Interview with

**Hans Stetter**

conducted on
June 8, 2005,

at the Technical University in Vienna,

by Phil Davis

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This is an interview of Professor Hans J. Stetter conducted on June 8, 2005, at the Technical University in Vienna. The interviewer is Phil Davis.

DAVIS

I think that I would like to go back a little bit in history and ask you, Hans, when did you first realize that you had talent in mathematics? And I'm thinking of, perhaps, elementary school or something like that.

STETTER

Well, I think it was in a sense realized before I ever went to school, when I was a very small child. So, and ever since, I felt very happy with mathematics, never had any problems and always was kind of outstanding and excellent. So for me it was clear early in my young life that I would try to stay in mathematics, and I did.

DAVIS

Were your father and mother interested in this or did they do something before you went to school or something like that?

STETTER

Nothing, no, no, that was automatic in a sense, and it was recognized by my parents, and they were happy about it, particularly my father, and they never minded.

DAVIS

Was he also a professional?

STETTER

My father had a degree in zoology; actually he did this work at a later time of his life, with Professor [Karl] von Frisch, the famous bee researcher, on the hearing of fish. This was quite seminal work that they did at that time. But he was a schoolteacher, a secondary schoolteacher.

DAVIS

Going back to those years, do you remember some book or something that was influential for you as a young person, a book in mathematics that captured your imagination?

STETTER

Not really, not really. I think it was –

DAVIS

Self-generated?

STETTER

Of course, you see this was the very bad time. I went to secondary school from l940 to l948 –

DAVIS

That was a bad time.

STETTER

Yes, so we were happy to just survive.

DAVIS

Yeah, that was during the war.

STETTER

And right after the war.

DAVIS

Now as you went to higher grades, were there some outstanding teachers that influenced you at that stage?

STETTER

The mathematics teacher I had for the last years of my high school was quite old, and many of my colleagues didn’t like him very much. But I think he had a good way of promoting our interest in mathematics, and did motivate us.

DAVIS

And then after that you went to the university –

STETTER

I went to the university with the objective of also becoming a secondary school teacher because at that time there was no real other career in mathematics. So with a minor in physics, I studied for the secondary school examination, which is approximately like a masters, and during that time I spent one year in the United States as an exchange student.

DAVIS

Where was this?

STETTER

This was at Colorado Aggies [Agricultural College of Colorado] –

DAVIS

Colorado Aggies?

STETTER

It is now Colorado State University at Fort Collins.

DAVIS

I see.

STETTER

There were only four senior majors in mathematics; I was considered a senior. We did completely individual work with the two professors who were there, and I think they influenced me as well. I remember. That year I took part in the Putnam examination and got honorable mention, and also I presented a paper at the national convention of the undergraduate mathematics fraternity, and I got first prize.

DAVIS

Very nice. And then, of course, things changed, I suppose the opportunities opened up a bit? But you're originally from Germany is that correct?

STETTER

Yes, yes. I was born in Munich and had spent there all of my life, except for being abroad. Up to age of 35, I lived in Munich, and then I got the professorship [at Vienna]. I think the crucial event in my life was right after the completion of my masters when - due to fortunate incidence -

Professor Robert Sauer, of the Technical University of Munich, with whom I had taken a few courses, offered me a research position. This was particularly decisive because he was one of the few people who were already working in, what would be called, numerical analysis and in partial differential equations. Actually in the good old tradition, he was not just an analyst but also an applied mathematician. He had studied and written a book on fluid dynamics[1].

DAVIS

That's right, I recall reading that book, years ago.

STETTER

And he was now doing numerical methods for gas dynamics, and so what became my Ph.D. thesis then must have been one of the first theses in, what was later called, computational fluid dynamics. But it introduced me to numerical analysis and from that time on I researched it myself.

DAVIS

You would say that you're interest in applied mathematics dates from 50s.

STETTER

That dates from 1953.

DAVIS

Nineteen fifty-three. Wasn't Sauer in some fluid institute during the war?

STETTER

[1] Robert Sauer, Einführung in die theoretische Gasdynamik, Springer, first edition (1942).

Yes.  Actually, he was in the group of scientists who calculated the flight paths for the V1 and V2 rockets that were sent to Britain.

DAVIS

Well, you know in those days I was, well I'm a few years older than you, and I was working, it was during the war, and I was working at NASA, in those days it wasn't NASA, it was NACA [National Advisory Committee for Aeronautics], because the space program hadn't been invented yet.

STETTER

Yes, of course I still remember it, NACA.

