A Conversation with Friedrich Götze

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Friedrich Götze
Photo kindly provided by Friedrich Götze (2011)

Abstract Friedrich Götze has made signal contrbutions to mathematical statistics, probability theory and related areas in mathematics. He also rendered many other important services to the profession. His 60th birthday provides and excellent opportunity for a conversation about his career and his views on various matters.

1 Early Days: A Talented Tinkerer

Interviewer: Friedrich, let us start at the beginning. You were born in 1951 in Hameln and as a boy you must have shown great promise as a scientist. Can you tell us about your scientific activities while you were in school?

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F.G.: Well, I grew up in a household where my father was a small grocer, and of course his perspective for my future was taking over his grocery. After finishing elementary school I wanted to go to the gymnasium. My father had something more practical in mind, but the teacher convinced him that the gymnasium would be a better choice. From there on, I did not show much interest in the grocery store, but rather in the libraries of our town. After a while I had all kinds of interests, especially in soldering together radios and some electronics that I was fascinated with at that time.

Interviewer: We have a beautiful picture of you from those days. It appeared in the local newspaper and shows you and a friend with some fantastic looking equipment you put together.

Friedrich and his friend Friedrich Hupe with their computer.
The photo was published in the local newspaper of Hameln: DeWeZet

F.G.: Yes, there was the centennial celebration of the school, and on this occasion each of the students should carry out a project, for instance some chemical or physics experiment. I was fascinated by computers, which were not available for the general public at the time, and I thought I would make a demonstration computer. And because the necessary components were very expensive at that time, I went to some of the so-called scrap-shops of the telecom companies where they were throwing out their switchboards made of electromagnetic relays. Huge numbers, which you could buy for a few Marks, just for the weight of them. I collected them and then we soldered them together to make accumulators and for doing some basic binary addition, and even multiplication which was a great thing. To wire these things up to have something like a main switchboard that would do these things and display the result, took us more than a year and I still remember that it was very difficult to keep all of these wirings in mind. I had a pal who was better at soldering than I was, and he did the soldering I told him to do. So we finally made it and had this thing displayed at the centennial celebration of our school.

Interviewer: It could actually multiply?

F.G.: Yes, it could actually multiply, but it took a while before you could see what had happened. It was very slow.

Interviewer: So we should consider ourselves lucky that you didn't go into electrical engineering or computer science.

F.G.: Of course computer science was something interesting for young people. But what you heard about it was not happening in Europe. It was happening in the United States. The first things we heard of—and were very fond of—where these programmable pocket calculators. But they were extremely expensive and cost about a thousand Marks, which was about half of the salary of an assistant. Also, it all happened in America, with Fairchild and later with Intel. To get into computer science you would have to go to the US.

Interviewer: Let me return for a moment to your father. Was he ultimately convinced that an academic career would suit you better than minding the store?

F.G.: Yes, sooner or later he recognized that the store was not my cup of tea and as my grades in school got better and better, he said okay, if that is your future . . .

2 A Wander-Student

Interviewer: So you went to the gymnasium and obviously finished there, and then went to Göttingen in 1970 to study physics and mathematics. There was probably very little computer soldering going on there, so you must have prepared yourself in some other way.

F.G.: Yes, I'm afraid that my computer would not have got me very far in Göttingen. I already mentioned my interest in libraries that would lend me physics and mathematics books for 2 or 3 weeks. During that time I copied what I thought was important in little notebooks. There were no copying machines in those days, so it all had to be done by hand.

Interviewer: So in fact you constructed your own private library. It is certainly a good way to learn. It takes a lot of time but you never forget these things anymore.

F.G.: That is right. Even though the lectures at Göttingen were more rigorous, I think I was quite well prepared.

Interviewer: So at Göttingen you basically studied physics with a second major in mathematics.

F.G.: With my kind of interests I might have become an electrical engineer, but I was not sure and felt that a general physics and mathematics education could not do any harm. Also computer science was still part of mathematics at that time. Finally, Göttingen was only 80 km from where I lived.

Interviewer: During your 2 years at Göttingen you switched from physics to mathematics as a first major, and passed your pre-diploma exam in mathematics. What had happened?

F.G.: Well, I passed through the usual physics and mathematics curricula. The lectures were all right, but in physics there were also these dreadful experimental sessions. I had expected to see wonderful new laboratory equipment, but instead we merely worked with traditional old instruments dating back to the 1920s. They really belonged in a science museum. This may well be useful to develop your skills with basic measurement devices, to trim them to higher precision and learn physics that way, but it was not very exciting.

Interviewer: I also moved from physics to mathematics after similar experiences. However, that was much earlier and you might have expected these lab classes would have been modernized a bit in the meantime.

F.G.: Well, the 1920s were the great days of Göttingen physics and, as generations of students before us, we were supposed to learn by using the same marvellous old instruments to repeat the experiments of those days, the outcomes of which you should of course know from your courses.

Interviewer: Another thing that turned me off was the way in which mathematics was handled in some physics courses. Our mathematics teachers taught us to be rigorous, but that didn't seem to hold for experimental physicists.

F.G.: I took lectures in quantum mechanics and I started pestering the teacher afterwards asking what the meaning of this measurement process was, because I had heard there was a debate among physicists about what they were describing by this. And then I got this nice reply: "Young man, first try to learn the trade and do your exercises. Leave this type of question to the time when you get a Nobel Prize and then you can do philosophy".

Interviewer: So much for physics and physicists. Anything remarkable about the mathematics courses?

