Interview with MathMedia

Interviewee: Frans Oort
Interviewer: Ching-Li Chai
Venue: Institute of Mathematics Academia Sinica

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Ching-Li Chai(CHAI): Good morning, Frans. Doing this interview is a pleasure. I didn't expect this.

Frans Oort (OORT): The pleasure is mine. It's a surprise for both of us.

CHAI: There is a ritual that we first invite people to talk about their formative years which is always interesting because people's backgrounds are all different. Would you like to say something about your formative years or when you are younger and decided to go to mathematics? Some people believe that mathematicians are born.

The beginning[1]

OORT: Let me first say something about life as a mathematician, and later I will tell some stories about my personal life. For me it is a surprise you can be together with other people, with different cultural backgrounds, in history, in bringing up, with parents on the one hand, while on the other hand you can be so close to other people. With other mathematicians all of a sudden you understand each other on a much higher level than you ever thought possible. This is one of the most exciting things I have seen in my life. Certainly this is true with my collaborator here; with Ching-Li I have so much in common, and we understand each other. I think mathematically we have a deep contact and I am very grateful for that. That's certainly

[1] Preparing for the interview I had a structure in mind; I indicate this structure in the printed version.

something I will emphasize later in what I am going to say. This is just an introduction. I propose we start chronologically and then go through everything what has happened first to me and then what happened later and so on. Is that a good idea?

CHAI: Sounds like a wonderful idea.

OORT: Let me tell you that I was about eleven years old when I decided that I wanted to do mathematics. For me the most attractive part of mathematics is that you can describe geometric situations algebraically. This was from the beginning one of my main motivations and has been for all years after that. That was the starting point. Let me also stress that in my years in high school, certainly in my years in my university, I was not a very good student.

CHAI: That's a surprise. This is new to me.

OORT: There are two explanations to that at least. One is that as a young boy I was very outgoing and wanted to play on the street rather than sitting down and do my homework. I am grateful to my father who checked my homework and put away my hockey stick every moment I was not ready with my duties. He saved me from going astray completely. In my student years, I loved to do mathematics but I was not very good in reproducing mathematics. In reproducing I ever was 7 out of 10 or something like that. I mean sitting down and studying a book and not seeing the motivation for the question or a theory, and then going step by step through definitions was not very good for me. My ability only started when the creation of mathematics started, not the reproduction. If I may rate myself, I think that's one of my weak sides, that I am not very good technically. However, I think, my intuition and my creativity are balancing off those defects. One of my former students once said, well, Frans you are not very good as a mathematician technically but you have intuition. That was already the case when I was in university.

Let me point out three people who were important to me during my early studies. One of those, probably you do not know his name, he was called Johannes Haantjes [2]. He was a very energetic young person.

[2] Johannes Haantjes (1909-1956); Dutch mathematician, speciality differential geometry; he died young.

However, sadly, he died very young. He died in 1956 when I was just an undergraduate. I was sad because I liked him very much. He was a friendly teacher, and also a very good one. In his lectures he showed the elegance of mathematics. He knew all freshmen by name. He had a file with all our pictures and he knew you by your first name and so on. In retrospect, I don't know what would have happened if he wouldn't have died, because I like him as a person but his mathematics was differential geometry and it's not at all my cup of tea, but that's the way it goes.

The second one was Kloosterman[3]. He was a number theorist. He had the same age as my father and he was such a nice person. He was one of the top mathematicians of that generation. He did beautiful things. He was so ... affectionate is not the word, kind is not the word. He was deeply involved with his students. Partly, I think, because he had no children and he considered perhaps students were his children. He was extremely friendly to me. He did something to me which was important. When I became a graduate student I went to him and asked for a topic for a PhD. Of course I did it because I liked him very much. I knew he was an excellent mathematician and a very good teacher. One of his slogans was "The most difficult courses should be given by the best teachers. So I am always giving the freshman course." He was fantastic. He had a very dry set of humor. One of the first lecture he said "let me show you that number 120 is divisible by every integer. Let's try; $1, 2, 3$, yes, $4, 5$, yes, 6 yes. Let's try another integer, 12 yes. You see, 120 is divisible by every integer." There also is a secret between his former students: "let us take an arbitrary number" and we know what he would choose..... . I went to him for a PhD topic and he gave me straight away one. I worked for it for one day. I went back to him and said I am very sorry but I think I am not going to work on this. An absolute marvelous thing he did was he didn't get angry. He didn't protest but he completely understood. That's is one of the ideas guiding my students. You shall leave the person doing his or her own things and never interfere with the deep feelings what they like and what they don't like. Kloosterman was really instrumental in that he gave me the freedom on the one hand, but on the other hand, he supported me very much. He wrote very positive reports when I applied for a grant and so on. Moreover he was very open-minded; he was convinced algebraic geometry was one of the main ingredients, especially for number theory; history has proved he

[3] Hendrik Douwe Hendrik Kloosterman (1900-1968), Dutch mathematician, best known for his work in number theory and representation theory.

was right. He was one of the main people in my mathematical childhood.

There was another person who was at that moment important for me. A person you will not know. His name was Johan Feltkamp[4]. He was a flautist. When I was very young I wanted to play flute; my parents did not listen to me, especially not on on this topic. In my first year in Leiden, my student town, I went to a shop and bought a flute and thought, gosh, I need a teacher. I phoned Feltkamp and I asked him could you please be my teacher. That was kind of strange because he was considered to be one of the best flautists of the last century. He was one of the absolute top people. Now imagine somebody who has never touched a flute before going to this famous flautist and asking "can you please be my teacher?"; that is completely crazy. It was a strange thing to do. But also even more amazing, he immediately said yes. He was so kind to come from Amsterdam to Leiden to teach me. Wonderful things I learned from him. First of all, he taught me how to make a tone and that has stayed with me since then. Once I played somewhere and a famous flautist was in the same house and he asked " who has this wonderful tone". That was me. That was what Johan Feltkamp taught me. But he never showed me how I should do things. He said. "Here is a flute. Now you start breathing and start trying to make a tone, and just listen to yourself". After some days, after some weeks, you would have your first tone. That's hard. He never touched my flute. He never said what I should do. He only said "why don't you listen to yourself. Is this okay? Is this nice? Can you improve this?" Not saying you have to do this and you have to do that. Just say, well, try it. Then we tried. Very patiently he listened. He said now it's improving, yes, yes, yes. That's a deep feeling that you have to leave another person completely free and find his or her own way of doing things. That has been in my memory and feelings with me and that's the way I tried to be with my children. That's the way I tried to be with my students. Also with my colleagues and friends. I mean everybody has her or his own way of doing things and own feelings. Those were my student times. However, sometimes I was lonely, it was not a very pleasant period of my life.

I got a PhD topic and that was completely inapproachable. It was much too hard. It was the problem about the irreducibility of the variety of plane

[4]Johannes Hendricus (Johan) Feltkamp (1896-1962) was a Dutch flautist. Together with Jean-Pierre Rampal and Marcel Moyse he was considered as the best 20-th century flautists.

curves of given degree with a given number of nodes. Francesco Severi[5] wrote a book about algebraic geometry: *Vorlesungen über Algebraische Geometrie*. There is an appendix Anhang F. In that appendix he proves this irreducibility theorem in a typical "classical Italian way" of doing mathematics. You see pages and pages of talking, no formulas. In those pages, as it turned out, there was a mistake. The mistake basically is: if you take two irreducible varieties, you intersect them, then the intersection is supposed to be irreducible. But the mistake is hard to find, it being wrapped up in many obscure arguments.

CHAI: It's a wonderful theorem. If it were true.[6]

OORT: It uses this wring argument, so the proof was wrong. If you intersect a circle with a line, you can get two points instead of one. That intersection is reducible. It was the mistake he made, but this was all wrapped up in long Italian sentences. Well, of course, it was in German but in this Italian style. That was the problem I was given, finding a correct proof for the statement made by Severi. I sat at my desk for a full year not being able to do anything. Then the person who was giving me this problem went away for sabbatical. As the great Dutch philosopher Johan Cruijff[7] said "every disadvantage have its advantage".[8] There I was with no good problem and no person to help me. I wrote to Aldo Andreotti[9] who was in Pisa. Mind you this was in the old time without internet and e-mails, you could just write a letter. It took a long time before an answer came. Later it turned out what happened. I mailed my letter to Italy. The secretary there forwarded the letter to Andreotti who was in the United States without adding stamps. The letter was sent by surface mail. It was sitting in a boat for a long time. Finally it reached Aldo. By return mail he immediately said yes yes yes, you are very welcome. That was really the first big change in my mathematical life.

[5]Francesco Severi (1879-1961), Italian mathematician, famous for his contributions to algebraic geometry, one of the classical Italian geometers

[6]Many years later Joe Harris proved this "irreducibility of equisingular families of curves" to be correct (1985); methods used by Harris were not at all available back in 1959 when I worked on the problem.

[7]Hendrik Johannes Cruijff (1947-), Dutch soccer player, won the Ballon d'Or three times in 1971, 1973 and 1974, regarded as one of the greatest players in soccer.

