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A conversation with Willem Rutger van Zwet

This interview was conducted at two occasions by Mathisca de Gunst, Chris Klaassen and Aad van der Vaart, all former students of Willem van Zwet (1934–2020). The sittings took place a while ago, in June 2000 and in 2001, in van Zwet's office at Leiden University, but the topics feel fresh. We start with a brief life history.

Willem Rutger van Zwet was born in 1934 in Leiden and passed away in 2020 at the age of 86. After attending gymnasia in Rotterdam and Den Haag, he studied physics and mathematics in Leiden. He chose to specialise in statistics, a fairly new subject at the time, and went on into a brilliant career in research and scientific administration. Throughout most of his career he was connected to Leiden University (his retirement lecture was titled 'No complaints so far'), but he travelled widely, with many visits to UC Berkeley and a half-time professorship at Chapel Hill from 1990–1998. During the cold war period, Willem played a significant role in the exchange between researchers on the two sides of the then 'iron curtain'.

Willem's research addressed asymptotic expansions and higher-order efficiency, and an eclectic range of other topics, including resampling methods, plant cell statistics and spatial stochastic processes (see [4] for an extensive review). Willem received numerous prizes and honours for his work (see [2]). A Willem van Zwet Medal and Willem R. van Zwet Award were established in his honor by the Bernoulli Society and the Dutch Statistical Society.

Willem used his talent as an organizer for Leiden University, but more so at the national and international level. From 1961–1996 he was almost continuously committed to the Mathematical Centre (now CWI) in Amsterdam. He was the co-founder and first director (1997–2000) of Eurandom in Eindhoven, and initiator of the Stochastics

Meetings Lunteren, the annual gathering of Dutch statisticians and probabilists with prominent speakers from all over the world, picked by Willem himself (1972–1999), which are still being continued. Perhaps the most impressive administrative achievements were international. Willem served as president of the three biggest international organisations in statistics, as Editor-in-Chief of two of the major journals, as board member of several international organisations, and held many other offices and memberships of programme, prize, review,

Willem van Zwet in 2013

advisory, restructuring, organising or publication committees (see [8]).

Without doubt Willem was the dominating force in statistics in the Netherlands for almost half a century. At one time Willem was the scientific father or grandfather of most mathematical statisticians in academia in the Netherlands and little could proceed without his knowing. Although Willem did not form a 'school' and was liberal as an adviser, his 16 PhD students took advantage of his authority, support and inspiration, and all of them pursued scientific careers at home or abroad [9].

Willem's career coincides with a period of major changes in statistics, and in the world of science in general, as also becomes clear in the interview below. Another interview [2] from 2006 by Rudy Beran and Nick Fisher is addressed more to the international community.

Study and van Dantzig

You taught us that "a mathematician always starts small in order to end big". How did you start? Did you study mathematics right from the beginning?
"No one studied mathematics right from the beginning in the fifties, but everybody studied physics. Only after going through three years of laboratory work, and God knows what, did I decide to study mathematics, when I found out that you could actually make a living in mathematics as a statistician. I had found out about statistics by a course in industrial statistics taught by Hans Sittig and A. R. van der Burg. Together with two fellow students, Jan van Ettinger Jr. and Jaap Fabius, we corrected the written homework. This was quite mysterious; we had no clue why they were dividing all these sums of squares, or what these sums of squares actually meant.[1] This is how I got interested in statistics. We had no idea of the mathematics behind it, or even whether there was any mathematics behind it, but we thought it was fun. Then I decided that I was absolutely fed up with physics, and studied mathematics for the second half of my studies.

I tried to find a professor of statistics, and there turned out to be one, who was teaching the 'theory of collective phenomena'. This is what he was officially teaching, but after a little trouble we discovered that it actually was probability and statistics. Thus I was a student in Leiden

David van Dantzig in 1934

and travelled once a week to Amsterdam to take David van Dantzig's course. This is how I got into mathematics and into statistics at the same time."

Why this strange title 'theory of collective phenomena' for a statistics course?
"Well, I have no clue, but it was probability and a lot of statistics. Of course, van Dantzig taught whatever he damn-well liked. He would start out a course and end up somewhere completely different. One course on fiducial inference was supposed to be a standard course, but after one or two weeks he decided, that he didn't want to teach about fiducial inference. He was writing a book review on Fisher's last book[2] and wanted to find out whether he could make any mathematical sense out of that. He sort of swung back and forth; one week he said: 'Oh yes! Now I understand what Fisher meant.' The next week it was: 'Oh no, it was all hopeless.' He ended up concluding that it was totally hopeless and wrote this wonderful and odd book review 'Statistical Priesthood II' [7]."

There was a number I also?
"There was a number one [6]; that was on Savage's book. These two book reviews were hardly read outside the Netherlands, but they were absolutely wonderful. They made quite a bit of fun of Fisher also. Van Dantzig could be really quite scathing. If he really felt something, then he would go out and say it. In the English speaking world, you don't do that sort of thing, but van

Dantzig surely did. He wasn't old fashioned at all about that."

Was he a pure mathematician?
"Yes, he was a topologist, I think, originally. He got his degree with van der Waerden. They were probably students together; van der Waerden was very young as a professor in Groningen. Van Dantzig was appointed as a lecturer in Delft in the 1930s, and [in 1940] immediately fired by the Germans, because he was Jewish. He apparently hid during the war in a very practical manner. I mean, it was possible to get hold of him, but somehow they never caught him. I think that already before the war, he had been studying probability a little, but during the war, he was bored and had nothing else to do, and also started to learn a little statistics.