F.G.: Well, there was certainly no lack of rigor there! Our first calculus course was taught by Brieskorn who had his first position as a full professor in Göttingen and later became a well-known geometer. He started his calculus course by teaching logic first, and we were trained so thoroughly that it took us three or four semesters before we could actually write our proofs in normal mathematical style again without using seven or eight quantors.

Interviewer: You were treated the rough way!

F.G.: Yes, but it was good training.

Interviewer: Having survived all of this, you did get your pre-diploma in mathematics at Göttingen in 1972.

F.G.: Yes, I did, even though there was a slight problem. I had completely forgotten that linear algebra was also part of the exam, and I had done nothing to prepare

myself for this. I discovered this shortly before the exam and literally worked day and night to catch up. As a result I overslept on the day of the linear algebra part of the exam. It was scheduled at 9 a.m. and I showed up at noon. Luckily they still allowed me to take the exam.

Interviewer: But then you left Göttingen and went to Bonn to continue your mathematics study. Why?

F.G.: I had a stipend from the Studienstiftung, a foundation supporting kids that did well at school. Among other things they held nice meetings in the semester break, organized by professors in various disciplines who were interested in young people and, of course, were looking for talent. I attended one of those seminars in Alpach in the Austrian Alps that was organized by Professor Hirzebruch from Bonn. He impressed me by the way he could explain essentially complicated matters in a simple way. It is rather common in Germany to change universities after your pre-diploma, so I decided to go to Bonn and study complex geometry and topology with Hirzebruch. I got my diploma under his guidance in 1975.

Hirzebruch had an interesting style. There was the Wednesday afternoon seminar. Everybody in the geometry group was supposed to be there. If you missed a seminar, he would say the next day: "I didn't see you yesterday". Then you knew that you'd better be there next week.

Interviewer: Sounds like Jerzy Neyman. Any other interesting characters in Bonn at the time?

F.G.: Definitely. There was Don Zagier. He was about my age and in those days usually dressed in a formal suit. We thought this a bit strange, but we took into account that he finished high school at age 13, received his master's degree at M.I.T. at age 16, and his Ph.D. with Hirzebruch in Bonn at age 20. He was about to finish his Habilitation when we were attending his lectures. He had clearly been a child prodigy, but without the difficult characteristics that often go with this. I attended his lectures on modular forms and was tremendously impressed by the speed at which he could do calculations on the blackboard. We used to say that he would be the only guy who could go shopping at a shopping centre for 2 weeks of supplies, and by looking at the numbers the cashier pushed, would know the grand total before she did. But he was also very practical and owned one of the first computers to check up on his number theory, and he was also interested in applied matters.

Interviewer: It is interesting to hear you say this. Many years ago I gave a lecture in Bonn on a topic in probability theory which most of the pure mathematicians present clearly considered a waste of their precious time. Afterwards Zagier took me out to a very pleasant dinner where we had a very sensible discussion of some probability problems. So apparently pure and applied mathematicians can get on quite well, but we have to realize that Don and you and I may not be the prototypical pure and applied types!

F.G.: At that time I also met another interesting person. For the work I did for my diploma I had to read an original paper in Russian and took a Russian course

provided by the university. However, like many older papers, this one was written in a verbal and descriptive style for which you need a better command of the language than my elementary course had provided. More important, during the course I met my future wife Irene. After I got my diploma in 1975, we got married in 1976.

Interviewer: After obtaining your diploma you were thinking of getting a Ph.D. with Hirzebruch in Bonn.

F.G.: It was not so clear what to do. Hirzebruch was quite pleased with my diploma thesis. However, it was also becoming clear that the expansion of the university system in Germany was coming to a sudden end. The oil crisis had frightened people, in particular the politicians who were no longer willing to finance further expansion. I was thinking of having a family and it was becoming increasingly doubtful that getting a Ph.D. in geometry would provide a stable basis for a family income. So I thought that maybe something more practical would be better. I looked around for advice and was told that a diploma is fine, but something more applied would be even better for getting a job in industry or an insurance company. Also, if you really want to have a decent career in industry in Germany it is an advantage to have a Ph.D.

Interviewer: It seems to me that you ended up by getting the best of both worlds. You did get a Ph.D. in an applied field and you did not end up in an insurance company. So off to Pfanzagl in Köln because it was close?

F.G.: Not so fast. My wife was studying medicine in Bonn and she thought I could perhaps be a medical doctor. This went as far as her taking me once to a dissection course in anatomy. She thought it quite interesting and very fascinating. But when I came into this large hall where a lot of students were around and people were opening up skulls, my lunch was protesting and I had to leave immediately. I thought this is not my cup of tea. So far for my taking up medical studies.

Interviewer: Yes, this sounds quite drastic. I believe that even for medical students, this is a test of stamina. Fortunately, less extreme forms of medical studies also exist.

F.G.: In view of possibilities in finance and insurance companies, I thought of brushing up the knowledge I acquired in Göttingen in courses on ergodic theory and measure theory of Ulrich Krengel. I was pleasantly surprised that the institute in Göttingen offered me a tutor position to complement my weekly allowance. It also seemed to make sense for my new career plans to renew old acquaintances, so I decided to return to Göttingen for a semester. Of course Krengel was there, together with visitors like Ahlswede, an American named Lee Jones who was the life of the party, and someone from Fribourg teaching rank tests in Hájek's style.

Interviewer: Was that André Antille, by any chance?

