An interview with Hans Sartorius

Journal Item

An Interview with Hans Sartorius

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This article highlights the classical control textbook *Dynamics of Automatic Control,* which was originally written in German and subsequently translated into English and several other languages. The topic was motivated by the article "What Is Your Favorite Book on Classical Control?," which appeared in the special section on Classical Control Revisited in the June 2007 issue of *IEEE Control Systems Magazine. IEEE Control Systems Magazine* welcomes similar articles that highlight the development of systems and control technology worldwide.

In 1994, prompted by the 50th anniversary of the publication of *Dynamics of Automatic Controls* by the German engineers Rudolf Oldenbourg and Hans Sartorius, Chris Bissell visited the surviving author Hans Sartorius at his home in Karlsruhe. This article is an edited English version of their conversation in German. Prof. Sartorius died in 2005.

Q. Prof. Sartorius, how did it happen that control engineering became your main professional interest?

Hans: It was quite by chance. I studied at Munich Technical University, beginning with mathematics, but also taking courses in the theory of electrical engineering. I've always been interested in mathematical modeling, particularly in electrical engineering. After graduating in 1937 I went to Berlin, to join Siemens, something not at all unusual for a young electrical engineer at that time. I got into control engineering in a rather curious way. When I was interviewed, they asked me if I smoked: cigarette smokers were taken on, but not cigar or pipe smokers! I used to smoke cigarettes at the time, so was accepted and sent to the Instrumentation Laboratory. There I was asked what my main interests were, and as soon as I answered "mathematics and the theory of electrical engineering" I got the answer "I can't stand theory, there's no place for you here." So I ended up in a new control engineering laboratory, run by Dr. R.C. Oldenbourg. We discussed my interests, and I have to confess I'd never heard the term "control engineering" before. But we got on famously, we'd both studied in Munich, and I'd done rather better than him at electrical engineering. "I can certainly use you," he said, and that's how we started our collaboration.

Q. What sort of controllers and control systems existed at that time?

Hans: Mostly for process control in the chemical industry, oil refineries, steam generation, and so on. We were concerned mainly with thermal applications. The temperature sensors produced only a weak voltage output, which was sampled using a chopper-bar galvanometer. That's why we had to deal initially with discrete control systems. Later we also employed continuous (pneumatic) controllers for applications in the chemical industry. One of my jobs was to develop pneumatic controllers.

Q. When did you begin to model such systems mathematically?

Hans: The general principle of closed-loop control interested me greatly. But not many people had investigated such systems mathematically or theoretically. The pioneers that I knew about were Stodola, Tolle, and Wünsch—who wrote a book in 1930 on controllers—and there was someone in Switzerland named Stein (see "Early Pioneers").

Early Pioneers

In the 1890s, A.B. Stodola of the Zürich Polytechnic modeled the control of hydraulic turbines. He was unable to derive a general stability criterion for linear systems and asked a colleague, A. Hurwitz, for help. The latter produced his version of the Routh-Hurwitz criterion, unaware of the work of Routh in England 20 years before. At the beginning of the 20th century M. Tolle wrote the textbook *Control of Prime Movers,* which was highly influential in spreading ideas about control in German-speaking areas of Europe. In Switzerland, T. Stein extended Stodola's approach in the 1920s. At the same time the German Karl Küpfmüller was developing a time-domain stability criterion that could more easily be applied to higher order systems. The real breakthrough, the Nyquist criterion of 1932 and related frequency response methods, filtered through from electronics into control engineering. One of the earliest German publications to promulgate this American work was A. Leonhard's *Automatic Control in Electrical Engineering* of 1940.

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I addressed the problem using differential equations. But then at the end of the 1930s two extremely important publications appeared in Germany: Wagner's *Operational Calculus* and Doetsch's *The Laplace Transformation.* The way to frequency domain methods was clear. This would have been between 1937 and 1940 or thereabouts.