After the war he had this idea — he and a couple of other people — that mathematics could contribute to the re-building of the world. They set up the *Mathematical Centre*[3] as a centre for applied mathematics, which was statistics and probability, and numerical analysis.

Van Dantzig was a very strange person. He was a pure mathematician, certainly also when doing statistics. I don't think he did any applied statistics, but he was a great, well, promoter of statistics. Van Ettinger was called the promoter, van Dantzig the visionary, and Hemelrijk the travelling salesman.[4]

Van Dantzig's main emphasis was on modelling. I think he was one of the first people who actually understood what a mathematical model was. To us, the three students in his class, it sounded clear and obvious, but he kept explaining it, until it was pretty boring. It seems that many people have great difficulties. Right now we teach courses on modelling and that sort of thing, right? I never could understand the problem, but that was probably because van Dantzig had very clear ideas about it.

The other thing was that he gave these more or less 'random' lectures. Quite often the proofs went wrong. At the end of the hour, he would say 'You'd better look at it yourself' or 'This is how easy it is to cheat students'. The courses were supposed to be from 2:00 till 4:00, but never stopped before 5:30. It really was tough, and you had to take them for three years before he would even consider letting you take the

exam. Then the exam wasn't on anything he had taught, but about some *Annals* [*of Mathematical Statistics*] paper to read. I actually got two; that was hard work."

It still attracted you to statistics?
"Statistics was still very rudimentary, I mean, they knew a lot more on probability in those days. But it was absolutely wonderful. Van Dantzig taught you a lot of things that nobody else knows, while things that everybody knows, he never got to. But it was spectacular. He would come into the classroom with a little piece of paper and start somewhere, and nobody knew, he included, where this was going. It was adventurous. I loved probability.

In Amsterdam they worried about the axiom of choice, but in Leiden they didn't see this as an issue. The axiom of choice, if you wanted to use it, fine. Van Dantzig apologised every time he was using it."

He had to do this because of Brouwer?
"I don't know, this was tradition, I guess. Yes, it was quite interesting with these three students."

One of them was Jaap Fabius?
"Yes, Jaap, and for a while there was Fred Steutel. We were sitting there for two years and had to answer all the questions, before Fred showed up. Then Jaap and I developed a technique of looking superior, and looking at Fred as if we were wondering whether he knew the answer. Van Dantzig forced him to answer. Poor Fred."

Was van Dantzig connected to international statistics and research?
"Yes, he definitely was, I mean, he was an international figure and they all knew him."

In statistics?
"The distinction wasn't that clear at the time. Probability and statistics were not totally separate fields, as they are now, unfortunately. They knew him, because he'd been to Berkeley and attended the European Meetings of Statisticians. Van Dantzig was an authority. He did some work in probability himself, and a lot of it was published by students."

PhD and Hemelrijk

When you finished your undergraduate studies, did you continue as a PhD student?
"I wanted to go to Berkeley. You would think van Dantzig would mind that his students were running away, but he thought it was fine. Berkeley was the magic word. Van Dantzig had been there and he was very much in favour and willing to write letters. But I was having such problems with the army. There was no way of going anywhere before doing my twenty-one months in the army. So Jaap Fabius went there instead."

After the basic training of two months and four months at the officer's infantry school, Willem got a position at a military laboratory via a fraternity brother of his future father-in-law. This laboratory developed a special type of sonar.

"It was fun, I mean, I learned some physics. And the physics wasn't so bad, it was mainly Bessel functions."

After your army service, you did not want to go to Berkeley anymore?
"I had lost so many years by that time, that I felt that I needed to move quickly. I was 26. I started out young, because I skipped first grade of primary school, but lost many years as an undergraduate. Van Dantzig had died in the meantime, that was the bad part. So I went to Hemelrijk. I remember, my worst fear was becoming a doctor of engineering science, or something, which Hemelrijk was teaching in Delft."

So Hemelrijk had not yet succeeded van Dantzig?
"No. When van Dantzig died, I started to talk to Hemelrijk when I was still in the army, and he said he would be going to Amsterdam next September. Then I got a job with Hemelrijk at the Mathematical Centre."

Was that job directed at getting a PhD or more like a consulting job?
"No. Well, you were supposed to get your PhD, and some people actually did, but quite a few never got there. When I arrived, Hemelrijk was there, Bloemena had just died and Stan van Eeden had just left to the United States. There was me, Hemelrijk and an undergraduate student or assistant. They gave me a healthy big pile of consulting things and said 'Go ahead'. I really had no clue, but Hemelrijk was wonderful. He showed up three mornings a week, Monday, Wednesday and Friday, and we would spend half of his time talking about all this consulting business. Koos Kriens, the head of the OR-group, helped a bit. Time went by very quickly, because you had to survive. I also taught a course, the famous teachers course.

After one year I was thoroughly overworked and on the verge of collapse. I didn't realize that at the time, but Hemelrijk got worried. There was absolutely no time for doing research, because I spent all my working hours doing consulting. They promoted me to 'sous-chef' after six months, but I still had nobody working for me. It meant a 50% raise in salary, so that was nice.

I must have told you the story of my PhD thesis. Hemelrijk had a folder. Whenever he met somebody who had a problem which he couldn't solve, he wrote it on a

Willem van Zwet (second from the left) at his student club Minerva in Leiden in 1954. First from the left is Jaap Fabius, who later became professor of probability in Leiden.