DAVIS

As soon as the peace was declared, we received, at NACA, all kinds of secret documents from Germany in the fluid line, in the aerodynamic line, and so on. That's where I first came across Sauer's name because I was about 22 or 23 at the time.

STETTER

At that time he had a contract with the United States Air Force

DAVIS

This was in Vienna?

STETTER

No, it was in the 50s, in Munich, and this was what actually paid for my first employment.

DAVIS

What sort of a character, personality was Sauer?

STETTER

Oh, he was, I think, a very strong but not outspoken personality. He was an extremely good diplomat which later led him to all kinds of presidential posts, President of the Bavarian Academy of Sciences, Recktor of the Technical University, but this was after this work that I did with him. Unfortunately, he died at the age of 71. He was very friendly with his collaborators and, I think, he has influenced me a lot actually.  He was like a father to me.

DAVIS

In those days I don’t think the term "numerical analysis" was even around and there were no standard courses in that in universities.

STETTER

At the Technische Universität in Munich, they started such courses because there was an active group which consisted of Fritz Bauer and Klaus Samelson, and a few other people. So there was a very strong group in numerical analysis. If it was called anything, it was called numerische verfahren [numerical methods], and later numerische mathematik. The Technische Universität in Munich had one of the three computers that were built in the 50s in Germany. They were at Munich, Darmstadt and Göttingen. The Munich computer became operative in ’55, so my Ph.D. thesis computations were done on an electro-mechanical computer.

DAVIS

We’re talking about the first generation of digital computing. I remember also at that period we worked in a "mixed mode", sometimes we went on the first generation and at other times on the zeroth generation computers, these mechanical adding machines and multiplying machines. Was the same experience that you had? -

STETTER

Well, once the PERM, as it was called, was really operative I think we switched to that very quickly. Now of course it was extremely slow, so if I wanted to solve a hyperbolic partial differential equation I spent all night –

DAVIS

Absolutely.

STETTER

But it was so slow that from the lights on the computer you could tell which part of the calculation was being done when it was in an internal computation. That was really fun.

DAVIS

My computational experience goes before the electronic digital computer and it was so tedious, took so long to do things, I mean, almost impossible.

I knew Bauer –

STETTER

Yes, I suppose so.

DAVIS

Because after I got my Ph.D. degree, which was in pure mathematics actually, I went to work with The National Bureau of Standards, in Washington. The National Bureau of Standards had one of the first electronic machines called the SEAC, and there was a big group down there of people who were investigating the possibilities of these new machines with respect to numerical methods. You know, what methods worked and what methods don't work, and so on. There was a constant flow of people from Europe into our group. The group was run essentially by John Todd, of course, with his wife Olga. I remember at one time Bauer came along with a number of these people that you mentioned in this [document] that you wrote out for me. I didn't meet all of these people here, but I met [Lothar] Collatz, I met Fritz Bauer, I met [Eduard] Stiefel. I met [Peter]Henrici, in fact I shared an office with Henrici, and I met Alexander Ostrovsky. I rather liked Ostrovsky.

STETTER

Yes, yes, I mentioned the conferences in Oberwolfach. I think this was one of the real merits of Collatz in post war development of numerical methods; he was like the brain center. He organized two conferences every year, one in spring and one in fall, in Oberwolfach, which brought together all the younger people from German speaking countries and some foreigners, who worked in numerical analysis. We became a very closely knit group. This, I think, was the reason why numerical analysis was very strong in Germany in the 70s and 80s. I think it's good now, but now it's good all over. But at that time it was really only second to the United States.

DAVIS

It was a rare thing in the States also. Was there a struggle? I notice that you mention your interest in education, and you put in some of these courses, was there resistance?

STETTER

Not really.  No, no, there was another thing which was perhaps not a matter of course, but in the late 60s, middle to late 60s, chairs for numerical analysis were introduced in practically every full scale university. At this university [TU Vienna], [it happened] in ’65, and I was the first holder of that chair.  And so we had extremely good job chances at that time because there were not so many young scientists who had done considerable research in that area and had proved themselves to be good researchers and good teachers.

DAVIS

In the early days in the States we experienced resistance. Resistance came largely from the pure mathematical community. Even von Neumann experienced this resistance at the American Institute of Advanced Studies in Princeton, and I certainly experienced it at Brown.

STETTER

I would say what happened in German speaking universities was that the pure mathematicians looked down on us.

DAVIS

Looked down, yes –

STETTER

They didn’t consider us to be full-scale mathematicians. But the independence of the individual professors in the German university system is much greater than in the typical Anglo-Saxon system, and so this didn’t matter so much. As long as the administration, which was always the Government, was supporting us, then we were fine.