[8]Yes, I know the grammar is wrong, but so was the grammar he used in this famous Dutch saying. This saying has been a true guide line and often a comfort for me in times that seemed difficult.

[9]Aldo Andreotti (1924-1980), Italian mathematician, worked on algebraic geometry, theory of functions of several complex variables and partial differential operators.

Aldo Andreotti and Pisa

Aldo Andreotti was the son of a famous sculptor. He was an artist. He was a very nice person. He was a great mathematician. He was a true geometer with a geometric intuition. He had an precious taste for mathematics. I obtained no results after several months, and reading a book I saw a problem, and I proposed to Andreotti to change to that; he gave me a very good advice: "if you keep moving from one to the next problem, you will not achieve much"; that was what I needed, and I cherished that advice for the rest of my mathematical life. I remember after half a year I had my first result. He was away for some time. I typed my result out and I still see the pages, 26 pages of mathematics, typed out, with symbols not on my typewriter added by hand in red and green. I gave the manuscript to him, expecting that he would go home and read it for two days and come with comments on it. He took my 26 pages, read through and said "Frans this is nice, but your proof should be intrinsic and not like this." He was correct. In my final PhD thesis this result was half a page and not 26 page.

CHAI: That can't be half a page.

OORT: Oh sure, it finally was half a page. My initial method was putting coordinates everywhere. Writing equations and so on. He was showing me the right way of doing things. Also Andreotti implicitly showed me how to give a topic to a student. You choose a question where the student has to be able to start. Then to understand what the basic problem is. Then the student has to learn quite a lot of useful techniques. And there must be at least some light at the end of a tunnel if you want to do a PhD research. I mean later in your career you can do things where there is no light in the tunnel. You can sit down and get desperate for many, many years. But as a student it is better to start somewhere. Andreotti gave me a beautiful problem which I liked, and on which I could start. I had to read Serre's *Faisceaux algébriques cohérents* (1955). I had to learn new things. I had to understand schemes and nilpotents. Andreotti had a preliminary manuscript of Grothendieck's Volume 1 of *Éléments de la géométrie algébriques* in his drawer and he didn't give it to me; his idea was it would be much better for me to find these things out by myself.

CHAI: In the drawer!

OORT: That was a very good thing, because I know if he would have given me these three hundred pages of difficult mathematics, I would have started and get completely lost; whereas at that moment, I really started myself. Here we see that things I find myself I understand much better, in general, than that I could learn by only reading. I like to quote Feynman[10] and come back to him later. Feynman once said "*I cannot understand anything in general unless I am carrying along in my mind a specific example.*" It was through examples that I started off to work. I worked through a lot of things. Well, you know my technique of getting really down to detailed examples and computations. Aldo Andreotti was nice to me. I can talk for hours about Aldo. Outside Italy, he was a very cosmopolitan person. Inside Italy he immediately changed into an Italian: complaining about bad things in the world, complaining about taxes, complaining about everything. When I made an appointment with him for say four o'clock, it would not before six thirty he would have time for me.

CHAI: It's like four o'clock Italian time, of course.

OORT: Exactly! Italian time! He was once in Amsterdam and there four o'clock was four o'clock. It was only back in Italy he was different. He had a fascinating personality. He was nice. He really understood my person and my needs. He was one of the big example of how to do mathematics and how to obtain a taste for good mathematics. To start with he gave me my PhD problem which I finished finally and then he was very happy with it. After one year in Pisa, I went to Paris.

Jean-Pierre Serre, Paris and much more

That was a good order, first Pisa, being basically on my own and perfect guidance and a topic by Andreotti. At least I had something done by myself. Then the lively Paris atmosphere. There I had the big chance, the big luck to become the student of Jean-Pierre Serre[11] who is a fantastic person. I have rarely seen a person of that quality

[10] Richard Phillips Feynman (1918-1988), American theoretical physicist, best know for his work in the path integral formulation of quantum mechanics.

[11] Jean-Pierre Serre (1926-), French mathematician, awarded the Fields Medal in 1954, the Wolf Prize in 2000, and the Abel Prize in 2003, made fundamental contributions to algebraic topology, algebraic geometry, and algebraic number theory.

in mathematics, in personality, in support, on the one hand. On the other hand, very sharp, if you say anything which is not correct, he would immediately go mad. You know that story about Serre sitting in the audience and somebody giving a talk, saying "this variety has no homology". Serre said "every variety has homology but you mean it is zero".

CHAI: The algebraic closure, that's the standard thing. One way to provoke him.

OORT: Well, I can talk for a long long time on Serre. He was very good to me. He was extremely supportive. Whenever I asked him we made an appointment in some Paris Café. There he would be sitting already before I came with a glass of beer, no introduction and no formalities, just "what do you want to know". Then I would ask him a question. He gave straight away a counter example. Anything more? Then I had another question and he gave a counter example. Then he said what do you really want to do? Then I explained to him what I was trying to do. Then he gave me two references and a few ideas, then he looked at me and said is that all? I said yes that's all for the time being, and he would leave and I would need two months or so to work out his suggestions. He gave me the right approach.

In Paris I also had contact with André Néron.[12] He had just proved a beautiful theorem and I visited him. I asked him to explain to me his theorem but I didn't understand much. I mean my French was reasonable but my mathematics was bad. "Morphismes p-morphiques"; he constructed what we now call "Néron minimal models". That was in 1961. It took me until 1967 reading the paper by Serre and and Tate[13] before I finally understood what the Néron minimal model is. I also met Grothendieck[14]. At the end of my Paris time I remembered that other problem I wanted to work on in Pisa; in one afternoon I completely solved it; during my Paris year I had learned so much that I now could easily prove something which was hard before. I recognized the way of looking at things as Serre had

[12]André Néron (1922 1985) was a French mathematician at the Université de Poitiers who worked on elliptic curves and abelian varieties. He defined and constructed the Néron minimal model.

[13]J-P. Serre & J. Tate - *Good reduction of abelian varieties.* Ann. Math. **88** (1968). 492-517.

[14]Alexander Grothendieck (1928-), German-born mathematician, awarded the Fields Medal in 1966, the central figure behind the creation of the modern theory of algebraic geometry.

taught me. That was my Paris time.

I think we should explain, especially to students, what learning by osmosis is. For a full year I followed courses in Paris. I didn't understand anything. Well, I thought that I didn't understand anything.

CHAI: That can't be true.

OORT: Coming back home, it turned out I was a different person, mathematically. Without being able to point exactly what I learned; this was *learning by osmosis*. You have two biological cells. If one cell that has a lot of salt and the other cell has no salt, then the salt will leak through the boundary between the two cells. That's osmosis. That's the way I learned mathematics. There was a lot of mathematical knowledge outside and there was no mathematical knowledge in my head but it leaked through without really being noticed. Some French students were nasty to me. Once we were sitting in a cafe and I asked a question. The person I asked the question looked at me and his eyes said "you are such a stupid and dumb person how can you ask such a stupid question". Without giving an answer he turned around and walked away. Later I found out that my question was perfectly reasonable, and the answer was still unknown.

Another time I was in Paris and I was considering another problem. I thought about it and I asked one of the French people that 'well, how do you do this?' Then he said "Oh, that's known." I replied: "don't think so." I explained to him what the gap was and where we didn't know what's going on, and what I thought about it. Then he said "Oh oh, is that so?" and walked away. Next week, I saw him in the same seminar and he was in the opposite side of that room and avoided any contact. Two weeks later, he produced a paper where my question was in there and he started solving it, without any reference to our conversation, nor to my suggestions made. That was a shock to me. I mean this was one of the big people. That was the Paris atmosphere. Serre had been given a lecture the year before. I asked the student "can I please borrow your notes?" He said "yes, yes" but never brought them. The reason being that if he would have given to me those notes and suppose I would understand them and prove something which he couldn't prove.... He avoided giving any information, any help, to me.

CHAI: Well, some people are competitive.

OORT: Very competitive. Thank you very much Ching-Li, that's the word. That was my Paris time. Serre was supportive and nice but the atmosphere of the colleagues was not very good. I was very lucky. I did prove a theorem that was a small epsilon in the whole theory, but at least it was something. It turned out Grothendieck who thought he had this part, didn't have it and needed my little result for his approach to the final result in his theorem of constructing the Picard scheme. At least I contributed something. I have been fortunate to have Andreotti and Serre as my true PhD advisors. That was my Paris time and my learning by osmosis. Perhaps it's good to have another topic and that's teaching.

Teaching

CHAI: Yes, I think you are a truly remarkable teacher. I mean just on the score of teachers I can't think of many people approaching this level.