Willem van Zwet and Jan Hemelrijk in the mid 1970s at the entrance of the Mathematisch Centrum at the 2e Boerhaavestraat 49 in Amsterdam.

piece of paper and put it in this folder. When you indicated you wanted to get a PhD, he handed you this folder. Most of the things in the folder were pretty awful, but there was one on order statistics. Because I didn't know anything about order statistics, it looked mildly interesting. So I looked at this problem for two months or so, and solved it! I was very, very pleased with myself, and went to Hemelrijk and told him about the piece I worked on. Then he replied: 'Of all the uninteresting things I have seen in my life, this beats everything.' But I persevered and made a thesis out of it, which I don't think Hemelrijk ever read. For myself, I think it is a nice thesis. It is totally idiosyncratic; it has nothing to do with anything else. The subject has become a little cottage industry; a number of people keep doing this sort of thing over and over, every ten years or so.

At the time I had no clue of what was going on in statistics and just did whatever I thought might work. I started reading journals, but it takes a while before you get any idea. At the Mathematical Centre everybody was doing rank tests, but I thought that was a non-subject. They were all doing that for generations, except for Stan van Eeden, who was doing isotonic regression, and Bloemena, who wrote a very nice thesis. That was another assignment I got: Bloemena died and I had to finish his thesis. It became a nice, but very strange thesis, with very complicated moment computations (see [5]). So I wasn't the only one who was fed up with rank tests.

Hemelrijk was basically not much interested in mathematics, but more into applied statistics, and he was bloody good

at it. Hemelrijk could sell you his grandmother. He was terrific, I have great admiration for him."

Do you think of Hemelrijk as somebody who had a lot of influence on your later career?
"I learned a lot from Hemelrijk. I am pretty fast, but he is ten times as fast as I am; I was continuously rushing to keep up with him.

It is hard to describe what you have learned from a person; I think it was more like an attitude towards statistics. Of course, there is always this influence of a thesis adviser. You spend an enormous amount of time with your PhD adviser, although Hemelrijk was not advising me on my PhD, but we were doing consulting together. At the end he asked if I thought that everything was correct. I don't think we ever talked about my thesis.

Naturally you go into another direction after that. When I left the Mathematical Centre, I felt that what was needed in Holland is to get statistics on a decent mathematical level. That was of no interest to Hemelrijk. So we were very different people with very different ways.

I believe that Hemelrijk has written somewhere that I am as fanatic as van Dantzig in mathematics. I don't know where he got this idea from."

Lector, full professor

After his PhD at the University of Amsterdam in 1964, Willem was appointed at Leiden University. He stayed there for the rest of his career.

You became a professor at Leiden University?
"No, I became lector, that is something very different. You had two kinds of 'lectoren'. One kind is a private property of a professor. There were a lot of professors who had their 'own' lector, who did all the work, of course. I was of the other type, I wasn't owned by anybody. I was of the type that had such an unimportant subject that you needed no full professor to do that.

Kloosterman [chair of the mathematics department in the 1960s] had no sympathy for applied mathematics, or whatsoever. But he was a smart person, and realised he needed it. He figured that it was a pretty risky thing to have applied mathematicians in his department, and decided to find out

which of these applied mathematicians had actually been a decent student in his number theory course. I think that was basically the criterion. So, Guus Zoutendijk was the first applied mathematics professor in Leiden. Guus ran the computing institute, 'Centraal Rekeninstituut'. I had the choice of being in the mathematical or the computing institute. Of course, I picked the computing institute.

I went on leave on the day I got appointed. Hofstee [from the 'College van Curatoren'] said that the university had been doing without statistics for 400 years, six more months wouldn't matter too much.

The irritating part of being lector was, that you couldn't get a PhD student, because a lector wasn't supposed to do that. I think Kobus [Oosterhoff] got his degree with me just after I got appointed full professor. I don't know what would have happened otherwise."

You were a lector for only a short time?
"For three years or so. I got an offer to become a full professor in Eindhoven. So they had to do something in Leiden."

Were there differences between the Mathematical Centre and the applied mathematics department in Leiden?
"In Amsterdam they all trooped to the Mathematical Centre for statistical advice, but in Leiden nobody seemed to have the need for that. That was fine, as far as I'm concerned, because I had been doing nothing but consulting for the past three and a half years.

In the first place, I had to set up all my courses, and whatever. Rob van der Vaart

Willem van Zwet and his wife Lucie in 1966, receiving congratulations from Hofstee, from the executive board of Leiden University, after his inaugural lecture.

had been teaching statistics. He was a biologist and had his own department of theoretical biology. He was the first guy to buy a computer, before the math department, where they didn't want anything to do with this. I had been a 'student assistent' with him; so we knew each other vaguely. After my first year at the Mathematical Centre, van der Vaart quit in an enormous row with the regents of the university. I got a letter or phone call from the chair of the math department, asking me if I would be willing to take over his course, in the middle of the year. I said fine and Hemelrijk said it was fine also. I remember that I knocked on van der Vaart's door to talk to him, here in Leiden, but there was no answer. So I opened the door and he pushed me out and was screaming at the top of his voice, scolding the administration.

But I took over his course, and I was teaching here one day of the week. It was a wonderful course, because it was really a mathematics course, with a book that was extremely mathematical to biologists. So we had to have a vote in class after a few weeks. They would all much prefer not to see this book anymore. Thus I changed the course and tried to explain all the things of statistics.