DAVIS

Collatz was, of course, of an earlier generation, and I think he wrote several books before the electronic age. He wrote one on eigenvalue problems[2] and there was another one on general methods I think.

STETTER

He was precisely twenty years older than I. He was born in 1910.

DAVIS

Yeah.  Some of these other people like Ostrovsky also
spanned –

STETTER

Ostrovsky was not really a numerical mathematician, but he was really interested in constructive approaches and –

DAVIS

He got interested in linear problems –

STETTER

Yes, yes, and polynomial problems.

DAVIS

Did you know Stiefel at all?

STETTER

[2] Lothar Collatz, Eigenwertprobleme und ihre numerische Behandlung, Leipzig : Akad. Verl.-Ges., (1945).

Yes, yes, because he was a good friend of Sauer, and he came to Munich quite often. So I met him on various occasions when he was in Munich, and also once or twice in Zurich. And I also [knew] Heinz Rutishauser. He died so early, he was a very vigorous person.

DAVIS

What would you say is the relationship between your Ph.D. work and work later on?

STETTER

Well, my Ph.D. was in the tradition, I would say, of the old work of Courant and his group. Courant in the –

DAVIS

PDEs?

STETTER

What do you mean?

DAVIS

Partial differential equations.

STETTER

Yes, PDEs, hyperbolic PDEs. Courant and his crew had developed the idea of characteristics and of the domain of dependence and so on, and so had [Hans] Lewy, Fritz John. And they had already some concept of numerical computation.

DAVIS

[Wilhelm] Magnus was there?

STETTER

Yes, but I don't think he worked in that area so much. And Professor Sauer, who was very strongly geometrically oriented, liked the idea of the mesh of characteristics and he found that this was really the natural mesh that should be used in nonlinear partial differential equations where the characteristics occurred and depend on the solution. So that you build the mesh of characteristics while you are proceeding in the solution. And this was one of the ideas which I followed later in my Ph.D. thesis. It was rather some work called "Interaction between the Flow about the Wing and the Fuselage", but with a very crude model mathematically, an infinite cylinder and an infinite plane front attached to it, but still I could use this method.

DAVIS

This was compressible flow?

STETTER

This was compressible flow –

DAVIS

Linearized compressible flow.

STETTER

The nonlinear part came only later. The linearization of course helped a lot.

DAVIS

That’s usually the first step.  You have done research in so many portions of numerical methods. Would you consider yourself a specialist or a generalist in numerical methods?

STETTER

I was, I think, a specialist over periods.  I started with hyperbolic partial differential equations, but after a while I found that as the subject developed, at that time I knew all the cream of the people working in that area Jim Douglas, Avrim Douglis, Seymour Parter and you name them. I discovered that in order to proceed further you would have to have a lot of technical knowledge about PDEs, which from my training I’d never even had. At the same time I had discovered that –

DAVIS

You mean theoretical material, pure mathematics ?

STETTER

Not pure mathematics –

DAVIS

Pure mathematics of PDEs.

STETTER

Yes, and at the same time I discovered that in ordinary differential equations the development of numerical methods was very rudimentary. And so I switched to ordinary differential equations, and for twenty years this was really my specialty.

DAVIS

This was where you did higher order difference methods?

STETTER

Yes, a lot of work and also very principal work. I also developed the terminology and the way of looking at things, and I think I have influenced this part of numerical analysis quite substantially. Also due to the book which I wrote in the early 70s which had a lot of principal developments and tried to –

DAVIS

This is the one you called "Analysis of Discretization Methods in Ordinary Differential Equations"[3]? So your work was aimed towards algorithms, towards error methods, towards theorems of some sort, asymptotics?

STETTER

Right, right, it was strongly aimed towards algorithms, but understanding the algorithms, understanding how they would work, what you had to watch in constructing algorithms. Because for ODEs, for first order systems of ordinary differential equations, it's so easy to write it down. Because you almost only had to discretize the derivative in a sense –

DAVIS

Then you worry about stability?

STETTER

Yes, and I introduced the terms local error, global error, and tried to really make that independent and enforce a normalization which would make things compatible between different authors and different ways of writing these things. And then, another subject that I think I pushed was

[3] H. J. Stetter, Analysis of Discretization Methods for Ordinary Differential Equations, Springer, 1973.

asymptotic expansions, both theoretically and practically in the use of Richardson's extrapolation. Richardson's extrapolation was something that I inherited from Sauer. We did Richardson's extrapolation on PDEs, already in the late 50s, beginning 60s, without much knowledge. But it worked, that was the fantastic thing.

DAVIS

Did you also experience some failures?