Q. And this led to your own book, *Dynamics of Automatic Controls*?

Hans: Yes. We were mainly interested in process engineering, although servosystems were also highly relevant. But because chopper-bar systems were so important for us, we looked at sampled-data control in depth. And I had a bit of a problem with the mathematics. At that time the z-transform had not been invented. So I used difference equations and applied the Hurwitz stability criterion by means of a bilinear transformation in the complex plane. We then wrote our book, in which we gave a general overview of the fundamentals of control engineering as we understood it at the time. It wasn't easy, during the war. Oldenbourg's family owned the Oldenbourg publishing house in Munich, so they undertook the publication. But it's a miracle that the book was ever published. It had already been typeset when a bombing raid destroyed the printing works—the type, the manuscript, everything! Fortunately, Oldenbourg had taken a set of proofs home with him, and the first edition of 5000 copies was produced photomechanically from these.

Q. The book seems to me to be particularly interesting for a number of reasons. It was one of the earliest classical control texts, it treated sampled-data systems, it used the Laplace transform, and it even ventured into notions of optimal control.

Hans: After the war a group of American officers came to Siemens, to see what we had been up to during the hostilities. Naturally, they were taken to the directors, but they said they wanted to meet Oldenbourg and Sartorius, since they were interested in control engineering! They showed us an English translation of our book, which was later also translated into French, Russian, and Japanese. So we became rather well known!

Q. You've already mentioned some of the early pioneers of control engineering. Were you aware at that time of the work of some others in the 1920s and 1930s, such as Nyquist in America as well as Küpfmüller and Leonhard in Germany?

[Editor's note: See "Karl Küpfmüller, 1928: An early time-domain, closed-loop stability criterion," *IEEE Control Systems Magazine*, vol. 26, pp. 115–116, June 2006.]

Hans: Nyquist's work was known before the war, although not often applied. We mostly used the Hurwitz criterion for stability assessment. We heard of Küpfmüller's work fairly late. Our book was almost finished when we came upon his papers from the 1920s. Küpfmüller analyzed the closed-loop system not in the frequency domain, but in the time domain, using integral equations. In our book we extended this approach by applying the Laplace transformation. Later we made contact with Küpfmüller and had a very pleasant relationship—he was an extraordinarily intelligent man. As far as Leonhard is concerned, I think he was working more or less at the same time as us, but we only got to know him later. I was called up for military service, but the high command decided that technical people would better serve the war effort in R&D, so I was discharged and went to work with Leonhard in Stuttgart. I got my doctorate there, and worked on torpedo control.

Q. Was there much contact in Germany during World War II between the various groups working on control problems? In the United States there was the National Defense Research Committee and in England the Servo-Panel. Was there anything similar in Germany?

Hans: Not in the control field. There was a VDI [German Engineers Association] standards committee, which provided an opportunity for control specialists to meet and maintain a certain contact. But there was no central organization by the state—all the various groups were independent, although now and again publications circulated, as you might expect.

Q. We've already mentioned your work on sampled-data systems. Did you develop your approach completely independently?

Hans: Yes. We were both very proud that our book had analyzed sampled-data systems (and optimal control) for the first time, and that we'd given a stability criterion for discrete systems. In 1951 we were invited to the Cranfield Conference (see "The Cranfield Conference"), where I gave a paper on the stability assessment of sampled-data systems. After I had proudly described our method, a delegate stood up, congratulated me, and then sketched out the z-transform on the blackboard. It was a difficult experience to see that it could be done so much better! But on the whole, when

The Cranfield Conference

The Cranfield Conference, held in July 1951 at the College of Aeronautics (now Cranfield University), was, possibly, the first real international conference on control engineering. It was organized by the British Department of Scientific and Industrial Research together with the Institution of Electrical Engineers and the Institution of Mechanical Engineers. Participants from outside the United Kingdom included G.S. Brown, A. Leonhard, N. Minorsky, R.C. Oldenbourg, W. Oppelt, H. Sartorius, and B. van der Pol. Almost half of the 33 papers concerned nonlinear or sampled-data control.

we got to know the work from America and elsewhere just after the war, we were amazed at how similarly control ideas had developed in various countries.