So I had been teaching here for a number of years before 1965, when I really came back to Leiden, and had time to actually work. I started to work with Kobus Oosterhoff on the likelihood ratio test for the multinomial, and we wrote a paper on that for the Berkeley Symposium of 1970 (see [13]). I got invited, it was a great sensation."

Berkeley

Right from the start of his appointment in Leiden, Willem went on leave to Eugene, Oregon, where Fred Andrews and Don Truax were his hosts. Fred recommended him to Betty Scott, so he could attend the fifth Berkeley Symposium on Mathematical Statistics and Probability in 1965. These renowned symposia were held every five years between 1945–1970, and were important in the development of mathematical statistics, in which Berkeley in general also played a central role.

"When I went to the 1965 symposium, I was very much impressed by Erich Lehmann. Erich has a wonderful capacity for making everybody feel good. I showed up

there and found out that he actually at least looked through my thesis. He was always quite nice to young people, making them feel at home, asking them out for dinner.

I had written this crazy thesis about weak ordering of distributions. It turned out, that was exactly what the reliability people were doing, only they ordered everything with respect to the exponentials, which was sort of a special case; and they were doing this with great gusto. I ran into Dick Barlow [Richard Barlow, one of the founders of modern reliability theory], probably after a symposium. I spent the summer of 1967 working with Dick on reliability things. This is, I think, how I got to know people at Berkeley a little bit. Dick was always very nice. I was in Berkeley again in 1972, for half a year, and that's when I got working with Peter Bickel. I hardly spent more than five minutes with Dick, although his grant paid for my stay. He never sort of objected to that. Yes, I think that was the connection with Berkeley at first.

Somebody asked me to discuss some Bickel paper, where he was proving an Edgeworth expansion theorem for some rank test. I looked at it and said that I could do much better than that. Peter didn't believe me, of course. We started writing together and published the results in 1978, the longest paper in the *Annals of Statistics* ever (see [3])."

Even if you left out a lot of computations.
"We left out a lot of computations and people are still worrying about that. There is an enormous amount of technical stuff in there, that you can use for anything. Since the paper is about rank tests and rank tests went out of fashion a few years later, lots of people haven't realised that a lot of stuff they are doing, is already in the appendix of that paper.

I remember very strongly that my main reason for going to Oregon, and going to meetings in the United States, was that I really wanted to find out where I was in the scheme of things. Writing my PhD thesis was fun, but it had nothing to do with what everybody else was doing. I just wanted to know 'How bad are you' or 'How good are you compared to others'. You have to find out by reading these people or talking to them.

In Eugene Don Truax got me interested in the work from [Samuel] Karlin on total positivity. I remember I gave a lec-

ture somewhere in the midwest about my research on total positivity. There was an elderly gentleman sitting in the audience and at the end he asked a question, and I answered it. After the lecture, he came to me and said: 'That was a fairly stupid question, wasn't it?' I said: 'Well,...' Then he said: 'I am [Isaac Jacob] Schoenberg.' He is the inventor of this whole idea. I didn't count on this at all. Karlin had picked up the thing from Schoenberg.

A similar thing happened to me during a European Meeting [of Statisticians]. I lectured about some Berry–Esseen theorem. Somebody stepped up to me and said: 'I am [Carl Gustav] Esseen.' I thought that the guy had been dead for 15 years.

Yes, I have met some historical figures. I was allowed to go to the ISI meeting in Paris in 1961. I went there without writing a paper; this was a high exception. There, I actually realised that people like Neyman and Fisher exist. That was a great discovery. There was a wonderful boat ride on the Seine in the evening, where I got to talk to Jerzey Neyman. This made an enormous impression on me, that the people you read, are real living human beings."

Did you speak to Fisher also?
"No. Fisher was there at the ISI meeting in Paris. Allan Birnbaum was trying to reconcile the usual inference issues in his lecture. At the end Fisher got up and started off that, of course, Birnbaum did valuable work, but this was total nonsense. It was a shame and a scandal, because Birnbaum was trying to reconcile Fisher's ideas with those of Neyman, while everybody knew that Fisher was right and Neyman was wrong. Neyman was just sitting there looking at his shoes."

Dutch statistics

That was in the early years. You mentioned, that you wanted to get Dutch statistics on a higher mathematical level. Did you have a plan for that?
"I thought that mathematical statistics was really in the saddest state. Van Dantzig had died and, as I said, he was mainly a foundations person. Stan van Eeden was doing isotonic regression before it was known. That was really nice, it came out of consulting obviously. Bloemena was also doing something totally original. But Bloemena died and Stan left. There had been a big brain drain.

The thing you can do is to have students. I think that has been reasonably successful.

But I also feel a little bit responsible, that in the meantime applied statistics got less attention. I have really a good bit of appreciation for people like Sittig, van Ettinger and Hemelrijk, who did a magnificent job in applied statistics. Ronald Does is trying to sort of revive the industrial statistics tradition in Holland. I mean, for years and years people like Hamaker and Sittig have had no successors. I felt a little bit bad about that. At the time I didn't realize it, of course."

Were you happy to leave the consultation at the CWI?
"I was quite pleased to get rid of that, because most of the consulting was really very short time business. You did something and you worked it out and that's it. You didn't have much time or occasion for thinking it over, or doing novel things. Very often it was standard and quite boring.