OORT: Thank you very much. When I was a student, I studied mathematics because I wanted to become a teacher. Actually I was a teacher for some time, substituting somebody who left and I was asked to take over. It was my good luck that I finally could do research, where I can combine research and teaching. Let me tell you one of the things I have always been doing in my freshman course on algebra. I ask students "Do you know the method of Feynman? Well, I will explain". Let me first tell you the story of Feynman going to Japan. It's in his book "*Surely you're joking, Mr. Feynman!: Adventures of a curious character.*" In Japan he was asking all kind of questions; a young Japanese physicists started to explain a theory to him writing a blackboard full of equations. I can imagine these Japanese scientists: they can really write down complicated formulas, in an almost mechanically way. In the middle of this whole explanation Feynman pointed at a certain equality in a complicated formula and asked "Is this correct?". The young person got nervous and went through the whole theory and found a mistake at this point. He wondered how is it possible that somebody can find this mistake? That you do by "*Feynman's method*": you have a non-trivial example in mind which you know inside out; everything that appears on the blackboard you test against your example. As soon as the example does not fit with the formula on the blackboard, the formula is wrong, because you know your example very well.

In teaching my classes I have always said: as soon as a proof starts you should have an example in mind and test everything that I am doing with this example. I could help them to do that in the following way. If I knew in two weeks I was going to give a rather complicated proof. I would first give a simple example of that situation as an exercise and they would have to do it. Next time, I say, well, please raise hands who did the exercise. I was angry at them because there were only very few who did. Well, I will explain this example. I took long time to tell them all details of that example. They did not understand why that was important but they liked it and finally understood everything in the example. The wonderful effect of this method is that they would then understand the proof without almost any further explanation. Then the difficult proof came the week after that, and, all of a sudden, in the room, in the audience I saw twinkling eyes: of course, this we know because we have seen that exercise, we have seen that example in detail. I would also stop in the middle of another proof to say "well, method of Feynman, what example you have in mind?" Then they would wake up and they had no example in their mind. Well, let's see, does anyone of you know a group that is not commutative? No, they didn't know any. Please think of the first exercise I gave in the first hour. Then they would say "ah yes, the group six elements ...". That's very important, to test everything by examples. We will come back to that later in what I am saying.

In my teaching I always have thought that you shall believe in what people are able to do. Ask them to put their feet firm on the ground and they should understand whatever is going on. I love teaching. I really did it for many hours. I am still teaching strange courses. Every two years I teach a course for elderly people in Holland. They sign up for the course. Nobody thought this was possible in non-trivial mathematics. The organization warned me "Frans, if there are less than ten people it won't go through." But the first time there were fifty people and they loved it. It's not elementary. The first time I talked about Wiles's proof of Fermat. I gave them an idea how that's working. Explained one or two details, and then showed them the wonderful BBC documentary by Simon Singh "Fermat's Last Theorem".

CHAI: Of course they could not understand the original proof.

OORT: Of course not! But I explained to them all kind of things. I gave them also difficult proofs. I also gave them some exercises. Also, they had to go home and reproduce the proofs themselves and so on. In fact, every time I do it there would be fifty people. One or two would walk out every angrily saying "This is much too hard." The other forty-eight stay and they love it. They don't understand everything but they love it. For example, once I spend one hour on explaining the topology of elliptic curve. Really I was producing the Riemann surface with a map of degree two branching at four points over a sphere and cutting it up and so on. They understood everything. And then they understood the donut-figure appearing in this wonderful BBC documentary on Fermat's theorem. Once you have understood the topology of an elliptic curve you know, at least at this point, what is going on.

Harvard

That is about my stage of mathematics after my deception with some of the French contacts. I had the big chance in 1966 to go to Harvard. It was like coming home. People at Harvard were extremely friendly. David Mumford[15] was generous. The first week already he explained unpublished material, things I had never seen before. He explained these to me, and later he allowed me to use these in a summer school. John Tate[16] was there. The insight and ideas of John Tate were remarkable and stimulating. And he kept track of things David and I were trying to achieve: watching our struggles, and joking "it seems you are getting nowhere."

CHAI: Yes, I have always heard about these stories when Tate was younger. The John Tate I saw was certainly much more mellow.

OORT: That was wonderful. They were extremely kind. Twice David Mumford has given me something that he had started or proved or initiated himself and he gave it to me to finish; that was generous and the opposite of what I had experienced in Paris. Also there were Barry Mazur[17], Robin

[15] David Bryant Mumford (1937-), American mathematician, awarded the Fields Medal in 1974, the Wolf Prize in 2008, known for the distinguished work in algebraic geometry. Later Mumford changed his field to the theory of vision.

[16] John Torrence Tate, Jr. (1925-), American mathematician, awarded Wolf Prize in 2002/03, and the Abel prize in 2010; distinguished for many fundamental contributions in algebraic number theory, arithmetic geometry and related areas in algebraic geometry.

[17] Barry Charles Mazur (1937-), American mathematician, known for his work in number theory.

Hartshorne[18], and Raoul Bott[19] Do you know Raoul Bott?

CHAI: Yes, he is one of those people who pretend not to understand anything.

OORT: But he understood many things. I once had this wonderful experience when Raoul Bott was the chairman of a session in a conference. There was somebody explaining some algebraic theory. There was a $\mathbb{Z}_2$ appearing. Tate was in the audience and he was getting more and more unhappy. Finally, all cohomology with values in $\mathbb{Z}_2$ was finite. Then Tate was really objecting because certainly this is not true. The confusion was that $\mathbb{Z}_2$ for topologists is the group of two elements, and for number theorists of course it denotes the ring of two-adic integers, a difference in culture. Then at the end of the talk, it was unclear whether the result announced was proven or not. After a heated discussion Raoul Bott banged his fist on the table and said “Let’s vote” thus ending the discussion. It was funny.

I had personal contact, but not so much little mathematical contacts with him. But especially with David Mumford, John Tate, Barry Mazur, and Robin Hartshorne I had many discussions, and I learned a lot from therm. Once, Hironaka[20] came and gave a talk. At that moment he was not at Harvard. His talk was a bit confusing. He really managed to upset some people in this distinguished audience. Finally, I think it was Tate, who said “Hironaka, please tell us, what is that you want to prove?” Hironaka turned around, smiled in a Japanese style and said “I want to prove that I can give examples.” That was it. The courage to defend yourself and in the middle of huge ideas (he got his Fields medal a few years later), not being disturbed, just go on and even for such an audience just follow your own examples and your own idea, that was marvelous. That year 1966-1967 was extremely important to me.

John Tate had proven a beautiful theorem on the classification of certain finite group schemes, but there was a little detail missing in his proof. I sat

[18]Robin Cope Hartshorne (1938-), American algebraic geometer; wrote the influential textbook “*Algebraic Geometry*”

[19]Raoul Bott (1923-2005), Hungarian mathematician, awarded the Wolf Prize in 2000, known for numerous basic contribution to geometry in its broad sense.

[20]Heisuke Hironaka (1931-), Japanese mathematician, awarded with the Fields Medal in 1970, best known for his work in algebraic geometry: resolution of singularities.

down and by a long computation and several deformation arguments and so on, I could prove that little detail. I showed my proof to him. However, a young person in France sitting in a train to military service heard about this question and he solved just on the train this little detail, even in a more general situation, with a clear argument what I did in one month in a special case.

CHAI: I guess it's P.D. right?

OORT: Yes, of course that was Pierre Deligne[21]. That was it. It's a marvelous, clear, and short proof. My contribution in the whole story evaporated by this beautiful argument by Deligne. I wanted my name removed from the paper. However, John Tate insisted that we should publish this together. I said it was his results and my contribution was zero; but he insisted that we should publish it together. The only compromise I could reach was to put his name first and mine second, which is not alphabetical. This paper then became my most cited paper in the citation index. John Tate was not publishing very much; probably my contribution was that this paper eventually got published. That is how my mathematical life started. If you look from that young boy who wanted to play on the street instead of doing his homework, until when I came back from Harvard, after intense contacts with Andreotti, Serre and Mumford in 1959-1967, I was a completely different person mathematically (although I feel, that my personality never changed from my early childhood until now).

I like to stress one detail, that is the attitude of (most) mathematicians. I have the feeling mathematicians are all in one big family. It's very remarkable in this field that every person is allowed to ask every other person a question. I think think is not the case in say physics, where there is a hierarchy, in which young people are not allowed to bother the really established people. In mathematics that is different. When you write to Serre he will answer by return mail and give information on whatever he knows. That is a kind of living in one big family. Also parallel to that is the admiration for mathematics itself. Do you know this quote[22] by Serre:

"*Its very simple, all you have to explain is this:*

[21] Pierre Deligne (1944-), Belgian mathematician, awarded with the Fields Medal in 1978, the Wolf Prize in 2008, and the Abel Prize in 2013, best known for work on the Weil conjectures.

[22] Jean-Pierre Serre, Lettre du Collège de France, no 18 (déc. 2006); see: `http://www.abelprisen.no/binfil/download.php`, page 30, footnote 4.

other sciences seek to discover the laws that God has chosen; mathematics seeks to discover the laws which God has to obey."

I mean, mathematics is the holy grail. It's the mathematical truth, that is what you living for. We feel that mathematics is so big and we are just humble servants. That's the attitude. I think that's what really makes mathematicians behave this way. Don't you think Ching-Li?

CHAI: Yes, well at least for people like me.