STETTER

Yes, we did experience failures. Of course, the interesting part was that we had to find out what the reason was. Almost invariably, it was something in the asymptotic expansion that was not according to the rule for certain reasons. So I think that the paper, I think it was '65 or so, about asymptotic expansions was also a seminal paper[4]. And then later this was also used on ordinary differential equations but there is a much wider spectrum of application. Like Richardson's extrapolation, there was the idea of defect correction[5], and this was interesting in the sense that in '73, at the biennial Dundee conferences, which were the big events in numerical analysis at that time –

DAVIS

Mitchell[6]?

STETTER

[4] H. J. Stetter, Asymptotic expansions for the error of discretization algorithms for non-linear functional equations, Numer. Math., 7, (1965).

[5] H. J. Stetter, The defect correction principle and discretization methods. Numer. Math. 29 (1978), 425-443.

[6] Perhaps a reference to A. R. Mitchell who started the conferences with Michael Osborne. The first two of which were in St Andrews and the rest in Dundee.

Mitchell, yes. I spent a lot of time in Dundee, in fact I knew Dundee much better than any other town in Britain.

DAVIS

We may have met in Dundee –

STETTER

I suppose so –

DAVIS

I think so. Mitchell invited me to Dundee.

STETTER

In this ‘73 conference, an Argentine astronomer gave a paper on the estimation of discretization error in the forward integration of differential equations of motion. And he had very impressive experimental material but he was not really able to underpin it with –

DAVIS

With some theorems?

STETTER

Well not a group of theorems but even to show why it would mathematically work.

DAVIS

What was his name, do you remember?

STETTER

[P. E.] Zadunaisky. And in the beginning, I really called it Zadunaisky's method. I was fascinated because most of the people said, well, that's crazy. This may work in this case but it cannot be a general approach. But I had immediately the feeling this is a very general approach of looking at discretization errors. It happened that we were on the same plane back to London, sitting next to each other. So I talked a bit more to him and when I returned I just sat down and tried to understand his work. And after a while I succeeded to really see through it and then in '74 I published the first paper in that direction, defect correction [see footnote 5]. It's also a very old method; it goes back to Richardson. I mean "The Deferred Approach to the Limit" is a title of a paper by Richardson[7]. By the way, Lewis Fry Richardson, I think, was one of the most important historical figures for numerical computation, because he did it at a time when nobody thought of it.

DAVIS

He was interested in weather.

STETTER

He was interested, and his book –

DAVIS

On weather prediction and so on -

STETTER

I think this is one of the most remarkable works in the history of computation.

[7] L. F. Richardson, The deferred approach to the limit. *Phil. Trans. Royal Soc. London, Series A* **226**: (1927) 299–349.

DAVIS

We're talking about a book called "Weather Predication by Numerical Processes", Lewis Fry Richardson[8] –

STETTER

This is in the 20s.

DAVIS

There is a Dover reprint, the original was in 1922, according to this. Was this your Bible for a while?

STETTER

No, not really a Bible, but I mean it is not the only thing he did. He did very intricate work, because he came from theoretical physics, and so on. He tried to include all kinds of effects, but he did not understand stability. Of course, at that time nobody could really do it.

DAVIS

No, you couldn't do it at that time.

STETTER

But he had also thought of a "human computer" [for his weather prediction]. Have you ever read that? He suggests that there should be an amphitheater to model the northern hemisphere. The inside of a hemisphere with little balconies, all around, to represent the nodes, and in each of these balconies there would be a few [human] calculators sitting and they would exchange data with the next balconies. And so this was a human computer that he conceived, and this shows his

[8] Lewis Fry Richardson, Weather Prediction by Numerical Processes, University Press (1922).

ingenuity, this fantasy. He thought that about five thousand people would be necessary to compute the weather on the northern hemisphere at a rate at which it actually was in real-time.

DAVIS

He anticipated the satellite reports on weather, I don't think we have five thousand stations even now.

STETTER

No, no, no. Five thousand people in that hemisphere, five thousand doing the calculations.

DAVIS

Five thousand people, not five thousand nodes.

STETTER

Of course, yeah. Of course you know that to do anything you have to have a sufficient rate of observations, which it was clear could not be realized at that time.

DAVIS

He has a map here where he wants to put the stations and the people, very interesting. Let's return to one of the things that you mentioned. What is your philosophy about the importance of numerical experimentation in mathematics, or experimentation via computer?