I regret that we have completely failed in this country to put statistics on the agenda in policy making or so."

Is that different from other countries?
"I don't know. It is probably different from Britain and also from the United States. In policy making we really haven't achieved anything. Last week I testified before the 'Raad van State' on the 'Betuwelijn'. My God, I might as well have stayed home. But I think we have to keep explaining. The Centraal Plan Bureau is basically this gigantic economic model that Tinbergen made and not very much else. They have been saying terrible things about the 'Betuwelijn', these recent years. But you make a very, very, I think, understandable statement about some statistical matters and then there is the 'landsadvocaat' who (a) doesn't understand what you're saying, and (b) interprets it as: 'Ah, it's nonsense. We all know better.'

The question is, of course, whether statistics is a science and if it is, whether it will continue to be a science. I mean everybody is peddling our waves nowadays."

Statistics a science?

Isn't social statistics one of the origins of the field?
"Friedrich [Götze] has put it pretty simple recently in a conversation. He said: 'If statistics is a science, there ought to be a central body of knowledge. Tell me what it is in relation to the problems that you are attacking.'"

Are you concerned that we don't have a central body of knowledge?
"I think we have that, but the development of practical statistics is away from that. For instance, it is clear that one of the central concepts — this is my personal opinion — that distinguishes statistics from anything else, is that you make statements, but you also say something about the chances that your statement, or whatever, is wrong. That is absolutely essential for statistics. But nobody is doing that anymore. Maybe it is too difficult, but as soon as that is lacking, you're just like any other picture drawer.

I think there is a big gap right now between expanding the central body of core knowledge and what actually happens in applications. Most applied work nowadays is on data-mining kind of things, and this has almost no, or very little, probabilistic content. I think that the moment that you ignore probability and just make statements, then you're no different from any other citizen out there in the street."

Is it clear that probability models are always useful in areas like data-mining?
"They are always useful if you can say something about the chances you're wrong. The problem is that you often can't, but then it is doubtful whether you're doing statistics, or any applied analysis or numerical analysis, or whatever. To me, trying to say something about the chances you're wrong is an essential element of statistics.

First, we were for a time in a totally closed off world doing our own things. For many, many years people have been working on a restricted set of problems and getting a nice and clean theory about that. Then there was in the 1970s or 1980s this swing back, and people were saying that statistics should go back to applications. But that is not so easy, I mean, you can immediately come up with lots of applied problems that you can't solve. It is a healthy sign that you suddenly are saying 'We know all this old stuff, let's look at new things', but then a long time is needed before you catch up with your theory.

Today again, the practical problems are running away. They are becoming more and more complicated, and any computer scientist can solve a problem, that we can't hope to do very much about for the next God knows how many years. If you can't catch up somehow, or do something useful, you can lose out. Somebody else will run away with your profession. That, I think, is a serious problem. I do think statistics is a valuable science."

Are computer scientists the main competitors?
"No, they are everywhere. Of course, the computer scientists have this enormous computing power. I think essentially you have to ask yourself questions — let me not phrase this as probabilities that you're wrong — about the relevance of your conclusions in some sense, and then probability models seem the obvious thing to do. If you have no model, or you have a model, but you don't worry about validating, I think you are doing a different sort of thing.

But... there are many doomsday profits. I mean, it is also an interesting kind of time. Things are changing rapidly."

Regarding applications, you mentioned government and industry, but not other sciences.
"I think our relationship with other sciences is much better than with industry and certainly with the government. There're lots of people in science who are willing to talk to you, because they realize they have problems that you can do something about. In physics and science this is very typical and very clear, usually they can do their own job. They have enough statistics and don't do it badly. I think the situation with social sciences has much improved. There was a wall between the mathematicians and the social scientists, but I think this is no longer true or far less true.

For the government statistics does not exist."

Can it be made visible by defining statistics as handling uncertainty?
"Yes, but they're not interested. What politicians hate most of all is uncertainty. If you tell a politician that the number of people that will be out of a job will be between 4 and 10 percent, he doesn't know what to do anymore. Which is not so surprising, of course. What they like, I've found, is that you give them a number and assure them it is a reliable number.

These people do have a difficult job. What the hell do you do when someone tells you it is between here and there?"

Maybe a statistician should say more?
"Yes, but anyhow they hate anything statistical. They really don't like them at all. And this is what we have to break down somehow."

Do you think this is a typically Dutch situation?
"No, this is a problem of many European countries.

In the US professional statisticians were heard by congress about the undercount. That was a big thing, which attracted a lot of attention. Lots of people were not represented in the census samples, and they worried if they should correct or not correct for that, et cetera. That was a debate where politicians were interested in the outcome. Lots of people, like David Freedman, were making statements. That was a probabilistic debate on a public issue. I can't remember that we ever had that in our country.

I think there are many more statisticians in the United States at every level. In England this is also the tradition, but even there you can't say, that there is a tremendous influence."

Bayesianism

At a joint meeting of the Dutch Statistical Society and the Dutch Mathematical Society, around 1980, Willem gave an opinionated lecture on Bayesian statistics, one of the paradigms in statistics. The lecture was to be published in *Statistica Neerlandica*, but was apparently refused on the grounds that it was overly negative or even 'schandalig'. In an interview at the occasion of becoming honorary member of the Dutch Statistical Society [*STAtOR* 1(1), 2000], Willem calls himself 'very moderate in this matter, but apparently other people do not agree'.