F.G.: You are right: it was. Everyone was in a good mood, we used to sit around posing problems to each other and I actually learned some statistics. Krengel was of course doing ergodic theory and to me that looked very much like analysis, and

rather than doing that, I might as well have stayed in geometry. I also turned down an offer from Ahlswede to come to Bielefeld with him. I was interested in finding a real statistician and someone told me there was someone named Pfanzagl in Köln, close to Bonn where Irene was studying. So one day I went to Köln, it was late in the evening, I didn't really expect to find Pfanzagl there, I knocked on his office door and said: "Hello. I'm a pure mathematician coming from Bonn, I have had some lectures in probability and I want to study statistics". This must have been something that he didn't expect and he was looking at me a little doubtfully. I handed him all my certificates which didn't look too bad, I would say. He was quite nice and willing to give me a try, offered me a tutor position, and asked me to give some lectures and seminars, which I did, and so I stayed there.

Interviewer: How long did you stay there until your Ph.D.?

F.G.: That must have been from 1976 to 1978 when I did my Ph.D. there. Of course I learned a lot of things and I was also involved in Pfanzagl's projects, correcting and checking manuscripts, helping him out in seminars, etc. The work for my Ph.D. thesis I did more or less on my own. Since Edgeworth expansions were a hot topic at the time, I wrote about expansions in Banach spaces.

I consider myself fortunate that I learned statistics from Pfanzagl. He got his Ph.D. with Hlawka in Vienna in number theory, worked in the statistical office of Austria, obtained a chair in the social sciences faculty and then moved to mathematics. But he was not content to investigate the mathematical properties of standard statistical procedures and recipes. He first wanted to discuss how appropriate such a procedure was for a given application. He loved to do mathematics, but never ignored his early history in applied statistics.

Interviewer: I have the same experience with my Ph.D. advisor Jan Hemelrijk, and of course, this is what statistics is all about! Unfortunately, many of today's mathematical statisticians lack experience with genuine applications, and as a result have little feeling for the validity of their models and procedures in practice.

F.G.: Pfanzagl had organized his life in an interesting way. He was at the university 2 days a week, when he had his appointments, taught his courses, attended seminars, etc. The remainder of the week he spent at his very nice home in the countryside on the other side of the Rhine, and during the semester breaks he was in Vienna with his family. Of course as students we also liked this 2-day workweek, because it gave us lots of time for research. If I would join my wife who had to be at the clinic at 7 a.m., it was a quiet time for me too, because if you appear at a mathematics department at 7 a.m., there is nobody there.

Interviewer: Friedrich, I think we have reached the end of your days as a student. You said earlier in this interview that it is quite normal in Germany to study at two universities. Now that we have seen that you moved from Göttingen to Bonn, then back to Göttingen again and finally to Köln, you won't mind that we call this section of the interview "A wander-student".

3 To the USA and Back

Interviewer: After getting your Ph.D. in 1978 you remained with Pfanzagl in Köln until you finished your Habilitation in 1983. However, in fact you spent quite a bit of time elsewhere.

F.G.: In November 1977 I attended my first meeting in Oberwolfach. It was on asymptotic methods of statistics organized by Pfanzagl and Witting. The meeting was crucial for my further development because I met you and Peter Bickel, which was the beginning of a long friendship and collaboration in various matters. At one point Peter asked: "Why don't you come and visit us in Berkeley?". Irene and I wanted to see the US anyhow, but it had to be cheap. So in 1978 we flew Icelandic Airways and made a detour through Mexico where my wallet containing my credit card was stolen in the underground of Mexico City. Via Atlanta we flew to San Francisco. By that time our money was almost gone and we went to the Bank of America where they advised us to let our bank send them a cheque for us. We followed their advice but the cheque never showed up.

Interviewer: Well, you couldn't have known that the European and American banking systems together are unable to send ten cents across the ocean successfully.

F.G.: At that time I was still very shy and didn't want to involve Peter who would probably have helped us immediately. Finally we got the money wired in some way. We went back via Princeton were we had friends, so apart from this one mishap, our first visit to the US was very pleasant.

The next year I went back to Berkeley as a visiting assistant professor for the academic year 1980–1981. When I told Pfanzagl about this plan, he looked a bit dubious and obviously thought he'd never see me again. In Berkeley momentous things had just happened. Neyman had died, and so had Kiefer who had just come to Berkeley. When I started teaching I found this a bit different from what I was used too. In a course for engineers you obviously have to follow the book, but I sometimes tried to explain a few things on a slightly more advanced level. That didn't sit well with the students, so Betty Scott, who was the department chair at the time, told me "Young man. You have to realize that we are teaching American students. You are not in Germany".

A fabulous occurrence during my stay was the Joint Statistical Meeting of ASA and IMS in Las Vegas. Participants could get a suite in the MGM Grand hotel for very much reduced prices, because the owners figured that statisticians who always talk about coin tossing, would be fanatic gamblers. Their gambling losses would easily make up for the reduced prices. When hardly anyone turned up at the gambling tables, they obviously felt cheated.

When my year as a visitor in Berkeley ended, I was wondering whether I should perhaps stay in the United States. However, at that time there were not very many positions at my level and my stay at Berkeley had spoiled me for the rest of the US. So I decided to go back to Köln and continue my career in Germany by getting my Habilitation, which I did with Pfanzagl in 1983.

4 Bielefeld

Interviewer: After your Habilitation in November 1983 you were appointed as an associate professor at Bielefeld in March 1984. They were obviously eager for you to finish your Habilitation.