STETTER

I think it is still undervalued, particularly it's undervalued by many of the pure mathematicians because I believe that almost all of the great mathematicians relied very strongly on real examples first, in order to make their ideas concrete and to lead them to quickly to an abstract form. And the pity is that in many cases up to now we see only the final result, the abstract final result, but not the way in which it was conceived, not only elaborated, but *conceived*. I think that many important mathematical results came from an observation, from an observation in an example. Not now of course in abstract fields of mathematics, which are abstractions of abstractions, but in mathematics that is still, say, just one level removed from describing reality. In my own experience, I have very strongly been led by looking at large amounts of data after trying computing and, in fact, I think this was a particular achievement [by me]. When students came and showed me their output, at that time you had still these huge printers which gave you pages and pages of long many digit numbers. And I glanced over it and said, “Oh, here is an effect”, here you see that you wouldn’t expect these numbers to be like this and nobody else saw it.

DAVIS

So the inspiration that comes from the output is very important?

STETTER

For me it is, and I think for most numerical mathematicians it would be because we deal with numbers. It is our job. And very often an actual computation does not really behave like we predicted it to, because we can only model the computation and the model may not be completely relevant. So I think numerical work without experimentation is really impossible.

DAVIS

Is there a role for intuition? Does one achieve a certain computational intuition?

STETTER

From my own experience, I would say yes. I think I can say that in my active time, I have had that intuition because very often it did happen that the results that I published were really started by thinking about things, by doing experimentation, and by taking the right intuition from the numbers which I got. Of course, doing the right experimentations, this is really a key.

DAVIS

Well that's experience isn't it? To pick the right one or is it luck?

STETTER

I think, no, neither. I think this requires a lot of thinking and this requires you to have preliminary understanding of what's going on because you want to see certain effects. So you must construct experimentations which are likely to show these effects. If you just run some examples you will normally not see anything because you will not get the effects that you are really after. So experimentation is meaningful only when it has been well-designed and to design a numerical experimentation is something which I think should be taught a lot more in numerical analysis courses.

DAVIS

I agree with this entirely. You have worked in so many different fields it's hard for me to pick some topic for you to say a little more about. But here's one that appealed to me, methods for computer tomography. Could you say a few words about that? That's something that you've written about.

STETTER

I haven't.

DAVIS

No? Oh, then I made a mistake.

STETTER

Well, maybe you are referring to early work that I started with Fritz Bauer in Munich and then completed later on the discrete Fourier transformation[9]. But I don't really take credit in that direction, but this was also a seminal work. First with Fritz Bauer and then continuing with that myself. The fact that when you do discrete Fourier transform work what you are doing in terms of the spectrum, is a superposition, a periodic superposition, of the original spectrum. So, if you have a spectrum that damps out that is a good spectrum [for that purpose]. And the idea that we had was that when you know the damping law then from the periodic spectrum which you get from the discrete Fourier transform, you can work backwards to the true spectrum by numerical methods.

DAVIS

This is probably what, tomography? An inverse problem, is it?

STETTER

Yes, but I never worked explicitly on tomography.

DAVIS

I see. Let me change the subject a little bit. Have you done some collaboration with colleagues or with students or with teachers -

STETTER

Yes, of course I have with students, although, I mean I have had quite good students, but for some reason or another none of them has really achieved worldwide renown. Partially because some of

[9] F. L. Bauer and H.J, Stetter, Zur numerischen Fourier-Transformation, Numer. Math. 1 (1959), 208-220.

the students which I have had in Vienna as collaborators they didn't want to leave Vienna, and I think for a real career it's necessary to go to other places. But that's beside the point. So, what was the second point of view? Whether I collaborated, yes. I must admit that I am not a person who is very good in teamwork.

DAVIS

You do solo work.

STETTER

I am more of a sole thinker, but this is also in my character and I am rather introverted and this is not so good for teamwork.

DAVIS

There are different personalities in mathematics.