What was the title of this lecture?
"It was 'Statistiek of Zwarte Kunst', and it is among my unpublished work. It was nothing deep. All I said was that I can understand that somebody likes to use the average loss over the parameter space as criterion, as long as you realize that this is just one particular average. So I

feel happier with a non-Bayesian solution if that exists. Hemelrijk always said that Bayesian statistics was fine, because it gives you simpler computations, but that is about the only reason. Van Dantzig was maybe not raving against it, but he was certainly against it. You should read 'Statistical Priesthood I' [6] [a review of Savage's book]. I never have met Jimmy Savage. He was very highly regarded, to my knowledge.

What I can't stand are these 'beliefs discussions'. At that time it was like, you had to be a Bayesian, everything else was wrong. They came up with the famous examples of where frequentists go wrong, silly examples of negative variance, or so. The technique was to define a frequentist in a way that nobody in his right mind would behave, and then shoot him down. I have really no patience with people like that. They don't seem to understand that the Neyman–Pearson theory is a stylised version of what you do in real life. We never completely behave like that."

Statistics and mathematics

We have been talking about mathematical statistics and applied statistics. Can you also say something about the relationship mathematics and mathematical statistics? Do you feel different as a mathematician, being a mathematical statistician?
"In mathematics you don't have any responsibility whatsoever for your choice of subjects. As long as it is a good mathematical subject — and nobody knows what that is, but everybody has ideas — you can do whatever you like. For a statistician, it has to have some relation to some practical situation, right? But given that, I don't think I would have selected different topics, if I had been a mathematician. You pick things for intrinsic interest.

The nice thing about probability, is that the problems are out in the street. This is true for number theory too, basically, but in a different way. I think being a pure mathematician, I would miss this aspect. I don't know how they do that. Once I asked a pure mathematician: 'How do you decide what is a good problem and what isn't?' This sort of shook him up terribly. He quoted all kinds of authorities. So you move the problem one step, and ask why these authorities think it is a good problem.

We statisticians always have some guidance, there has to be a certain relation with what is going on in the world out there. Often in a certain area they are trying to solve this problem, and other people are trying to create knowledge to help solve that problem. But somebody there has to make that choice first."

Number theory seems like a great field. Didn't you like number theory?
"A more interesting question is the way that pure mathematicians and statisticians deal with each other. You know all about that. In Leiden it is reasonable, we have always had the policy of 'live and let live', I won't meddle with them and they won't meddle with me. But it is not much more than that. There are other universities in the world, some not far from here, where the situation is radically different. Pure mathematicians make long noses to statisticians. This may be true in Leiden too, but they do it behind my back; so it doesn't have any effect."

Van Zwet school

Some people in the United States built a department. You didn't create one in Leiden, but you got a big department in The Netherlands, because almost everywhere in Dutch academia are your scientific children or grandchildren. Do you think this is a healthy situation, the van Zwet school?
"I see no problem, because I think people are sufficiently independent.

I am not — how do you say that — somebody who creates a school of followers, not at all. In fact, I really prefer to work alone, perhaps the only exception is Peter [Bickel]. I am interested in people writing a thesis with me, so I work with them, but having a graduate student is finite. I have hardly ever worked with students after they have finished, very, very seldom. Building up a group is not at all my way. I am much more of a problem solver than maker. You remember that in the old days, I used to have my students at the CWI in Amsterdam, to create some distance."

On the other hand, we have spent many afternoons here. Any time we knocked on your door, we were welcome to stay here the whole day.
"Yes, that is true. I spent an enormous amount of time with my students."

Did you like that?
"Yes, because each and everyone of you always worked — and that is not your doing alone of course — on something which interested me. I never asked people to work on something that did not interest me.

And so, yes I spent incredible amounts of time on two things, that nobody spends much time on: graduate students and the *Annals of Statistics*. No other editor has ever spent as much time on it as I did. If I start doing things really enthusiastically, I don't think much about the time."

Peace-medal Prague

Being, in his own words, a 'great believer in collaborating internationally', Willem has been very active in the international statistical community. He was the Editor-in-Chief of the *Annals of Statistics* and of *Bernoulli*. He was also president of the International Statistical Institute (ISI), the Institute of Mathematical Statistics (IMS) and of the Bernoulli Society for Mathematical Statistics and Probability, among many other things (see [8]). He was a leading figure in opening up dialogue with colleagues in Eastern Europe. He received the peace medal from Charles University in Prague in 1988 and an honorary doctorate in 1996.

You got a peace medal.
"Yes, I got a peace medal in Prague — at the time I enquired whether that was only given to communists, but it still exists.

[Rafail] Khasminskiĭ [Institute for Problems of Information Transmission, Moscow] and I got it together.

The relation with Eastern Europe, I always felt, was really important. The meeting in Tashkent [Bernoulli-IMS World Congress, 1986] was great. We needed years before [Yurii Vasil'evich] Prohorov [Steklov Mathematical Institute of the Russian Academy of Sciences] could be convinced that we organised it there. And there were other things that we have done. I think the Bernoulli Society is one of the few international scientific bodies that was sort of bringing people together very easily. [Albert Nikolaevich] Shiryaev [Steklov] would tell us whom to invite, realistically [from the USSR]. I have spent a lot of time doing the European Committee [of the Bernoulli Society]. That was really much more in some personal capacity. I was chairman of this committee, but I am not really much interested in the regular business of running a society. They usually have pretty capable people in their office."