F.G.: Yes, shortly before my Habilitation I was invited to apply for this position. So I went there and gave a talk on asymptotic statistics. I don't think that the pure mathematicians had the slightest idea of what I was talking about, until I mentioned that a variant of the Cramér-von Mises statistic was associated with a nice theta function. You could almost hear a sigh of relief from the audience: at least this person has heard of theta functions! So in early 1984 I was offered the position and accepted.

You may not remember, but earlier I had applied for a job in Amsterdam. Piet Groeneboom was at the CWI in Amsterdam at the time, but he was visiting Seattle and had not applied for the job. However, while the selection procedure was underway, Piet suddenly showed up and declared an interest in the position. At that time I was still considered very theoretical and no match for Piet, who was appointed.

Interviewer: So if Piet hadn't suddenly shown up, you might have been in Amsterdam.

F.G.: Yes, I was actually thinking that maybe I should learn a bit of Dutch and bought a dictionary. Irene and I both went to Amsterdam and I gave a talk there. But this idea didn't last very long.

And then I accepted the offer from Bielefeld and Irene said: "Great! Now this uncertainty over what is going to happen is past, and we go to Bielefeld". But once we came here and she saw the university building, she looked at me and said "You don't intend to stay here very long, do you?".

Interviewer: Well, admittedly Bielefeld University is not exactly a shining example of modern architecture. But there is some truth in what the Russians used to say about the architecture of Stalin's time: "It is best to be inside these buildings, where you can't see the outside".

F.G.: Yes, and when I learned a bit more about Bielefeld and the department, I was quite pleased and felt that I had landed at a quite good place.

They didn't have these barriers between the different kinds of mathematics that they had at many of the classical universities: an institute of pure mathematics here, one for applied mathematics there, and perhaps one for stochastics somewhere else, with each group tending its own little garden. In Bielefeld they had just one mathematics department. That didn't mean that they didn't have problems with allocation of funds or the hiring of new professors, but they knew they had to talk to each other and reach a compromise.

Interviewer: Even at the same institute people often didn't talk to each other. When I was appointed in Leiden, some of my colleagues thought it was ridiculous to have a professor of statistics. What stabilized the situation was the common interest. In my case people began to realize that in 20 years they might not have any students if they only taught pure mathematics. I'm not saying that everybody liked everybody else, but they could work together.

F.G.: Another pleasant thing about Bielefeld University was that you were appointed as a professor of mathematics, rather than geometry or statistics. That meant that you could change your field of research to another area of mathematics as long as you taught your courses. It was also possible to take a double teaching load in 1 year and be completely free for doing research the next year. Finally it was a lively and scientifically excellent department.

Interviewer: I understand that the philosophy of Bielefeld was certainly favourable for applied mathematics. Please tell us something about the concept of 'Mathematization' that was so typical for Bielefeld.

F.G.: The task of founding Bielefeld University in the nineteen-seventies was carried out under the leadership of Schelsky, a single person rather than a committee. Mathematics was one of the founding departments of the university because Schelsky had the idea of 'Mathematisierung' in mind. He wanted to change the social sciences and all other fields where people worked qualitatively into quantitative sciences. For that he needed all kinds of mathematics, especially applied mathematics. But not only a mathematics department, but also some bridging institutes such as the one for mathematics and econometrics. Such an institute was not permanent and would have to be reviewed every 8 years.

Schelsky was a conservative sociologist—I don't know whether this species still exists—and at that time universities were going wild. Especially the newly founded ones like Bielefeld were very progressive, including some of the staff. Schelsky found this a little bit too much, and left.

Interviewer: I suppose the mathematics staff also contained its share of progressives.

F.G.: We had a number of people in the staff who were joining the student demonstrations. There is one story of a young, energetic and leftist professor who organized a large student demonstration and marched with the students. He had his bicycle with him and after a while he looked at his watch and said: "Oh, I'm sorry. You march on. I have something important to do".

We had a very progressive institution here where all the members including secretaries and non-scientific staff had considerable voting power in all affairs of the university. This didn't make life any easier.

Interviewer: We have had the same in the Netherlands. It wasn't too bad because in the end people were usually willing to listen.

F.G.: Yes, but an older colleague told me that once in a while they had to strike a deal. They would tell the students: "We really want to appoint so-and-so as a

professor, whether you like it or not. But the next appointment you can make." Of course this would never happen, even though there was a signed agreement which is presumably well hidden by now.

Interviewer: I always have to explain to people why I stayed at the same university throughout my career. So let me put the question to you too. Why are you still here?

F.G.: In 1987 I was offered a full professorship at Kaiserslautern which I turned down. In 1989 I turned down a full professorship at the T.U. Berlin. The day Irene and I spent in Berlin to discuss the matter happened to be November 9, 1989. In the afternoon, with a small child in a stroller, we went to the wall to see people sitting on the wall and dismantling it. Then in 1990 I accepted a full professorship in Bielefeld. Finally in 2003 I turned down a full professorship at the Humboldt University in Berlin. A complicating factor at that time was that now that our children were growing up, Irene had just been licensed in Bielefeld as a doctor in residence, which allowed her to resume her medical career. It was highly unlikely that she could be licensed in Berlin, so moving there would have been a major sacrifice on her part.

Interviewer: From what you just told me, I think there is more to it than that. You explained that in the department in Bielefeld there were no barriers between different kinds of mathematics. People were appointed as professors of mathematics rather than geometry or statistics and they could change their field if they felt like it. Well, what could be a better place for someone who had difficulty deciding whether to get a Ph.D. in geometry or statistics and actually worked in number theory later? It seems to me that Bielefeld and you were made for each other, even though the Bielefeld architecture is best forgotten.

