one is kind of recoding of the other. You sing your melody in a different arrangement, but you can from a general algorithm derive the one and the other, and the general algorithm which worked for both cases, that was the g-algorithm that I mentioned before. The paper on the quotient-difference algorithms and the epsilon algorithms was combining Rutishauser and Wynn, this was presented in a conference that was held by Rudolph Langer, at the University of Wisconsin in 1959.[28] I remember very well this particular place. It was something different from Oak Ridge, it was something different from Householder. Langer was supported by the Army, not by the Atomic Energy [Commission], and that made of course a completely different style. That, I remember, was a kind of a surprise to me.

ULF HASHAGEN

Could you please tell us a little bit more about this different style?

FRITZ BAUER

No, just that the Army is much less fashionable than the Navy is, and the Navy is much inferior to the Atomic Energy people. [Laughter] That's a matter of style. For example, when I was transported over the ocean by MATS, the Military Air Transport System, normally I went with the Navy, that means under Navy auspices, and I was usually seated in the general's machine that started every week from Frankfort. But when I was invited by the Army, I was just in the GI machine. [laughter] In the 1960s a new theme came up for me, so-called norms. And I wrote a number of papers related to norms, connected to another concept that became important: fields of values. The normal field of value as everybody knew it at that time was a field of value subordinate to a quadratic form that includes the eigenvalues. But it was to me obvious that just a particular norm was used, the Euclidean norm. And whenever you have any other norm, when you are able to define the conjugate norm to this given norm, then you just do with this conjugate norm and the given norm what you otherwise would do with your Euclidean norm from the left and from the right: You use a dual pair of vectors. And you get fields of values that still include the eigenvalues, but sometimes you get much better fields of values. For example, the

[28] Friedrich L. Bauer, "The quotient-difference and epsilon algorithms," pp.361-370 in *On numerical approximation: proceedings of a symposium conducted by the Mathematics Research Center, United States Army, at the University of Wisconsin, Madison, April 21-23, 1958*, 1959, ed. Rudolph E. Langer, *Publication No.1 of the Mathematics Research Center, U.S. Army, the University of Wisconsin*, University of Wisconsin Press, Madison.

Gershgorin disks that everybody knew at that time, can be obtained by using a particular norm, the maximum norm, and its conjugate, of course, is the sum norm, duality leads here to the consequence that only a finite number of dual vector pairs exists. There are many other norms in between that you can use in order to get better estimations. And there was another paper with Ted Fike, "Norms and Exclusion Theorems," that was very much connected to fields of values: find regions where the eigenvalues cannot be, then they have to be in the rest.[29]

ULF HASHAGEN
Who was Ted Fike? Was he from the U.S. too?

FRITZ BAUER
He was working at Oak Ridge at that time. I didn't keep contact with him later on; he left and went into industry. Another problem that was coming up was the awkward condition of a matrix. A matrix can be rescaled. Rescaling it by changing the exponent only is a very simple process, in fact it doesn't make any rounding errors, but by rescaling a matrix you can give it a better condition. That means that the eigenvalues are not so much spread out. And there are certain inequalities that involve the Euclidean condition of a matrix, therefore conditioning of a matrix is an interesting field. It also had to do with what I mentioned before, with the field of values, that you get better fields of values. To aim at a better field of value and to aim at a better condition is a process of the same sort, and one and the other are very much aiming in the same way.

ULF HASHAGEN
I understand.

FRITZ BAUER
Yeah, that is about what I did in numerical analysis in those years. My list ends around 1968, and since then I worked almost exclusively on computer science themes, programming languages, programming strategies, software engineering.

ULF HASHAGEN
For what reason did you leave the field of numerical analysis?

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[29]Friedrich L. Bauer and C. T. Fike, "Norms and Exclusion Theorems," *Numerische Mathematik*, 1960, v.2, pp.137-141.

FRITZ BAUER
First, this work with the computer and around the computer absorbed more and more of my time, and absorbed more and more of my interest. And programming languages was something one really had to do. Then going into a programming methodology was even much more difficult and much more needed. And then the subject of software engineering came up, and this was again something that one had to do. And after all, after about twenty years I had a sort of feeling that I had done enough in numerical analysis now and it would be better I do something else.

ULF HASHAGEN
Okay. Professor Bauer I would like to get you to say a little bit more about the way you came into contact with the American scene in numerical analysis. When did you meet Householder the first time?

FRITZ BAUER
Householder I met for the first time in 1955 at a meeting that Alwin Walther had organized in Darmstadt. There were quite a number of foreign guests, but Householder was among the ones I was most interested in. I had not seen him before but I had already known of his work, and it turned out that we had from that moment good relations. Later, starting in 1958, I was almost every year, in summer, in Oak Ridge; I visited Householder and stayed there and worked with him on numerical problems. He also came frequently to Germany, came to our house, and in fact in the end Householder married the elder sister of my wife.

ULF HASHAGEN
Okay.

FRITZ BAUER
Among the other people I met in the year 1957 – when I was sent for the first time to the United States, financed by the Office of Naval Research – were: at UCLA, George Forsythe, at that time, later I met him frequently in Stanford; Wallace Givens at that time; Gene Golub, already at Stanford; and Bob Bartels at Ann Arbor. And of course I visited places like NYU where Richard Courant was; I had already had discussions with Courant. These colleagues had made quite an impression on me and we had become very good friends. I spent a quarter of a year in Stanford, this was the spring quarter in 1967, invited by George Forsythe, who at that time was still alive. I had to give a class and I lectured on the theory of norms. I was very proud that

I was invited at all, and then that I was given an opportunity to speak on the theory of norms, which was something quite in development at that moment. Norms at that time were normally Euclidian norms, and I was concentrating on almost all norms except for the Euclidian one, about which practically everything was known already. I remember it so well because Forsythe had my notes published as an internal Stanford report and for many years I had given these reports to my colleagues and friends and students if they were interested in norms and if I wanted to bring them closer into this line of research. I also developed a friendship with Olga Taussky-Todd (30.8.1906 – 7.10.1995), who became the first female mathematician in the Bavarian Academy of Sciences, with Jack Todd and Hans Schneider, and with Richard Varga. Altogether I had many good friends in some other parts of the world. I had very good friends in England like Jim Wilkinson, in Italy, in Switzerland of course – Eduard Stiefel and Heinz Rutishauser mainly, and also Alexander Ostrowski in Basel. He was a very nice man, although not everybody would at that time see this so easily because he had a kind of a way, not always showing his appreciation. But if one would get in contact with him, then one had the feeling that he was an honest man, and this is something I appreciated very much in Ostrowski. There was one student of Ostrowski, Walter Gautschi, a Swiss who was at that time several years in Oak Ridge, which I remember very well because he was a younger man and I had many, many social contacts with Gautschi in Oak Ridge. I had even some friends in Praha, Ivo Marek, and in the Soviet Union. One was Andrei Ershov (1931–1988) in Novisibirsk, but Ershov was not a numerical analyst, Ershov was a programming man. He was one of the founders of informatics, quite generally. Unfortunately, he also died too early. Yeah, and that's all about my life in numerical analysis. In the 1970s I concentrated more and more on computer-oriented subjects. I organized a meeting oriented towards numerical analysis in 1974 in Enzensberg, near Füssen; a Gatlinburg meeting in the series Alston Householder had started in Gatlinburg. This was a kind of my adieu to numerical analysis.

ULF HASHAGEN
Professor Bauer, I thank you for this interview.