Dean

Willem was dean of the Faculty of Science in Leiden in 1982–1983, at a time that the university was experiencing financial difficulties.

How about being dean?
"Running 'een faculteit', which I have done for two years, is much harder than an international society."

Why?
"Usually, you have to cut back, and nobody wants to be cut back."

Was it coincidence that you were dean at a time when important decisions had to be taken?
"There is a moment you can't escape. I think it is wrong to try to escape. There are obviously some people with absolutely zero or negative talent on these things and you'd better avoid them. But otherwise I think sort of everybody has to do it one time."

You must have done a good job, as it appears that after that they wanted you as rector.
"That is another danger. You have to say no to everything afterwards. It is very sneaky. You are doing nothing but sitting in meetings all day, and you're beginning to consider this as work. And then, after you're through with one job, they immediately ask you for the next one. So if you don't look out, you are in there forever. You have to take positive action to get out of the circuit.

I didn't think it was unenjoyable, because it gives you great insight in the human mind and human nature. Even though that doesn't make you terribly optimistic."

But you refused to run for rector?
"I think the university depends more on deans than on the rector. We used to say about the rector that he doesn't have any money."

Eurandom

Eurandom was established in 1997 as a European research institute for the study of random phenomena, located at the TU/e, and initially funded by a special grant from the Ministry of Education. It was the brainchild of Willem, who served as the first director from 1997–2000. Research programmes in mathematical and applied statistics, probability theory and stochastic operations research were carried out by postdocs and graduate students, mostly from abroad, and coordinated by Dutch senior scientists in the relevant areas. Workshops, seminars and a visitors programme were part of the scientific activities. In 2007 funding for postdocs stopped and Eurandom became a workshop and visitor centre. At the time of the interview it had just taken off.

From left to right Willem van Zwet, Jiři Anděl, Volker Mammitzsch, Peter Mandl, Peter Gaenssler, and Jitka Dupačová at the 1983 Prague Conference on Asymptotic Statistics.

Can you tell us about Eurandom?

"It is a great idea. So far I think we have been doing fine. Of course, there are things that are not perfect and ought to be done better. But on the whole, for something that has been going for a year and a half, I think it is really quite good. I think especially the probability science was spectacular. There is a difference between the different areas, because it is harder to get good people in one area than in another. So you can't really expect that everything takes off at the same speed.

We are now negotiating to get the future nailed down, because the first five years, we just live from our government money. After that we really need money from other countries, as well as NWO.

I think that what makes me optimistic, is not just that pretty good research is being done, but also that the first group of people that started a year and a half ago and are now looking for positions elsewhere — because we have two years appointments — seem to be pretty successful. They are going to good places. So it is a nice half way house, so to speak. That is not our main purpose, but it is a good side effect. I think the kids are extremely enthusiastic, everybody I know; they all say they love it."

Oberwolfach

Oberwolfach?

"Ah, my favourite resting place. There is only one Oberwolfach. It is a wonderful institute.

Yes, of course my April fool lecture! Long ago Dick Dudley, John Kingman, who were visiting Holland, and I, were going to a meeting. I was driving. During the ride John Kingman says: 'Do you realise that next Wednesday is April Fool's?' So, we discuss this for a while, and we make a pact that if any of the three of us had to speak on April one, he would give an April Fool's Lecture. We agree to this, not knowing that the first thing John Kingman does on the day of arrival is to speak to the chairman to have me scheduled at nine o'clock on April one. So the next day it becomes clear that I had to speak on Wednesday. I worried a great deal about this. I went to the organiser of this meeting, [Peter] Gänssler, and asked him what he would think if I gave an April Fool's lecture. He said that it was acceptable, if I promised
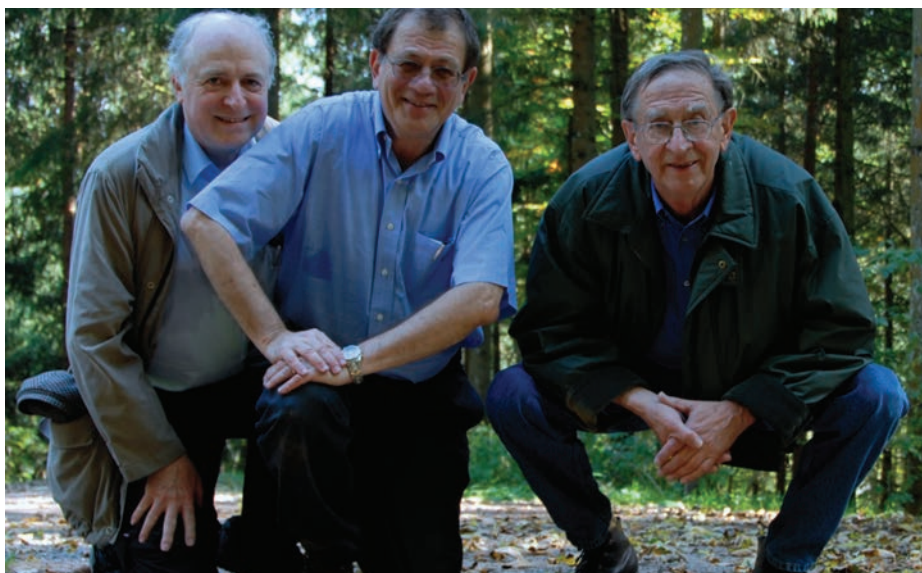
From left to right Friedrich Götze, Peter Bickel and Willem van Zwet at an Oberwolfach meeting in 2007.