F.G.: You may have a point there.

Interviewer: And after 1989 there were the Sonderforschungsbereiche (SFB's) in mathematics that made Bielefeld such an attractive place to be. People tell me that you played an important role there, as a person who had the interests of all of mathematics at heart.

F.G.: The first SFB in Bielefeld started in 1989 and was devoted to "Discrete structures in mathematics". It was a broad collaborative effort to combine discrete methods used in combinatorics, information theory, but also (numerical linear) algebra, number theory, topology and arithmetic algebraic geometry. Many of the younger people in the department profited from the increased possibilities of communicating with colleagues and visitors from other fields within this SFB. Personally, I started out working on asymptotic approximations in mathematical statistics, but slowly moved in the direction of more discrete objects in stochastic algorithms and number theory related to this.

However, some of the senior members of the department who enjoyed a great reputation in a particular area of the SFB were more interested in seeing their leadership for the whole project acknowledged than in investing time and resources into collaborations with others. I served as chairman of the SFB for a number of years, and it was not always easy to balance the various views and keep the peace.

After the end of this SFB in 2000 there was an interim phase when the Bielefeld department went through a considerable generation change. My own interests in between pure and applied mathematics found a home in a subsequent smaller collaborative grant from the DFG, followed in 2005 by the current SFB on "Spectral structures and topological methods in mathematics". The explicitly stated aim of this SFB is to study developments connecting these rather diverging classical areas of mathematical research. Thanks to the efforts of all of the senior and junior colleagues who joined me on this adventurous road through largely uncharted terrain, this collaborative program has been quite successful in two 4-year periods so far. In the last 3 years the department has also been successful in hiring new younger staff members, who are eager to accompany us on the path we have chosen. Perhaps mathematicians are becoming more adventurous.

Interviewer: Well Friedrich, it looks like you are in Bielefeld to stay.

5 Oberwolfach

Interviewer: Friedrich, you mentioned earlier that we first met in Oberwolfach in 1977 and I would almost say: "Where else?". We have both spent a significant time of our lives there, but you have also been involved in its organization. So let's talk about the Mathematisches Forschungsinstitut Oberwolfach.

F.G.: I was appointed to the Beirat around 1990 and stayed there for nearly a decade. Then I became a member of the Executive Board of Oberwolfach, which I still am today. So I can speak about Oberwolfach during the last 20 years.

The present institute was built in the late sixties and the seventies. In a very courageous move, director Barner started the building procedure. Before the funding contract with the VW foundation had actually appeared in writing, he already ordered the construction companies to start and the bulldozers arrived. Well that was the way you did business in those days. You could count on oral agreements without fear of sudden budget cuts. So in 1968 they first constructed the building with the rooms for participants, kitchen, dining room, wine cellar and office, together with the bungalows. Then in the seventies they tore down the old villa and replaced it with the new building with lecture rooms, library, etc. Of course this was a great improvement, but at the time many people were sad to see the villa go. The institute had started there three decades earlier, and it had become a symbol for Oberwolfach.

The next 20 years passed without major problems. The State of Baden-Württemberg had no financial problems and Barner's relationship with the State administration was excellent. However, there was one inconsistency in the financial set-up. The VW Foundation paid for erecting the buildings, but not for their maintenance, and the State paid for the operational cost, also excluding maintenance. So Barner agreed informally with the authorities to save a bit of the operational cost to pay for maintenance. Again, such agreements were quite common in those days.

However, times changed. In the nineties the financial situation of Baden-Württemberg deteriorated, budget cuts became necessary, and of course Oberwolfach suffered. Perhaps even more important was the generation change in the State government. The old guard with whom Barner had such excellent relations went out, and a new generation arrived that was more interested in budget cutting than in longstanding relations. They insisted on new State rules of accounting superseding the previous arrangement that allowed Oberwolfach to save funds for maintenance.

Interviewer: Of course this is the risk of being funded by a single organization. I seem to remember that repeated efforts were made to be funded by the Federal Government too. Wasn't there a list of institutes funded by both the State and the Federal Government, the so-called blue list. Did Oberwolfach ever get on this list?

F.G.: Barner's successor Matthias Kreck (1994–2002) started to increase the annual budget to a level necessary for running Oberwolfach by obtaining support from individuals, industry and the European Union. Efforts were also made to get Oberwolfach on the blue list. The role of the blue list itself had changed with time. It was now called the Leibniz Foundation and also served the needs of institutes in the former DDR. To get on the list was a very difficult project that advanced only slowly, but we suddenly got help from an unexpected quarter, namely President Rau of the Federal Republic of Germany. On a visit to Denmark, during a dinner at a meeting on science and technology, Rau was seated next to our friend and colleague Ole Barndorff-Nielsen. Ole was quite indignant about the way Oberwolfach was treated by the German authorities and raised the topic with Rau. Rau listened carefully and on his return to Germany he sent what he had heard down the bureaucratic channels, and lo and behold, we received a call from the Federal Ministry of Science and Technological Development inquiring whether Oberwolfach needed something. Of course this was only the beginning of a long bureaucratic process, because Oberwolfach is not a standard research institute, but in the end Oberwolfach joined the Leibniz Foundation on January 1, 2005, and the foundation is now rather proud of us. So Oberwolfach is now supported by the Local as well as the Federal Government, and for a start, all buildings received a major overhaul to bring them up to date.

Interviewer: I'm really happy to hear this. Congratulations!