OORT: Of course you always have our own experiences. Like I have told you Jean-Pierre Serre he can be very angry when somebody makes a mistake or anything like that, but the core is that he thinks mathematics is important. If you mistreat mathematics that's bad. That's something essentially what is going on. That was what I felt in Andreotti, in Serre and in many colleague-mathematicians.

Midlife

I would like to come to another phase in my life. Let's call it mid-life. I was about thirty-five and I was considering giving up mathematics. I knew exactly why: research is something you do usually in solitude. You just sit in your office and stare at a piece of paper and nothing comes out. I am a very outgoing person and therefore I really thought about giving up mathematics. I was then a professor in Amsterdam. I thought I should change my life and start doing something completely different.

CHAI: That would be a really hard decision. I mean you are a professor in Netherlands. It's different from a professor here. I don't know how many professors there were. Very few I think.

OORT: They made especially this chair for me in Amsterdam in 1967. I considered to become a professional flautist or a social worker. The reason was that I felt I had little contact with other people in my professional life. And, I was a little service to the society. I contemplated and considered this for one and a half year. I'll tell you my argument why I did not change. The argument I derived from Grothendieck, like he taught me many things, but especially I learned this from him. Grothendieck was one of the really exceptional mathematicians in the last century. He was extremely gifted for abstract thoughts and finding things by pure thought. In society, sometimes the following mistake is made: if a person is good in X, then some people think he is also good in Y, and in Z, and so on.

CHAI: Which of course is completely false.

OORT: Exactly! I remember once I read a book about Bach. One of the arguments was that he was such a superb composer that he should also be very good at other things, such as computing. However, this seems not to be true. There is a little note where he added his daily spending and the addition was just wrong. Why would he be good at adding numbers? He had a other talents (impressive, it is one of the great loves of my life). Grothendieck was a superb mathematician. In the 1970s, around that time he started considering of changing his life and changing society. He founded a movement called *Survivre et vivre* (to survive and live)[23]. That was a great idea. It was the time of Vietnam war, of the atomic bomb, and so on. We really should make revolution against these things. Grothendieck started a movement to do this. It turned out that, although he was very good at mathematics, he had zero insight in mass psychology, in economic behavior, and so on. One of his things he wanted was to disarm all countries immediately. Everyone knows this is not possible. Of course you can try slowly starting reducing armies. There is no way that you can abolish weapons immediately. However he had an idea that was possible. After one year, his movement had 28 members. That was the great Grothendieck who started this thing and he had no idea where to go[24]. In Nice at the International Congress of Mathematicians (ICM, 1970) he was there and he organized a big meeting. You were not there Ching-Li.

CHAI: No, I was born too late.

OORT: It was in a large, round amphitheater. Grothendieck would give a session on his movement. The impetus was there. We were enthusiastic. The room was packed and people thought now it's going to happen. He had the prestige, he could achieve great things. Grothendieck walked up, looked around and said "Does anybody have an idea?" I was amazed. Grothendieck who in mathematics always had a very clear cut idea now asking for our ideas. Already in 1958 he had a vision on what he was going to do and he had completely laid out new foundations of algebraic

[23]A political group aimed at pacifistic and ecological goals, founded in 1970.

[24]Johan Cruijff once said: *Speed is often confused with insight. When I start running earlier than the others, I appear faster. I hate people start running without knowing where to go.*

geometry. And here in 1970, but not on mathematics, he asks "Does anyone have an idea?" The session ended in a complete disaster. People started shouting and arguing. There was zero outcome. That taught me a lot. That taught me that if you are good at one thing it does not mean you are good at something else. I could do something right in mathematics. It's not so clear as a flautist I could also perform well (well, it was clear to me: no). After some time, I decided to spend more time on the things which I really love to do but to stay in mathematics. Since then I am much happier, combining my fascination for mathematics with other things I love to do.

How to do mathematics

Next to this, I would like to say something about the way you do mathematics. It has been for me a fascinating topic and I am thinking again and again about it. I have been very fortunate to have seen and being aware of the two extremes. One extreme is Grothendieck and the other extreme, if I may say so, is the way Mumford and Serre are in mathematics. Let me explain. You can do mathematics by just doing the most abstract, pure way of feeling things and then trying to produce ideas and theories. For some people, this is the ideal way of doing mathematics. In that way of thinking, everything else is just bad and shall not be done, a waste of time. Of course Grothendieck was one of the people who was able to do that. He was magnificent in producing grand ideas. He considered his mathematical material in an abstract way. If he was looking for something, and there is an abstract, pure-thought method, I am rather sure that he would have found it. If there is anything you want to know and Grothendieck considered it and did not find it, then it is plausible there is no pure-thought argument to do it. Yuri Manin[25] once said "*I see the process of mathematical creation as a kind of recognizing a preexisting pattern.*"[26] When you study something, according to Manin, the essence of the process of mathematical creation is the recognition of a "preexisting pattern". You just look around, study a question and the only thing you have to do is to look for is a pattern that already exists and that you have to find. Yes, in many cases I feel this is essential for mathematicians. This is the one extreme way of doing mathematics

[25] Yuri Ivanovitch Manin (1937-), Russian/German mathematician, known for his work in algebraic geometry and diophantine geometry.

[26] The Berlin Intelligencer, 1998, pp. 16–19

Let me tell you, when I came back from Paris, I had my first job in Amsterdam. The first thing I did was I started a seminar and the participants were to discuss examples. Everyone was invited to find a nice, good and difficult example on whatever you are thinking of. That was my medicine against this complete abstract Grothendieck-way of doing things. That is one extreme: the road of pure, abstract thoughts. The other extreme is everything you want to find you first start by making an inventory on what is known. You study examples. You try to find various directions. That's like going through the mud, a mud of equations and examples. My view on doing mathematics really is a combination of both. After some years I became associate professor in Amsterdam; you can give a lecture to a general audience, introducing yourself. The title of my speech was "*Vlijt, visie, verificatie*" in Dutch (diligence, vision, verification). The first word means *diligence*, the second word is *discovery* or *finding new idea* and the third word means *verifying details*. The combination of these three is the way to do mathematics, I think.

Diligence is that you first look for patterns, for examples, for all the data, for work done before. Andreotti once said "Frans, you first must make all the mistakes all people before you made before you can make any progress." That is kind of true. Diligence: you work through a lot of details. Then it may happen that if you see all these examples, all of a sudden, click, you have an idea. You have a discovery. A famous example is Kepler[27] who was studying the planets and their orbits around the sun; he was teaching, talking about the five regular solids, such a beautiful mathematical topic. It occurred to him, in a flash, that there are five regular solids and five planets. This cannot be a coincidence. If you put spheres around and inscribing these solids, you get various spheres, and the radii of these spheres could be the radii of the planets going around the sun: Kepler's Platonic solid model of the Solar system from Mysterium Cosmographicum (1600). The diligence was that he thought about this problem for a long time. The vision was noting this possibility of corresponding structures, the discovery, of *the analogy between the five solids* (octahedron, icosahedron, dodecahedron, tetrahedron, cube) *and the five planets* (Mercury, Venus, Earth, Mars, Jupiter, and Saturn). The "only thing he had to do" was to verify the details. He sat down and wrote down

[27] Johannes Kepler (1571-1630), German mathematician, astronomer and astrologer, a key figure in the 17th century scientific revolution, best known for his eponymous laws of planetary motion.

the mathematical formulas. It turned out his initial idea was not correct. If you take the laws of gravity, and describe an orbit in general you do not get a circle, but an ellipse. In this way he found his famous laws describing the orbit of an object (a planet) moving in an orbit around a center of gravity (the sun). Much later it was found out there were not five but nine planets. His initial idea, for many reasons, was wrong, but it triggered Kepler to perform the necessary verification, and to find the truth. This way of thinking seems very characteristic for scientific research. Shimura[28] once said about Taniyama[29] : "*he made mistakes, but mistakes in the good direction*".

After step one, diligence, in mathematical activity step two is discovery: the quantum jump, the insight. Step three is checking details of what you expect to be true. That is something which is really important. I was very much impressed by Serre who knew so many examples inside out. Much later when I read correspondence[30] between Grothendieck and Serre I saw the influence he had on Grothendieck by communicating non-trivial examples. Also see correspondence between Grothendiek and Mumford[31].

In the same time, I was impressed by David Mumford who knew his abstract theory extremely well on the one hand, on the other hand, he was

[28]Goro Shimura(1930-), Japanese mathematician writing important work in the theory of complex multiplication and modular forms.