STETTER

True. But to compensate I have always tried to appear in many conferences in order to interact with other researchers in the same area, in conferences to see what they were doing, to show them what I was doing, and there were collaborations coming from that. The strongest collaboration I think I have had during the last part of my active scientific work; it was on numerical polynomial algebra. Because this was an area into which I tumbled without the necessary mathematical background. I had never really had any training in commutative algebra. Bruno Buchberger [Professor of Computer Mathematics at Johannes Kepler University in Linz, Austria] said there's a very strong school of computer algebra in Austria, working on - Gröbner bases is the term. He told me that they are doing everything as discrete problems, with rational numbers, and exact computation. If you do it on a computer you get a blow-up of digits, and these things of course you feel should be possible to do in floating point. And he said well sometimes it works, sometimes it doesn't at all. And couldn't I try to look into that? And I started to do that, and I was fascinated from the beginning. Particularly I was fascinated by the fact that there was a wide continent between numerical analysis and computer algebra, namely, numerical polynomial algebra, which had not, virtually not been touched at all. For historical reasons it is clear. When computers became able to do not just numerical work but data work as well, data processing in the mathematical sense, work with formulas and so on, a group of algebraists found that this was a niche where they could get money so they invented computer algebra. And computer algebra was very highly supported by governments because you could promote it very nicely. Although the people who did get [funded] were almost exclusively pure mathematicians. They were really interested in purely mathematical problems not with applications to real life. And so the development of computer algebra had remained an internal mathematical development. Although it would have been very fruitful if applied. But here I'm just thinking in particular of the polynomial part of computer algebra. This is dealing with polynomial systems of equations which are somehow the simplest form of nonlinear equations. The fascinating thing about polynomial systems of equations is that you can treat them globally; most nonlinear systems you can only look at locally. So this was a fascinating subject, but, while I had done numerical analysis, I had no time for that and thought that it was occupied by others. But now I saw that something might come from looking at computer algebra.

(END OF TAPE SIDE A)

(SIDE B: continues)

<u>STETTER</u>

I systematically went to conferences of computer algebra. These people are very highly trained in algebra, abstract algebra, but not really in linear algebra, particularly not in algorithmic linear algebra. Linear algebra can also be abstract. And with my knowledge and very strong relationship to numerical linear algebra I saw many things from a different aspect than they could. There is the basic problem of finding the zeroes of a polynomial system of equations. And you can deal with this algebraically in two ways. You can define the polynomial ideal and try to represent that ideal by a basis, like the Gröbner basis which was their big computational achievement. From this, you *may* have an easier way of getting the zeroes. And the other way is to look at the quotient ring, which if you have a finite number of zeroes is a finite linear space with a multiplicative structure. And this multiplicative structure, because multiplication is a linear process with respect to a fixed basis of a quotient ring is given by matrices, by numerical matrices. I saw all of a sudden, after some effort, that the eigenvectors of these matrices contained explicitly the zeroes that you want. They must do so. Eigenvectors can of course be computed numerically. If you get only approximate values, you don't care so much [because] you can improve them by Newton's method. This of course is something out of the question for algebraists, because something which is not exact doesn't exist [for them]. But, of course, this was the [proper] way to put it into numerical form. This paper appeared in a very odd location due to the fact that it was presented as an odd conference in '88 [10] and it's now considered the seminal paper for [the numerical treatment of] polynomial systems of equations. But I must say that what gave me the idea of thinking in terms of eigenvectors was not only my strong relationship to numerical linear algebra, but also Oskar Perron, the famous mathematician of the first half of the last century, who was also one of my teachers at the University of Munich. He wrote one of the textbooks on algebra [11], at that time. Before he used the very classical forms, he talked about these things in a way that didn't mention eigenvectors as such, but which resembled the way that in linear algebra they talk about them. And so I got the idea that there must be some kind of connection, and then when I saw the quotient ring structure, and so on, I found that they

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[10] Auzinger, W., Stetter, H. (1988). An elimination algorithm for the computation of all zeros of a system of multivariate polynomial equations. In Numerical Mathematics, Proceedings of the International Conference, Singapore 1988, volume 86 of Int. Ser. Numer. Math., pp. 11-30.

[11] Oskar Perron, Algebra. Vol. I, Die Grundlagen. Vol. II. Theorie der algebraischen Gleichungen, Berlin and Leipzig, Walter de Gruyter, 1927

could do it.  Oskar Perron was an old classical mathematician and he liked to use in his classes "epsilontics".

DAVIS

He was big on epsilons?

STETTER

Yes, and he was one who would say epsilon half here, epsilon quarter here, epsilon the quarter here, so that in the end he would have one epsilon, and he had all his notes on used envelopes, written in a very tiny writing.  He was 70 at the time. He was an extremely convincing mathematician and his way of thinking was algorithmic, as with many classical mathematicians. And I think this also made me not become a pure mathematician.

DAVIS

Do you have opinions about, for example, the Bourbaki movement?

STETTER

Well, this happened when I was a very young scientist, then at the Technical University [of Munich]. Of course we looked at these books; we tried to teach them to each other. Somebody would read some part and put them before a few colleagues and would explain it. But we gave up after a while, and I think rightly so.  It was a very important attempt, I think.

DAVIS

It was an attempt of unification in a way.

STETTER

Yes, yes, but the goal was too large.

DAVIS

These ideas in polynomial algebra, do they extend to polynomials in the matrix variable?

STETTER

I would believe that they do. There is nothing about that in my book. These are polynomials over the complex numbers in order to avoid the difficulties with the real numbers. But the complex numbers is a closed field, so all the zeroes will also be complex numbers, and one does not go beyond that ever.