F.G.: There were also important changes in the scientific program of Oberwolfach. There used to be annually returning meetings with very broad topics and organized by the same small group of people. This doesn't happen anymore.

After Kreck left, we were fortunate to find an excellent successor in Gerd Martin Greuel. Greuel was able to increase the amount of additional grant money from various sources considerably. He was also responsible for making Oberwolfach a center for mathematical documents of various kinds. This was in line with the application to the Leibniz Foundation where Oberwolfach was presented not only as a meeting place, but also as a keeper of records, including those of its own history because many important mathematical results were first presented at Oberwolfach. Starting with the excellent library which has many books that universities cannot afford because of budget constraints, Greuel added electronic

records of the Oberwolfach meetings where one can find what was discussed at any given meeting and who were present.

Interviewer: Yes, I have just taken a look at this on internet and I'm impressed. It is a real service to the profession. You can also find out how often you have attended Oberwolfach meetings yourself, which could become a new mathematical game like comparing Erdös numbers. Let me add that I like Greuel very much.

F.G.: Everybody does, but he has turned 65 and will be retiring when we have found a successor. We are hopeful to find a good person.

6 Relations with Eastern Europe

Interviewer: For many years you and I have both been heavily involved in establishing relations with fellow scientists in Eastern Europe and the former Soviet Union. It all started when a few major scientists like Hájek and Révész showed up in places like Berkeley and Oberwolfach and we got to know each other. Next the European Meetings of Statisticians came to be held in Eastern Europe with some regularity: first in Budapest in 1972, next in Prague, Varna, East Berlin and Wroclaw. At the same time Western participation in existing locally organized meetings in Prague, Budapest and Vilnius increased sharply. In 1975 the Bernoulli Society was founded partly to build bridges between Eastern Europe and the West and in 1986 the World Meeting in Tashkent proved its success. At that time you had attended quite a few of these meetings.

F.G.: Yes, it was often the only way to meet people from Eastern Europe and the Soviet Union, because they would be allowed to attend meetings in the West only rarely. After the collapse of the communist regimes this was still difficult for financial reasons. The Tashkent World Meeting was the first statistics meeting in the Soviet Union attended by a huge number of participants from all over the world, in particular from the United States. For many of them, used to Cold War rhetoric, it was an eye-opener to see the circumstances in the Soviet Union for the first time and notice the natural anarchy that was present everywhere and made even getting there an exciting adventure.

Interviewer: These meetings were always held as far away from Moscow as possible to avoid official interference. In that respect Vilnius was good, but Tashkent was even better!

F.G.: At that time we didn't have many possibilities to invite colleagues from Eastern Europe and the Soviet Union to visit us and take part in research projects. That changed after the collapse of the Soviet Union. First the Soros Foundation provided funds for this and then the European Union started a program named INTAS for scientific collaboration with the former Soviet Union. Living conditions of scientists in the former Soviet Union were desperate: salaries were not sufficient

to cover basic needs, if they were paid at all. Many scientists were looking for opportunities for leaving the country and finding a position in the West. The INTAS program was aimed at joint research projects of participants in the former Soviet Union and in the European Union. The former participants would receive a supplement of their salaries, whereas the latter should apply for and administer the research program, which turned out to consist of writing an endless series of reports and solving problems with the Brussels bureaucracy and Russian banks. Most of the people running INTAS had no previous involvement with the former Soviet Union, which made it difficult for them to understand what goes on in the minds of the many different peoples who made up the Soviet Union. We also learned quickly that calling Brussels bureaucrats before 3 p.m. was useless because they were apparently having a good lunch. The banks turned out to be a more difficult problem. The cost of transferring money to participants was astronomical without any guarantee that the money would actually arrive. I understand that in some cases the money was actually brought to Moscow by a messenger who handed it out at the airport to people showing a passport with the right name.

Before we had even heard of the existence of INTAS, you and I both received a request from friends in Moscow to apply for an INTAS program for collaboration with a large group of really excellent probabilists and statisticians in the former Soviet Union. We did and proposed a program that needed a large number of special skills that were well represented in the group of participants. The program immediately got funded in the first round in 1993 and was extended for another period each time it ran out, until INTAS stopped operations in 2006.

Interviewer: Until my secretary and I both retired in 1999 we did the administration of the program in Leiden. My secretary used to refer to this job as the INTAS disaster, or words of similar meaning. After that the job went to you in Bielefeld, so it is clear that we truly shared the load.

One final remark: There was no provision for travel in the INTAS grants because the INTAS philosophy was clearly that everybody should stay where they were. No mass emigration to the West! However, there was no rule against consultations between different groups in the program, so of course quite a few people spent quite some time in Bielefeld and Leiden.

F.G.: After INTAS stopped, the Deutsche Forschungsgemeinschaft (D.F.G.) provided new programs for joint research with scientists in the former Soviet Union, which allowed them to spend time in Bielefeld. Other visitors were supported by the Humboldt Foundation and by the Sonderforschungsbereiche that we have had in Bielefeld.

Interviewer: Yes, there used to be an entire corridor in the institute in Bielefeld that was jokingly referred to as Moscow Boulevard.

Let me raise a further point in this connection. I have the impression that in probability and statistics we have had excellent relations with our colleagues in the former Soviet Union much earlier than in any other branch of mathematics. This may well be due to the fact that we always made contact on a personal rather than

an institutional basis. When the idea of the World Meeting in Tashkent came up, the Bernoulli Society, with much help from the members of its Russian branch, discussed the matter at length with all of their Russian friends, until almost everyone was confident it was a great idea that might be possible. Then after much lobbying the Soviet Academy of Science decided not to oppose it and it went through. If the Bernoulli Society had chosen the institutional way and wrote a letter to the Academy, they would certainly have received a negative reply, if any at all.