[29]YutakaTaniyama (1927-1958); Japanese mathematician, working on automorphic properties of L-functions of elliptic curves over any number field; on November 17, 1958 he wrote: *Until yesterday I had no definite intention of killing myself. But more than a few must have noticed that lately I have been tired both physically and mentally. As to the cause of my suicide, I don't quite understand it myself, but it is not the result of a particular incident, nor of a specific matter. Merely may I say, I am in the frame of mind that I lost confidence in my future. There may be someone to whom my suicide will be troubling or a blow to a certain degree. I sincerely hope that this incident will cast no dark shadow over the future of that person. At any rate, I cannot deny that this is a kind of betrayal, but please excuse it as my last act in my own way, as I have been doing my own way all my life.*

[30]Published by the Société Mathématique de France in 2001, edited by P. Colmez and J-P. Serre; translated and edited as a bilingual version by the American Mathematical Society in 2003. For reviews see
http://www.math.jussieu.fr/~leila/corr.pdf
http://www.ams.org/notices/200309/rev-raynaud.pdf

[31]David Mumford–Selected Papers, Volume II: On algebraic geometry, including correspondence with Grothendieck; Edited by Ching-Li Chai, Amnon Neeman, and Takahiro Shiota Springer, July 2010. For a review, see:
http://www.ams.org/notices/201302/rnoti-p214.pdf

not afraid of doing computations. I will tell you a story. There was an international congress in Stockholm (ICM 1962). People were discussing algebraic geometry. There was a question handed down to us from the old Italian geometry which (in modern technical language) asks is the moduli space of certain space curves reduced? (We are now able to formulate this with the help of scheme theory.) It was an open problem. Nobody knew the answer and it was already open for seventy years or something like that. On the next day, one of the participants had an idea. It was a young person. He said that the answer is no, for space curves of the degree fourteen. Everybody was amazed. People asked why fourteen. He said that he computed through the cases of space curves of lower degree. If you see his final paper it's really an amazing computation. Long exact sequences and you have to take the maximal dimension of this and minimal of that. Through this long computation he finally proved that the moduli space considered has dimension at most fifty-six and the tangent space has dimension at least fifty-seven[32]. It was the young Mumford who produced this example. This is something almost unthinkable. Having the courage to go through such computations and having the insight that you shall look at that point. Then the perseverance to go through that complicated string of ideas; it is impressive to see the computation in that paper. Don't you think so?

CHAI: He's a smart guy as we all know.

OORT: Yes. Let me first come back to my hesitation to go on in mathematics. Later I will come back to Mumford. At that time, I was seeing an interview by Jan Oort[33]. He was an uncle of mine, an astronomer. He was a nice person and very gentle. In an interview, there were three aspects . First of all, he was explaining that by pure thought you obtain the key to a lot of things. The interviewer was not very interested in essential things. The interview was getting nasty and he wanted to see all medals. Reluctantly Jan Oort showed one or two of the medals but wanted to explain on what he was really so excited about. But the interviewer wanted then to end this conversation. However Jan Oort said "No, no, I still have one more topic." He said "You see, my wife has always been

[32]D. Mumford – *Further pathologies in algebraic geometry.* American Journal of Mathematics **84** (1962) 642–648.

[33]Jan Hendrik Oort (1900-1992) was a prolific Dutch astronomer. He made many important contributions in the field of astronomy and was a pioneer in the field of radio astronomy

extremely nice to our visitors." They lived in the observatory in Leiden and they had lots of international visitors. The atmosphere of that house was really remarkable and his wife was the center of that. Those three aspects of this person completely fascinated me: the total non-respect for medals, the appreciation for his family atmosphere and especially the way he talked about this pure thought and scientific interests. For me it came at the right moment: you "are allowed" to contemplate about questions and you don't need to do applicable things. Be curious and pursue your own thoughts. That was very much parallel to what I thought. Later in an interview people asked me what you think is interesting. I said "Well, it's fascinating that you can make progress by pure thought." Jan Oort discovered the structure of the galaxy. People thought about it but nobody knew for sure because you just see the milky way, how do you know what is the structure of that. You cannot look perpendicular to it. Then by pure thought Jan Oort proved that experiments, models, and mathematical formulas supported the view that the galaxy is a spiral and it is not moving at same speed everywhere but with various speed at various parts. That was his pure thought after ideas and experiments of other people.

Let me come back to David Mumford. It is nice to see if you plant a little seed then a flower will come up. Sometimes you don't recognize the flower from the seed you put in. This happens to me once in a very peculiar way. David Mumford approached a question and he had an idea about liftability of abelian varieties to characteristic zero.

CHAI: That's what I was guessing.

OORT: That was a marvelous idea. That was something of completely non-Grothendieck type. To this problem you can have a general approach. That solves the question in certain cases but you get stuck at other cases. I have seen Grothendieck doing this several times in his life. You have a problem and what you do you make a "machine" (that is in my terminology). You construct a big machine, you feed your a problem into the machine, and you wait. If a good answer comes out you are happy. If nothing comes out you get stuck. I have seen Grothendieck abolishing a questions because the machine got stuck and he gave up (or he would generalize the problem to a more difficult one; sometimes that works well). At this particular problem, Mumford had a different approach. You first observe that in certain cases you can solve the problem. That's kind of mechanical. In that case the general machine provides what you want. Once you understand the idea and the patterns behind it, this is clear. But all other cases, you don't know how to handle. Instead of giving up Mumford sits down and starts thinking, can I change a bad situation into a good situation. But that change is non-canonical. It's not automatic. You really have to do something. You have to understand examples. You have to role up your sleeves and go on. He made a preliminary computation and he was convinced this was correct. He gave that idea both to Peter Norman[34] and to me. When I corresponded with Mumford about this problem he said "Well, Frans I am rather shaky about my idea. I don't know whether it's true." Peter Norman and I started working on this wonderful idea by David Mumford. The theory of displays, invented by Mumford, is marvelous. It allows you to perform the necessary computations. You have to make your hands dirty, you have to do something. In the final proof, suggested by Mumford, we indeed showed that bad situations can be deformed into good situations, where the general idea (the "machine") would do the rest of the work. It is hard mathematics. This proof is a combination of clever ideas, difficult computations, and a general approach resulting in a theorem that, I think, is non-trivial.

CHAI: I think still now a days it shows that the singularity of $\mathcal{A}_g$ with arbitrary polarization-degree is a local complete intersection.

OORT: Yes! That was not at all clear before. Don't you think so?

CHAI: I think still for others like Shimura[35], not many other cases are known.

OORT: Now I come to the seed and the flower. Much later I started working on a conjecture by Grothendieck made in 1970. He tried to solve a certain problem which he thought was reasonable. The pattern is clear. You can write down a general theory and you can make a machine. You feed a problem into the machine nothing reasonable comes out because it's much too complicated. I started working on that. The problem clearly is the key to understanding this structure. However, a solution seems not to come by pure thought. It took me a long time and then I found a general pattern which would solve the problem in very nice cases, in special cases. I

[34] Peter Norman, American mathematician, with interests in abelian varieties and in theta functions

[35] Goro Shimura (1930-), Japanese mathematician, best known for his influence in number theory.

was really happy and it took me a long time to really formulate the correct approach. Then I had the idea that this general pattern would give a proof in all cases. I asked one of my former student Hendrik Lenstra[36] to help me finding a proof for the detail I needed. That did not work out well, he gave a counter-example to my specific question (I was grateful), and that was the end of my idea. I could start all over again. And, sometimes I considered that perhaps I would never know the answer to this question.

Now I come to your question. Why don't you give up? Well, I like the problem very much. It was very intriguing. Of course it was a little flattering if you could prove something Grothendieck could not prove but that was not the main point. I started all over again, in my way, going through examples. I took a sabbatical leave from my job. I just served as a Dean of the mathematics department and I got some time off. I went to Princeton where Johan de Jong[37] was working. Very patiently he listened to me every day for a long period of time to my new computations and my new examples. I went through many examples and I had no idea where to start and what to do but I had the hope to see a general pattern. On the last day of my stay, Johan was away because he moved to MIT. All of a sudden I had an idea (and immediately I thought this was the key, this should be correct). If I can prove that then everything is okay. I went to Nick Katz[38] asking whether this would seem possible. My idea is what we now call the purity theorem. You have a set of sub-varieties each given by many equations (and these were constructed by Grothendieck and by Katz). The number of such equations is large; however my idea was that each time one variety was contained in the next one, the codimension would be one. This seemed not a good idea, because you need in general more than one equation to define that subvariety. I went to Boston and asked Johan do you still have time for me. He expected again examples were coming but he had patience with me. I explained to him my idea and he was flabbergasted. He said "Yes, of course Frans. This is the key to everything. But shouldn't we first check this in an example to see whether

[36] Hendrik Willem Lenstra, Jr. (1949-) a Dutch mathematician, working in number theory, computational number theory and is well known as the discoverer of the elliptic curve factorization method and a co-discoverer of the LenstraLenstraLovász lattice basis reduction algorithm.

[37] Aise Johan de Jong (1966-) a Dutch algebraic geometer, working at Columba University at present. In 2000 he received the Cole prize of the AMS.

[38] Nicholas Michael Katz (1943-), American mathematician, working in the fields of algebraic geometry.

it really works?" I said "Okay" and I started. "Can I watch you while doing the computation?" "Of course." I went through one example and it came out exactly as we wanted. Then we started working on the general situation. I had an idea of a proof but in my plan certain details were lacking. However we were convinced this should work, and after one month we found the missing details.