DAVIS

You know that I've been asked to give a talk on Thursday - (knock, is someone at the door?)- and I'm going to be talking about the general question how do we know when a problem is solved. Now this is a question to which everybody has an answer. Everybody that I've run into has an answer to this, from little children all the way up.

STETTER

This is true, when I read your title I thought well this is really a difficult question.

DAVIS

What is your initial reaction to this question, from your experience of course?

STETTER

While you are solving a problem I think you can never tell. You hopefully will get an answer, but I have experienced so often and in particular in my work in numerical nonlinear algebra, that you think that you have solved a problem and then years later you, mostly in preparing a presentation and trying to make it understandable, discover that you really should have approached the problem from a different angle and that this solves the problem much better and fuller than you did it before. If I may, I can give one example of that. When I started to give papers at the conferences of algebraists, of course I was an outsider, they didn't really take me seriously. They didn't know of my achievements. They saw that I was a mature mathematician, but most of the people didn't know anything about my previous work, so I was just a newcomer. There were some very serious algebraists who said well numerical basis representation of polynomial ideals that *cannot* work, that's simply out of the question. At that time, I thought, well, they don't think of approximation and they don't understand that you can iteratively improve things and so on. And only when I was writing the book [12] and spreading out in a textbook form different ways of looking at things, I found why they were right. The reason is that you clearly cannot have approximate basis representations [in general] because almost invariably the bases are over-determined. There are more polynomials in the basis than the dimension of the space, so it must be over-determined. And this means that the polynomials in that basis must satisfy relations some *exactly* otherwise the system is inconsistent. And somehow they felt that if you have approximate coefficients in the basis, they will never satisfy the relations, of course, and so what you have is an inconsistent basis and this doesn't tell you anything. I am sure that the good ones didn't just push it away because they thought well what is that, but this must have been in their mind but nobody could tell me it. I often asked them why it cannot work, and they said many things. But this is the very simple reason, and so what you must do is really understand these relations numerically, and show that you can work with them, that you can use them to put it right.

[12] Hans J. Stetter, Numerical Polynomial Algebra, Society for Industrial and Applied Mathematics, Philadelphia, (2004)

DAVIS

You can deal with the inconsistencies.

STETTER

Right, you can deal with them all the time, with over-determined systems. But of course, here you have to do it in the right way and that's it.  So this shows that solving a problem may be a long way in coming, and we may need to change [our approach] while we are working on it.

DAVIS

In the areas in which you are interested, what unsolved problems do you see for the future?

STETTER

Well, there is one unsolved problem which will never be solved but only achieved better and better, is to have [automatic] evaluation for general types of problems. So that you construct a code which you tell all kinds of things about your problem and then give it the data, and so on, and which will then be able to work on that problem well in the numerical sense, especially if it is an involved problem, not just straightforward one.

DAVIS

This is a kind of a dream.

STETTER

Yes.  This has been a dream I think all through my career because there was also an interest in my work in assessment of numerical software. I have been a member of the IFIP Working Group on numerical software from its beginning, in '74, until I dropped out in the late 90s. But through that work in that Working Group, I was constantly confronted with these questions and my attitude was we should at least try to find evaluation criteria for numerical software, mathematical evaluation criteria. I elaborated some of those in ODE, initial value problem ODE software. A major assessment criterion that I created and designed is tolerance proportionality. That is you can expect that if you set the tolerance in order of magnitude, then the error will be appropriately well-behaved, which is normally not at all the case. Only over very narrow stretches of tolerance do you normally have this tolerance proportionality. But to understand what are the properties of the algorithm and its implementation that influence tolerance proportion, this was also for a while a subject that I felt was important.

DAVIS

Well coming from this point of view how do you view packages? Very popular packages such as MATLAB?

STETTER

Well, I think of them as wonderful tools because they make it possible for thousands or even millions of applied mathematicians to use good numerical software to solve their problems. With the present speed and storage power of computers, it doesn't matter whether it's done optimally or not. All you do, if there are restrictions, is put your tolerances down to smaller values, and even if it's not the ideal algorithm for your problem you get decent answers. If you use a little bit of experimentation to see how many digits you have really gotten, then except if you have problems for which the software really is not designed, you get good answers.

DAVIS

But of course in packages like MATLAB you don't automatically get any statement about the tolerance, the error of the output.

STETTER

That's right.

DAVIS

Is that a limitation?