F.G.: Yes, definitely. The relations we had with the former Soviets were based on the fact that we attended all of these meetings together, got to know each other at a very early stage and developed a mutual trust. I have never noticed this in other branches of mathematics to this extent.

We should also not discount the role of the various nationalities in the Soviet Union. To organize a scientific meeting in Vilnius with a large international participation would be strongly supported by the local government, because it would show that Lithuania was not merely a part of the Soviet Union but also internationally recognized.

Interviewer: You are absolutely right.

7 German Reunification

Interviewer: Under the communist regimes, scientists basically had the same problems everywhere in Eastern Europe and the Soviet Union. Still East Germany was a somewhat special case. How did the German reunification affect East German science and scientists?

F.G.: The situation in East Germany was indeed very special. Two neighboring countries with the same language, the Eastern part watching Western television and seeing the wonderful world of Western luxury, while they couldn't buy bananas in the supermarket. After 1989 the original idea was that the two countries would remain separate for the time being, each with its own currency and a cheap-labor part in East Germany, and then slowly evolve into a single country. But it quickly became clear to the politicians that with the heavily guarded border gone, there was nothing to stop people moving to the more prosperous part. So they had to act fast and the East German parliament decided to join West Germany. It was not unification of two states and of two political systems, but of East Germany becoming part of the West German republic, with all of the legal and bureaucratic consequences that this implied. Of course this was easy for the West Germans because they wouldn't have to change anything.

But this created a problem for the sciences, because in East Germany, like in most Eastern European countries, scientific research was organized through the Academy of Sciences, and the East German academy employed about 30,000 people under the heading of scientific socialist production. Presumably these people did scientific research to enhance socialist production. This was not a happy idea, because the

people employed by the academy were not content to do purely applied work for industry and mostly did their own thing, whereas industry wasn't pleased to be told to let these people interfere with production, socialist or not! Most of this would have to go, because there was no way to employ 30,000 people at Western salaries, as would be a legal obligation for all civil servants.

A similar problem existed at the universities where people below the rank of professor also had permanent positions and would have to be paid Western salaries if they remained. Finally there were people with Party or Stasi affiliations which should not be retained. All of these problems would have to be solved before everybody became civil servants under West German law.

As a first step all members of the scientific staff of the Academy and the universities were dismissed. It was decided to follow the West German model where research is performed at the universities and at a limited number of specialized institutions with a far smaller staff than the former Academy institutes. Every qualified person could apply for one of the available positions, which meant that after being dismissed one could apply for one's previous position or any other. At the same time scientists from outside East Germany could also apply. At the universities there were honors committees of East German members as well as external hiring committees to reappoint people. This created uncomfortable situations where people like you and me had to review senior East German colleagues competing for their own jobs with young applicants from West Germany.

Interviewer: Yes, that's what I really found shocking.

F.G.: Another question was how to fund the specialized research institutions. Max Planck didn't want them. Fraunhofer didn't view these institutes as helpful for applied research with industry. So the only place for these institutes was the blue list of institutes financed jointly by the local State and the Federal Government that I mentioned earlier when speaking about Oberwolfach. For this new role the blue list was renamed Leibniz Foundation, which certainly sounds more dignified. When discussing which of the institutes should go to Leibniz I heard the representatives of the Federal Government make a promise that I never heard before or after. They told us to decide which institutes were really of a very high level, and whatever you find good, we'll pay for. It was amazing!

Interviewer: And did they actually keep their promise?

F.G.: Yes. Well look at the West German deficit at that time.

Of course this gave many people a very bad time. They had permanent positions and never expected to have to look for another job. But there was no other way. It all had to be done in a few months, which was a hectic time for all of these committees too. I was mainly involved with the Weierstrass Institute which was cut down from 200 to about 80 people.

So this was the reunification process, but then it was argued that this would have been a unique time for a real unification, in the sense of also cutting out some of the fat in the West German system. But as you can imagine with these time constraints other people argued convincingly against this.

Interviewer: Should things have been done differently or was there really no alternative?

F.G.: Theoretically we could have gone to a new structure together. But the West German institutions would not have been in a great hurry to submit plans and would certainly have fought this.

8 Visiting Committees

Interviewer: Friedrich, we have both spent a considerable amount of time on visiting committees that show up at mathematics departments of other universities and report what we think of them. If this opinion is not fit to be printed, we typically confuse the reader with a mountain of generalities.

F.G.: In Germany the visiting committee is a fairly recent phenomenon that was introduced when the golden years of expansion were over and serious cutbacks started in the eighties. Before that time all universities were supposed to have been created equal and it was blasphemy to try and rank them.

Interviewer: I think that this was true in most other countries too, and if it was done earlier, it was merely an intellectual exercise without serious consequences.

F.G.: By the end of the eighties the data collected by the visiting committees on research and teaching began to play a role in the allocation of funds.

Interviewer: Still the effect has generally been pretty minimal. The mathematics department at Leiden is usually declared to be the best in the country, but this never brought us a penny. It does make us more popular with the president of the university, though.

F.G.: Yes that is true in general. But at some smaller universities founded during the time of university expansion the staff was roughly of the same age so there was massive retirement 20–25 years later. In such a case it is probably not a good idea to let these senior people decide on the direction the department should take, so outside advice can be very useful.