That idea of this codimension-one phenomenon seemed new; it was the start to a whole set of ideas. Here you see the diligence. You first know go through examples and you know the landscape. You know details. Then you have an idea (the vision). If you prove this is correct (the verification), you are sure and everything works fine. Much later I realized that I was following the same pattern as Mumford had in this other proof. Namely that you have problem; in certain ("nice") cases you can prove what you want; then you change the bad ones into the good one and for the good ones you already know what you want by general theory. This was really one of the beautiful aspects of my mathematical career. I was working on this for seven years. I would have been equally happy even if I could not prove it. That's okay. Why not! In the middle of that I thought perhaps I die before I see it. Mathematics is beautiful and difficult. There are many things you don't know. I am just too stupid for it. That's perfect as it is. But of course finding a thing like this proof, that was great. A lot of things have evolved from that. It's not that I am very proud of it, but to unravel this beauty is so rewarding, and I am also happy seeing other people using this result.

One of the details is a complicated proof. To my surprise, nobody has found a better proof. That is still something which no one else knows how to do it in an other way. That's strange. So many times when you find something, then after a few years a much better proof comes out. This week I will give a proof of the Weil conjecture for abelian variety that André Weil proved ; he wrote three books to prove this; now we have two lemmas (general structure, one based on ideas in a letter by Serre) to prove this result. That's quite a general pattern: you find something in mathematics and later people find much more clever ways of doing it once the problem becomes an exercise. The little seed Mumford planted in my brain came out. At the moment I was working on it I did not realize I was following this pattern by Mumford, trying to first change a general situation to a much better one, then the much better one having a general solution. But later I realized I had followed this pattern by Mumford also in my proof of this Grothendieck conjecture. That explains my choice between the two extremes. One extreme is general theory, building a machine and the other extreme is doing examples. Sometimes you have to work hard on details to obtain results. In 1966 Grothendieck wrote to Mumford: " .. I found it kind of astonishing that you should be obliged to dive so deep and so far in order to to prove a theorem whose statement looks so simple-minded" I think the beautiful reality, and the real beauty of mathematics is that you sometimes have to "dive so deep and so far".

For a long period of time, people thought very low of my mathematics. I was going through all kind of examples, but that is not the way a decent mathematician shall behave. Also I was working in the field with really obstacles for progress. Yuri Manin wrote his PhD thesis in 1966. He has a beautiful way of describing certain objects. It gives a nice description of large classes of these (abelian varieties in positive characteristic). However in his theory there is no description what happens if you "go to the boundary" (meaning in this cases changing the p-structure). That aspect was still missing in his theory. It turned out to be difficult. This conjecture by Grothendieck pinned down what you have to know before you can really start understanding that behavior. The beginning was in 1966 and 1970. It was not until the late 90's that we finally found a good approach to this, to such structures.

In that period of time I was doing all these computations and all these structures. Well, in all the structures I needed perhaps some more clever person would have done this somewhat faster. I think developing all the structures was necessary. Especially one structure I was extremely fond of was the theory of foliation. I will tell you a story. Johan de Jong once said to me "Oh Frans, I would have loved to have that theory of foliations in my name." I said "Johan, that's okay. Why don't we swap? You get my foliations." and he looked at me "and I will get your alterations." 'No, no, no.' Alterations is a powerful technique Johan has developed and that can be used instead of resolution of singularities.

My mind is not suited for the Grothendieck type mathematics. I have been very much inspired by Serre and later by Mumford who know their general theory quite well but they also know specific and difficult examples, and they are willing to allow an approach based on specific situations (next to general theory). They have inspired me. Their inspiration of having examples and building up from the bottom has been quite important for me[39].

Colleagues and cooperation

I have three more aspects which I would like to discuss. One is as you understood, for me, doing mathematics partly is very social affair. I prefer to do it with other people rather than sitting at my office. That reflects itself in the number of people I wrote papers together, with many collaborators. I enjoy in discussing with other people.

CHAI: Which must be true for instance in the paper with Tate and a paper with Mumford.

OORT: As I told you the paper with John Tate my contribution was zero.

CHAI: Well, not really.

OORT: Finally what appeared in the paper no argument in the paper directly comes from me. I know another paper where my contribution was 95 percent. We only had a discussion and I had the final idea and wrote it down. After that the other person said shall we put your name to it. He agreed. That's happening. Of course, in good cooperation finally you don't anymore know who had the starting idea and who had the technical idea and so on. That's something marvelous. To all these thirty-one people I have very good memories. That's really very nice. Of course I think Ching-Li is one of my collaborators; there has been a very intense way of working together and we are complementing each other. In methods of working Ching-Li is much better technically in carrying out ideas. I am very happy about that. That's one of the things very dear to me. All these people writing papers together, all cooperation I enjoyed.

Ideas, conjectures and expectations

Then I would say something about my way of doing mathematics lately. Like I told you one of my former student said "Well Frans you are not very

[39]Serre wrote: " ... obtaining even good results 'the wrong way' using clever tricks to get around deep theoretical obstacles could infuriate Grothendieck." See the correspondence Grothendieck-Serre, p.18.

good technically but you have got intuition." In 1995 my former students organized a conference because I turned 60 at that moment. It was a very nice conference. It was nice because quite soon people forgot completely why they were having a conference but they were enjoying mathematics. I was not supposed to give a talk, but I thought there should be some contribution from my side. Secretly I made a paper, a preprint with some of my mathematical ideas and especially open questions. It's called "*Some questions in algebraic geometry*". Originally there were 23 of such question. Then I thought "no, that is not possible."

CHAI: 23 is not a bad number.

OORT: Indeed, but Hilbert in 1900 presented 23 problems[40]. Presenting 23 problems now is kind of pedantic and I crossed out a few. Those questions turned out to be crystallization points of ideas. Recently almost all of them have been proved and most of the ideas proved to be correctly. Many times I see people starting on it. New ideas come up. New roads are taken. Some proofs are given in a different way than I expected. There was one conjecture about complete subvarieties of a moduli space, I had an almost proof for it and I am still working on that "almost proof". But I get stuck. Then other people took it up and gave a proof I don't understand at all but they really prove it. All these ideas, all these conjectures were signposts for people where to go and where to start working. I am very happy to see these ideas are fundamental. But also you see the roots of these ideas.

One of these conjectures starts from a paper by Ching-Li in 1995. That paper was submitted and I was very fortunate to be able to read a preliminary version. It was parallel to ideas I had myself. Ching-Li had this beautiful theorem and I had a conjecture about a more general behavior. Since then we work on it and we are now generalizing Ching-Li's theorem, finishing the proof of my conjecture. That is a typical example of an idea coming up in one person and almost the same idea coming up from a different person. A combination of these two can create something new. Several of these conjectures have been proved now. And in the mean time more questions have come up.

[40]German mathematician David Hilbert published a list of 23 problems in mathematics that were unsolved at the time and several of them were very influential for 20th century mathematics.

Mathematicians: one big family

I would like to come back to my terminology of the big family. Mathematicians all over the world are in close contact and ideas flow back and forward quite freely. I have several e-mails at least once a week of some young person writing to me very politely. I am working on this and this, could you please tell me where to look for and how do you do this and this. I take time answering these. Last month (November 2012) I had two visitors. One was a Japanese and one was Iranian PhD student; both came to me with their ideas and questions. That kind of structure, that kind of atmosphere is extraordinary. Another mathematician feels like someone in your family, who you know already and you can discuss with this person whatever you like. You have interviewed Professor Mori[41]. Let me tell you two stories about Mori[42]. In 1983 we both took the Shinkansen, the bullet train, from Tokyo. He was going back to Nagoya and I was going back to Kyoto. We were in a very good mood. I told him that I had seen a preliminary version of the paper in which Faltings[43] was proving three famous conjectures[44]. Mori said "That's interesting. Can you tell me the proof."' I told him the rather complicated proof. After an hour and a half the train stopped. The first stop was Nagoya station. He said "Thanks very much Frans, I understood everything" (remarkable).

CHAI: That was 1983.

OORT: 1983, yes, that's remarkable. You know each other and all of a sudden you sit together in a train and you explain something and the other understands everything. The other anecdote is also nice. I was invited to Nagoya to give a talk. Johan and I had written a paper on hyperelliptic

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[41] Mathmedia, Institute of Math., Academia Sinica, **33**, 3–18.

[42] Shigefumi Mori (1951) is a Japanese mathematician, known for his work in algebraic geometry, particularly in relation to the classification of three-folds. He was awarded the Fields Medal in 1990 at the International Congress of Mathematicians.

[43] Gerd Faltings (1954) is a German mathematician known for his work in arithmetic algebraic geometry. He was awarded the Fields Medal in 1986 for proving the Mordell conjecture.