STETTER

This brings me to another problem which is only partially solved in any subject, even in numerical linear algebra where one has very strong insight. Namely, if I have data of limited accuracy, which is the normal case in applied mathematics, what is a meaningful answer to the problem? How many digits are meaningful given the accuracy of the data. A considerable point of that book [see reference 12] is an attempt to answer this question for polynomials. In polynomial problems, it is far more difficult than for linear problems.

DAVIS

But in linear problems we're now dealing with the matrices like a thousand by a thousand.

STETTER

I know, there it's the size that is the problem. Here [with polynomials], of course, we are still thinking of relatively small systems because otherwise the computational work is very excessive. But I think it is something which is also not sufficiently realized, and this goes back to my very early days in numerical computation. As soon as computers became more powerful in many publications, I would say in most publications, when they gave results of experimentation, they gave precisely the digits that the computer output and it was normally ten, ten or twelve digits, without any indication of where the meaning of that number stopped. I think a very important part of responsible numerical work consists in telling where the meaning of an output number stops because after that it’s chance.  A concept that I introduced here and that I have introduced much earlier in differential equations is equivalent data, sets of equivalent data. If I have only two or three digits after the [decimal] point, then every value which rounds to these digits is only equivalent, as a datum. I don’t know how it will continue, and this corresponds to equivalent results.  So, for a result of a computation with data of limited accuracy, you can either think of it as a set, but I’d rather think of it, well I think of it in a way as sets, but to try to compute sets is meaningless.

DAVIS

This is a kind of an interval analysis?

STETTER

Yes, I will come to that in a minute.  I think of a result rather as *valid*, more or less valid. If it can be interpreted as arising from data in the valid data set then I consider it as a valid result.

DAVIS

This is sort of like Wilkinson’s backward error analysis.

STETTER

Yes, backward analysis, essentially. Only in the nonlinear case, particularly if you have singular problems. That’s another aspect here; most interesting problems in polynomials are in some ways singular. So it’s not so easy to do the backward analysis. This is another thing that I tried to treat in my book. I think interval analysis for anything but linear problems is inconsequential, because it has so many circles. I have also been associated with the interval methods group in Karlsruhe, because they were my friends, some of them came from Munich. For a while, I thought that this would be the way to answer the question that I just posed, of valid results. But when I became more familiar with it, and in particular when I tried to apply it to differential equations, I found that for any realistic problems it was simply impossible. And in polynomial problems if you have nonlinearities it's impossible from the beginning to create it, because you run into the singularities. So, [in my book] I have remarks about interval analysis that devalue it. I hope that my friends at Karlsruhe are not too much annoyed.

DAVIS

I'm going to raise one final question because I am getting tired, I don't whether you are getting tired. Let us suppose that you are teaching and a young person says to you that he would like to do some doctoral work in numerical methods, and so on. What knowledge or training, what courses are now important for that person to know to come into the field of numerical work?

STETTER

Well generally speaking, before aiming at a particular area in numerical work, I would say you should have a good classical training in mathematics, and I would say training like it was done fifty years ago. I don't know how it is in American Universities, although I suspect that it is like in our Universities, from the beginning there is too much abstraction. The young students play this game, for them it's a game, they are very good with abstractions, very soon, but they don't know what it is good for, what they are really dealing with.

DAVIS

But in connection with your polynomial algebra you had to learn some abstract algebra –

STETTER

I had to learn some commutative algebra, but I did that only as far as I needed it, as I was going along, in a sense. I found that what I really needed was, again, only the classical part of that subject and none of what is now considered the real jist of that subject.

DAVIS

You would recommend classical analysis, complex variables.

STETTER

In particular linear algebra, but done not in an abstract way. Very often, even in our technical university, linear algebra is taught as an abstract mathematical subject. When the students come into the numerical course, we soon see that they have no idea what they are talking about because they don't see spaces and geometry. I think geometry, the way of looking at a subject geometrically and seeing the n [dimensional] spaces as three [dimensional] space is extremely important for numerical work. In order to conceive of numerical methods, I think you must somehow see the problem as a realistic one, not as an abstract problem. If it's in n dimensions you can do so only if you see it in three dimensions. Of course, it is not clear whether what you are seeing in three dimensions is applicable to n dimensions, but very often the ideas which come from seeing it in three dimensions will somehow generalize.

DAVIS

So, are you recommending an excellent experience, geometrical experience, to build up geometrical intuition.

STETTER

Right. I think this is an important part from numerical work, and so is practical work, solving non trivial work problems originating in some applied environment to make sure that it's not a problem where you assume that everything is exact, but you are in tune with the problem. This I think is important, and some love for theory.

DAVIS

Well thank you very much. I think we've had a very fine session.

STETTER

Yes, I enjoyed it.