to give a serious lecture afterwards and realised that I shouldn't do this again. So I gave this totally outrageous lecture. I proved all kinds of incredible things. And they all sat there and listened. Part of the deal was, if it would be obvious that nobody would react and think, that John would ask a question to make it clear. Of course, again, John being the sneaky man that he is, just sits there in the back of the room and lets me struggle. So it got worse and worse. At one point I walked between the two rows in the old building and stood still next to Georg Neuhaus. I said: 'Georg, this means that not only Le-Cam's first lemma, but also the second and the third are false.' I ended up by writing on the blackboard 'Today is April Fool's', and they finally got it. That afternoon every single German participant stopped by me on the walk [to Sankt Roman] and said: 'Of course I knew but I didn't want to spoil the fun.'

So I was not invited to Oberwolfach for two years or so, as a penalty due to my outrageous behavior."

Nevertheless as a statistician, you have been there most.

"Well, I must have been there fifty times. I love it. We had great fun in Oberwolfach."

How are these meetings important to the scientific knowledge?

"I don't know that they are. I guess it is a relaxed atmosphere and you get to know people. In those days the terrible word 'networking' didn't exist. I guess they called

it 'connecting with a couple of friends that you can rely on'. You know, this is a place where you get to know people really well. So you can count on them. For instance, when I needed an editorial board for the *Annals*, you are really demanding a lot from people. I think one person turned me down, but the rest, in about three days the crowd was there. We really worked hard and had a lot of fun. You can only have that if you are with a group of friendly people."

Major influence

You already mentioned a lot of people, but who has been a major professional influence on your career?

"I guess van Dantzig. It depends, within which boundaries these questions are. He had a major influence on becoming a mathematician or even statistician.

I guess a major influence for me as a student was Edsger Dijkstra. This may sound surprising, because he is a computer scientist. He was three years senior as a student. We could get along quite well and compete. He was a smart cookie.

Hemelrijk, I guess, for reasons I've explained.

I guess the person who probably influenced me most is Peter Bickel. In my farewell address I have said: 'We are both a little different than we would have been if we had never met.' At least, Nancy [Bickel] complains that Peter has some idiosyncrasies of mine now, and I have probably some of his.'"

Lehmann became a doctor honoris causa in Leiden [in 1985]. What convinced you to nominate him?

"In the first place Erich, as a human being, is a great person. Secondly, his influence on the profession is immense. He has been a stimulating influence and help to so many people. He has done first rate scientific work, but also his books [11, 12] were important to statistics."

Are these books still relevant? Will his work go to the next century?

"Yes it belongs to the central body of statistics. I think it will go. I mean, yes, in the sense that all the ideas come back in new general settings. I personally like his testing book best.

If you say 'Is this statistics today?', then, of course, many other things are better, but yes, this belongs to the central knowledge."

Doing mathematics

Willem retired from his position in Leiden in 1999, a year before this interview. From 2000–2003 he was Editor-in-Chief of the journal *Bernoulli*.

What will you do after Bernoulli?

"That is three years in the future from now. I want to do some mathematics. I have

to finish a number of things, before I do other things. As long as you don't go daft completely, mathematics is easy to do. It is fun too."

Easy compared to what?

"Compared to physical work, or whatever. You may get bored, I don't know about that, you may loose interest, but I haven't got to that point yet."

You once said that you might have done anything else, for instance that you could be a judge.

"Oh, yeah, lots of things. I got into this business totally random. The only thing I didn't like was physics."

Would you have liked being a judge?

"Why not? Another thing is being a simple-minded manager. I have realised quite often that in the last ten, fifteen years I have spent at least half my time managing things. I could have really made money had I done it somewhere else. It is crazy to play the role of a management person in the university. It is the most stupid place to do that."

But you like it in one way or another?

"Well, I don't dislike it. I don't know. I have an unfortunate tendency of thinking that, when thinking I am right, I am right. You may have noticed that. I like to convince

people. I think quite modestly that I am fairly successful with that.

People have such stupid ideas about running an organisation. For instance, here in Leiden most of my colleagues think that you have to cheat. That happens to be the only thing you should never do, because if you cheat once, that is the end of it. It is funny. People have no idea about how to run things. They are always thinking I am cheating them. It took me years to convince my colleagues here. I think they're finally beginning to understand that I am not cheating them all of the time. You have to have a certain basic honesty. You don't have to tell all of the facts all of the time to all of the people, but you certainly do not lie about it."

Looking back

What did you like most about your life as a statistician?

"I have said that in my final address: working with various students. And working with Peter [Bickel] and Friedrich [Götze]. Friedrich is really exceptionally bright, so is Peter of course."   ◁·····

Notes
1. These would have been the orthogonal decompositions of square norms, used in the *analysis of variance*, divided by their *degrees of freedom*.
2. R.A. Fisher is considered the most important figure in statistics in the first half of the twentieth century. Late in his career he introduced the principle of 'fiducial inference' (which never caught on).
3. The Mathematical Centre was founded in Amsterdam in 1946. The statistics department of it was run by Jan Hemelrijk, the successor of van Dantzig. Since 1983 it is called Centrum Wiskunde & Informatica (CWI).
4. Van Zwet referred here to [1], whereas in [10], Hemelrijk is quoted to have called van Dantzig the prophet, Sittig the missionary and Hemelrijk the travelling salesman.

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