[44] G. Faltings – *Endlichkeitssätze für abelsche Varietäten über Zahlkörpern.* Invent. Math. **73** (1983), 349–366.

curves in abelian varieties[45] in which we need a trick[46][47] invented by Mori. Just before the talk, I went to Mori and I said "in one stage of my talk, you will be very unhappy but please don't protest." "Okay. Okay." He said. He is a funny guy. I explained the Mori trick in my talk and everybody said 'yes, yes.' But I omitted one of the most important details, one of the conditions. I looked at Mori and he understood that this was the moment he should not protest; he just smiled and understood. I went on with my talk, with the wrong version of his trick. From that wrong version, I deduced that moduli schemes of abelian varieties in characteristic zero contain rational curves. Then people got nervous because that is known to be false. They looked at me "are you really proving that?" Well, I use Mori's trick and we obtain this conclusion. But that is wrong so somewhere I am missing something. Then I confessed that I have forgotten, on purpose, one of the crucial conditions in the Mori trick, namely that the class of the rational curves should be bounded. Then I included that condition and we saw that wrong conclusions didn't follow at all, and everybody was happy again. Then I gave a proof of this trick and I used it in my talk. But the fun was Mori was sitting there and playing this game with me and not protesting. People in the audience saw this essential detail cannot be omitted, and they understood why it was also important in our proof I was presenting.

Such are nice adventures in the life of a mathematician Let me tell you one other story. I really like to insert this one. In Bonn there is a research institute called the Max-Planck-Institut für Mathematik and is founded basically by Friedrich Hirzebruch[48] who died few months ago[49]. He was a fantastic mathematician. He was a nice person. He was instrumental in uniting the mathematicians of East and West Germany after the wall came down. One of the things he did was organizing the Arbeitstagung

[45] F. Oort & J. de Jong – *Hyperelliptic curves in abelian varieties.* Published in: "Manin's Festschrift", Journ. Math. Sciences **82** (1996), 141–166.

[46] S. Mori – *Projective manifolds with ample tangent bundles.* Ann. Math. **110** (1979), 593–606.

[47] Serre once wrote to me: "*About theorems being proved by general methods or by tricks. The word trick is pejorative. But one should keep in mind that a trick in year N often becomes a theory in year N + 20.*" I think this applies to "Mori's trick".

[48] Friedrich Ernst Peter Hirzebruch (1927-2012), German mathematician, awarded the Wolf Prize in 1988, working in fields of topology, complex manifolds and algebraic geometry, and a leading figure in his generation.

[49] See the obituary in the New York Times, June 10, 2012. Atiyah recalled: " ... he would lead you along and you didnt know where you were going, and then suddenly at the end of the lecture a beautiful thing would emerge. It was a work of art, a little theatrical production that gave the appearance of being ordinary, but they were very carefully planned."

(working meeting) every year. It is a conference completely different from anything else I can imagine. The structure is as follows. There is no program. The first talk is decided. That was usually Atiyah[50]. Before the first talk started, Hirzebruch would go up to the board and would ask for suggestions. Everybody could make a suggestion for a speaker and a subject. There were certain conditions. You could not propose yourself. The person proposing should also propose the title of the talk. Then by some process we would see the most favorable to the audience and those talks were chosen for the next two days. That was really a master piece of Hirzebruch. Have you every been there, Ching-Li?

CHAI: Yes, in 1995.

OORT: Of course. It was fantastic how Hirzebruch performed the program discussion, a remarkable process. In a large audience how do you make a program? Hirzebruch was the master. First of all he had a superb view on mathematics. Then of course his network was very good so he knew already what's going on and what were the hot topics. On the other hand, if there was something completely new that he didn't know and it was proposed he would listen and put it on the list of proposals. This was always working well in an extremely friendly and good atmosphere except for one day. There was a very very nasty colleague of Hirzebruch who was really making his life impossible. I know that Hirzebruch wanted to resign from his position some years before because this colleague was so nasty. With his letter in his hand he walked to the mailbox and somebody met him on the street and asked what are you doing. He said I am going to resign; luckily there he was then talked out of this decision. But this colleague was still impossible. At one of these meetings, discussions for the program, this person shouted "this person and that person should give a talk" and Hirzebruch was kind enough to write these names down. He did choose one or two of them. Two days after, the next program discussion came up, this person again shouted and had a lot of arguments and a lot of names. It was impossible to make remain reasonable. This person said "we shall have local people." Hirzebruch replied "but, this is a local person" although it was someone often visiting Bonn. It was a joke but this person didn't give in. It was really getting nasty. We didn't like it. What would you do? Hirzebruch, as always gently robbed his hands, and said "I

[50] Sir Michael Francis Atiyah (1929-), British mathematician, awarded the Fields Medal in 1966, specializing in geometry.

have a good solution to this problem. I think we shall have a democratic decision." I thought "oh no, something like ninety people in the audience. Democratic decision! What do you think you are going to do?" He said "We are going to vote on this but only those people who have attended all previous Arbeitstagung meetings are allowed to vote." Except Hirzebruch himself that was only Atiyah and nobody else. Almost all of us laughed and thus ending that nasty session. Afterwards Atiyah and Hirzebruch were sitting at a table and they made the program.

I thought this a case of elegance and friendliness based on real scientific values. You knew he was generous to other people. He would never block other people or obstruct other people. He had a good insight in general mathematics. If somebody really insisted on certain topics he would give in and then perhaps something could come out. Intelligence and friendliness based on really scientific values. I have taken example of that and I think that's the way you should behave.

I had many students. I have been especially privileged by really good people coming. I have explained in the beginning the spirit of the way Johan Feltkamp taught me playing my flute. That has been my guiding principle for my students. I have never forced my students in any topics they didn't want. I myself gave back my problem the next day already. I think everybody has his or her own taste. It's almost impossible to do mathematics on something not only you don't like that but which also does not fit into your system. Everybody has a taste, an immediate contact with mathematics. Once there was an undergraduate student in Amsterdam who did an exam with me. This is Rob Tijdeman[51]. He is a gifted person. But the algebraic theory we discussed in this little exam wasn't at all his taste. He didn't feel at home with it but as soon as he is doing analytic number theory he knew exactly what he had to do, and he proved beautiful results. That's the same with me and that's the same with my students. They all have all kind of tastes. They ask for a topic. Then I would always give them a topic which was in their line. Once there was a student who wanted to do a master thesis with me. I think he liked my style of doing mathematics; he came to my office and asked for a topic. I saw in his eyes that he was very afraid if it would be something about schemes, or some other abstract topic. With him I did the same I usually did in such a situation. I gave three different subjects. The condition was: on each paper

[51] Robert Tijdeman (1943) is a Dutch mathematician, specializing in number theory.

he was only allowed to spend not more than one morning on it. Even if it was a very complicated paper. Then he should come back very soon and then tell me which of the three topics he would like. I gave him one problem in coding theory, I expected that he would appreciate that, and two other papers. The other two topics he didn't like at all but the coding theory he liked immediately, and he became one of the big specialist in coding theory.

That was my approach to guiding students. Let them choose their own topic, follow their own inclinations and feelings. Then of course you have to guide them. One of the main thing I did with students is when you do mathematical research: maybe you lose time in doing things that seem to make no sense, doing an idea here, doing a computation there, having a vague idea and so on. Once I had an idea taking my bath in the morning. There is a certain question we could not solve. But all of a sudden I saw that for prime numbers bigger than 1400 it would work. The idea only came to me because I had no piece of paper, no direct access to computers or whatever. Just my unobstructed thoughts. Professor Katsura[52] and I then worked on, we saw the idea did work out well, and we proved our theorem for all primes at least 11.

I would ask my students to have ideas, just at random, trying all kinds of directions, going here and there, going to the library. Once I had a student who went to the library and he took the wrong book from the shelf. That was Bert van Geemen[53]. He wanted to have a look at a certain book but just by chance took the book next to it, opened it and thought it was wonderful. He started working on that topic. Such a coincidence can make you progress. However after some time, you have to sit down. Then I would say to students "now you have done a lot of running around and so on; please take a piece of paper and write down very precisely what's the question; what do you know." Then it turns out that some questions are complete nonsense. As soon as you formulate the question precisely you see that one of them is a phantasy which is nonsense. This going back and forth, sometimes roaming around and developing ideas just at random, then being very precise on what you are doing can be fruitful. That is what I think one of the things the thesis advisor has to do making that process very precise. Once I had a student who wanted to do a much too difficult problem. I said "this is much too difficult. Would you ever make any progress on that?" After three years, I said "Now listen, this is going nowhere. I give you

[52]Toshiyuki Katsura, Japanese mathematician specialized in algebraic geometry.
[53]Lambertus N. M. van Geemen, Dutch algebraic geometer, now working in Milano.

one month and within one month you decide on a different topic which we you really finish." I asked for one more year of grant. He obtained it and finally he finished. I am absolutely sure without my intervention he would gone on with that difficult topic[54]. Mainly it is hard because we even don't know where to start. Do you want to do computations? Do you want to apply theory insight? Of course he tried a lot of things. We did a lot of discussions. It was much too hard (and up to now this is still an unsolved problem). I am glad he was willing to change his topic, and I was happy to see him finish a nice piece of PhD research. Basically the choice for topics would be their own. That would be their taste. But it was my task to have structure in their research. I am proud that none of my students have failed.

CHAI: Fail in the sense?

OORT: Well, quite often I see that somebody comes and starts a PhD project then turns away, doesn't finish the project, doesn't finish the research or what so ever. That never happened with any of my students.