Q. One of the most important aspects of classical control is systems thinking. When did that notion emerge in Germany?

Hans: When I came to the control field, every sector operated differently. There were procedures for process control, thermal engineering, and torpedo control, but they bore no relationship to each other. You couldn't talk about systems thinking. But we recognized the commonality and tried to bring everything together, using a common language and common symbols. Perhaps that's the significant thing about our book, that it made people recognize that commonality, and that they could work together. Control engineering as a subject area got a great boost from the Cranfield Conference. All the control engineers got together and for the first time got to know each other. And we Germans decided to organize a similar conference—not only for participants from the West, but also for Russians, Poles, Chinese, and Japanese. We were amazed by the response to our invitation to the 1956 Heidelberg Conference. It was a global occasion, and as a consequence the International Federation of Automatic Control (IFAC) was founded. The first IFAC conference was held four years later in Moscow.

Q. Can we turn now to another aspect of your professional career, your role in industry alongside your academic positions in the 1950s and 1960s?

Hans: During the war our Berlin premises were completely destroyed. We found ourselves in Erlangen with nothing to do! My parents owned a nearby flour mill. By that time my father had died, and I took over the family business, at the same time doing some work with Siemens. When the Karlsruhe plant was rebuilt, I returned there. I had the desire to become an academic, a university professor, but my boss in Berlin suggested that I do both— accept an academic position, but continue to contribute to Siemens R&D. That was not so easy to arrange! But in the end I got a teaching position in Hannover, where I taught three days a fortnight, while also directing control R&D at Siemens. At that time there was no chair of control engineering at Hannover, so one was created. The first control professor in Germany was Oppelt, I believe, in Darmstadt, and Thoma succeeded me in Hannover. Unlike America, as I later learned, it was most unusual in Germany to combine academic and industrial work.

At Siemens in Karlsruhe I worked first in R&D but later took on the management of the whole process control operation. As time went by, we naturally introduced computers for process control and automation. I remember our first attempt at computer control for an oil refinery. Initially we tried to use a single large computer for the entire refinery. Clearly that approach was impossible, since a computer failure could then compromise the safety of the whole plant, so we speedily went over to small units. It was a fascinating period, a time when real pioneering work could be carried out.

Q. Many thanks, Professor, for this fascinating conversation. You were certainly one of the true pioneers of control engineering.

AUTHOR INFORMATION

Chris Bissell is professor of telematics at the Open University, U.K., where he has studied the history of control engineering. He is a frequent contributor to *IEEE Control Systems Magazine*. His most recent article is "The Moniac: A Hydromechanical Analog Computer of the 1950s," which appeared in the February 2007 issue. Further information about the early controls work mentioned by Hans Sartorius in this interview can be found in historical articles on the Web site http://technology.open.ac.uk/tel/people/bissell/.

I have almost always felt fortunate to have been able to do research in a mathematics environment. The average competence level is high, there is a rich history, the subject is stable. All these factors are conducive for science. At the same time, I was never able to feel unequivocally part of the mathematics culture, where, it seems to me, too much value is put on *difficulty* as a virtue in itself. My appreciation for mathematics has more to do with its clarity of thought, its potential of sharply articulating ideas, its virtues as an unambiguous language. I am more inclined to treasure the beauty and importance of Shannon's ideas on errorless communication, algorithms as the Kalman filter or the FFT, constructs as wavelets and public key cryptography, than the heroics and virtuosity surrounding the four-color problem, Fermat's last theorem, or the Poincare and Riemann conjectures.

—J.C. Willems, "In Control, Almost from the Beginning Until the Day After Tomorrow," *European Journal of Control*, vol. 13, p. 75, 2007.