Interviewer: Sure, such cases exist, but in most universities they are rare.

F.G.: Yes, but outside advice is becoming more and more common. A generation change that I just mentioned is one thing, but it now happens regularly. There is this excellence competition between universities, which forces them to choose main areas of research. Anytime a number of positions in a department are open for reappointment and a change of direction might be possible, the new constitution gives the university the option to make such strategic decisions its own responsibility. Of course they are also a bit at a loss what to do, so they call for outside advice.

Interviewer: I can imagine that you are concerned about this as it sounds really rather extreme to me. Does this also exist in this extreme form in other countries? Of course the president and rector are formally responsible for everything that goes on in a university and there is much talk of departments choosing main research areas, but in practice the department still decides whom to appoint in the Netherlands. I suppose that this outside advice also leads to a new bureaucracy to handle this advice and act on it?

F.G.: Exactly! The office of the president or the rector is acquiring a whole new group of people who organize these reviews, help in formulating long-term policies etc. So far these kinds of jobs seem to be more attractive to people with a background in the humanities rather the natural sciences.

Interviewer: It sounds gruesome. I can see only one positive side to this, which is that using visiting committees is certainly better than basing decisions on numbers of publications and citations. Bureaucrats usually prefer these numbers assuming that they provide "hard" evidence in contrast to peer review which is considered "soft".

9 Scientific Interests

Interviewer: During this interview we have repeatedly seen that your interest in mathematics doesn't stop with mathematical statistics and probability theory. During the symposium held on the occasion of your sixtieth birthday, Professor Hirzebruch who guided the work for your diploma in geometry, gave a lively account of this work. Let us now talk about your recent work in number theory.

F.G.: Willem, as you well know I started out in mathematical statistics with an interest in asymptotic expansions for the distributions of nonlinear statistical procedures like goodness-of-fit procedures, such as the Cramér-von Mises test. These may be viewed as expansions for the probabilities of ellipsoids in the Central Limit Theorem (CLT) in function spaces. The methods I developed for obtaining such expansions seemed to be interesting for a group of the Kolmogorov / Linnik school in probability in Russia working on these questions since the sixties. In the first SFB (1989–2000) in Bielefeld I originally worked on statistical problems in Markov chain Monte-Carlo, image restoration, as well as resampling methods, time-series and stochastic processes. But a number of researchers applied for and received Humboldt-fellowships to work with me on Berry-Esseen type bounds and asymptotic expansions for quadratic forms, U-statistics and Student statistics. These were Bentkus, Bloznelis, Rachkauskas, Tikhomirov, Zalesky and Zaitsev. The Humboldt-Foundation also helped to finance via Humboldt-Prizes the collaboration with Rabi Bhattacharya, David Mason and you.

Of all of the remaining open probabilistic questions concerning the rate of convergence in the high and infinite-dimensional CLT for regions defined by

quadratic forms, I felt that one was particularly important. It was raised in the seminal work by C.G. Esseen (1945) who proved that the error in the CLT for balls in dimension d is $\mathcal{O}(n^{-d/(d+1)})$. He noted that for sums of random vectors taking values in a lattice, his result is the equivalent in probability of classical results in analytic number theory by Landau and his students in the 1920s. They proved asymptotic rate bounds for the difference between the number of points of the standard lattice in ellipsoids of fixed shape blown up by a large radius factor and their corresponding Lebesgue-volume.

Interviewer: How did you approach these problems in number theory?

F.G.: In order to find the optimal rate in he CLT I started to study the old papers of Landau, Hardy and Littlewood and related papers by Weyl on this topic. First of all, I found out how rewarding it is to go back to the original sources concerning a problem. There you see the full force of the original arguments, whereas later publications often deal with refined versions of combinations of several methods of often undisclosed origin, which makes understanding the basic ideas and the further development much harder. It was very interesting to see a variety of methods that were either similar to or different from the ones I had used for the probabilistic questions. After intensive work I found ways to combine stochastic ideas with those of the classical analytic number theory establishing in this way a firm link of both worlds, where distributions on lattices turned out to provide the worst cases to be dealt with in the CLT.

I finally succeeded together with V. Bentkus to show the optimal rates of order n^{-1} for a sum of n vectors in the CLT, as well as in corresponding distributional problems in number theory in dimension 9 and larger. The chain of arguments started in number theory, improving Landau's bounds by new ones of optimal order and after that proceeding by representing distributional errors for sums by averages over errors for multinomial distributions on randomly selected lattices.

Interviewer: What was the role of the dimension in this problem. Esseen did not have any restriction in his bounds as far as I remember?

F.G.: It was clear by old results in number theory that dimensions 2–4 were different, hence one could not expect the same rate of convergence $\mathcal{O}(n^{-1})$ in the CLT for this case. But it took nearly a decade to get from dimension 9 to the final result for dimensions 5 and larger.

First this was done for ellipsoids in number theory in 2004, but the transfer to probability needed results for indefinite forms in number theory, which were obtained by means of quantitative equi-distribution results for orbits of 1-parameter (unipotent) subgroups jointly with G. Margulis 5 years later. The final transfer of these methods to the CLT in dimension 5 and larger with rate $\mathcal{O}(n^{-1})$ (without any $\log n$ factors) has been achieved last year jointly with A. Zaitsev. This closes the circle back to the original problem of Esseen.

Interviewer: Friedrich, thank you very much for taking so much of your time to tell us something about your career and the many activities that went with it.