CHAI: That's remarkable!

OORT: One of the ingredients was that I didn't accept students if I didn't know them well. They were supposed to have the creativity and ability to perform well. Once an undergraduate came to me and wanted to be my graduate student. I asked do you know anything about algebraic geometry? Well, I want to learn that. I said okay, here is a theorem in algebraic geometry, please try to understand this structure and these ideas. If you can understand these details then we can go ahead. I gave him three different proofs of the Hurwitz theorem[55], analytically, algebraically, and the topological proof. But he didn't get anywhere. He thought this was hard. However I think this something basic in algebraic geometry and not very difficult. I am sure if I would accept him as a PhD student this would not be the right decision, and it could have ruined his life. I had another student who is now a very good friend of mine who worked for a master degree. He wanted to be my PhD student. I gave him a problem for his master degree and I knew how to solve it. He was good in understanding and reproducing mathematics. He had top grades. However, as soon as he

[54] Is the rank of $\mathbf{Q}$-rational points on elliptic curves over $\mathbf{Q}$ bounded or unbounded?

[55] Or, the Riemann-Hurwitz theorem; this relates in a covering of Riemann surfaces the genera of the two curves involved, and the ramification indices.

had to do something creative he could not do it. He would come to my office saying he didn't see where to start. He didn't know what to do. He had a lot of knowledge, but no new insight. At the end of that period he was convinced he was not able to do any research in mathematics. He changed his life and he is very happy now. He is now writing a PhD on something completely different. He is very happy that he found out beforehand that he was not the right person to do active research in mathematics. I had several such cases where people were thinking well this is a good thing to do. Then beforehand we found out that was not a good thing to do and the student would agree, and they would look for something else. I am very happy with that. With several of them I still have very close contact. That was the family. I think I shall conclude on this topic and I thank Ching-Li for all the nice years we had; still we hope to have more nice years together.

CHAI: I see that you would like to conclude but several people have talked to me, wanted me to ask this question which may not be well formulated and I don't want you to feel being ambushed.

OORT: No, no, not at all.

CHAI: First of all you know that there are some historical coincidence between Taiwan and the Dutch. Also there are similarities either in geographical sense, i.e. close to the sea, as well as both places are densely populated. There is a similarity in population although one is mountainous and the other certainly is not so. Different people have asked. Some are mathematicians and some aren't. Here are two different countries. Both are not very resourceful in some sense. Then in terms of scientific or cultural development, somehow we understand that in The Netherlands there were some traditions being part of Europe and there was the golden age and so on. Still for instance in terms of development in science, even say, in area of algebraic geometry, I don't know much about the history. But after the war there was a certain period of time. Of course you play a pivotal role almost single handed established the Dutch school of algebraic geometry and number theory, which is quite remarkable. People have been wondering what made that happened. I know this question is ill-posed. The question people ask of course in Taiwan we have not approached anything close to that level. There are resources in people in the sense of intelligence. I mean there are intelligence people.

OORT: Sure, sure. It's very nice that you ask, but this is something I have not prepared to talk about. I understand your question that you want to see how algebraic geometry developed and whether you can learn from that development as far as Taiwan is concerned.

CHAI: Yes, or number theory or any other area.

OORT: At least two aspects. One is it would be very good for development if you have a tradition but not a suffocating tradition. If you wanted to do differential equations in Sweden that was almost impossible because there were towering figures. They would dominate the whole field and everybody would feel very small and so on. In Holland we had a tradition of some geometry, for example Van der Waerden[56]. After 1945 there were no algebraic geometry towering figures. When we started there was nobody who was teaching us. We had to go abroad to import knowledge. Initially I thought it was a drawback. When I came to Amsterdam there was not a single person specialized in geometry or in algebra. A course on group theory was given by given by someone who came once a week from Delft (a technical University) and he lectured about algebra. There was no algebraic geometry, no commutative algebra or whatsoever. I was completely on my own. I thought it was bad but probably it was much better, because I could develop my own ideas. I had my own school. I had very nice students who immediately wanted to join in, who had their own ideas and started with me. They rolled up their sleeves and started working. We had a kind of tradition but no suffocating dominance of people. That is ideal ground. Then, after some years Nico Kuiper[57]came to Amsterdam, and he created a stimulating mathematical environment there. Do you know Kuiper?

CHAI: I know some person by that name but I don't know whether that's the same person.

OORT: He was in differential geometry, topology and later he became the director of the Institut des Hautes Études Scientifiques (IHES) in Bures (near Paris). He was a very broad minded person. He created a good atmosphere. We teased each other as he asked me "Frans, is this corrected in characteristic p?" And I would say "Well, Nico there is one characteristic zero but infinitely many characteristics p". In a description

[56] Bartel Leendert van der Waerden (1903-1996), Dutch mathematician, remembered for his work in abstract algebra and algebraic geometry.

[57] Nicolaas Hendrik 'Nico' Kuiper (1920-1994), Dutch mathematician, worked in differential geometry and topology.

of Raoul Bott by Barry Mazur, Bott says "I can't say that there is any mathematics that I don't like"[58]. The same was true with Nico Kuiper. He started a new type of seminar with participants from all over the country. It was called "de schovenclub"; the Dutch work "schoof" stand for sheaf, and "club" has the feeling of a group of youngsters gathering in order to do something very interesting: the goal was understanding sheaves. Mind you this was in the 50s, even before FAC was published[59]. That initiative was to understand new developments. That seminar was nation-wide (as you you know the country is not so large; you can easily have a seminar on a day, where participants commute by train on that very day). It started in the 50s and carries on until now. I did it for many years. We come together from universities all over the country (and now mathematicians from some universities in Belgium also participate). We discussed new things. As soon as I became aware of some developments, e.g. by reading preprints or hearing a rumor I would give a talk on it. Of course these talks were very non-final, but we tried to introduce new ideas. Sometimes my own understanding was not perfect. Sometimes I wrote notes and later I saw these notes quite often on desks of my students, with scribbled remarks and questions on it. This seminar allowed us to follow new developments from the first beginning. For example, once we had this seminar on modular curves. That was at a very early stage. There were number theorists, geometers. They really were very interested in modular curves. That was long before Wiles proved his famous result, long before what Mazur described in his long paper.

CHAI: The Eisenstein ideal[60].

OORT: Yes, the Eisenstein ideal.

CHAI: So the seminar was in the early 1970s.

OORT: That was a very fruitful seminar. It created the atmosphere that you can be interested in everything, not only in your fields of research but in everything. It creates all kinds of cross-fertilizations. In those seminars

[58] `http://www.math.harvard.edu/~mazur/remembrances/raoul_bott.pdf`; *Raoul Bott as we knew him.* A celebration of the mathematical legacy of Raoul Bott, pp. 43–49, CRM Proc. Lecture Notes, 50, Amer. Math. Soc., Providence, RI, 2010.

[59] J-P. Serre – *Faisceaux algébriques cohérents. Ann. of Math.* **61** *(1955), 197–278.*

[60] B. Mazur – "Modular curves and the Eisenstein ideal." Publications Mathématiques de l'IHÉS **47** (1977), 33–186.

from 1950 until now we had several long projects. One was on moduli. One was on singularities. One was on arithmetic algebraic geometry. These activities aimed at young people understanding new developments, but also the senior researchers profited. Mathematicians like Johan de Jong come for these activities. You create an atmosphere for them to understand new things and automatically good people and a lot of knowledge comes out. Also, in Amsterdam I had for several years a seminar "*wiskunde in wording*" (try to create new ideas in mathematics, or: mathematics under construction); PhD students were invited to talk about their research, their questions, unfinished ideas and so on; usually formulating and talking about such things was helpful, and other people came with suggestions and questions.

CHAI: The colloquium system at Utrecht I saw was remarkable. I don't know whether that was special to Utrecht or it's a general tradition.

OORT: No, it's the general atmosphere in the whole country.

CHAI: The style of the colloquium itself at Utrecht I found just remarkable. It's not easy. I am secretly hoping to be able to transplant a part of it.

OORT: We have a style that we insist that a colloquium is for everyone. But of course it's impossible to discuss difficult details with people who have no idea about elementary prerequisites. We have a system that a colloquium talk is can be divided into two parts. The first part shall be aimed at a general audience and introduces the topic and is elementary. The second half can be much more technical; nobodies minds if you walk out after the first hour. That is totally permitted and it is not an insult to the speaker. If it's a topic that you don't know at all but you want to get a general feeling then you go to the first hour. The speaker can have the second hour with much more freedom to discuss difficult things.

The talk on prime numbers which I will give here is in the style which was invented at Utrecht. It is the following. If you want to teach mathematics, there are two ways. One way is that you take a problem and then you start studying the problem; you collect knowledge you need for that problem. That's very inefficient because in mathematics some problems are quite trivial and you can solve them within two weeks but other problems are extremely hard: to build up the techniques and understanding the basics it will take you many years. There is not so much in between. For example if you go to a hospital and take a MRT scan. The mathematical technique behind that medical application is deep and it is not something that you can easily explain. The right approach in teaching is that you start with elementary calculus, linear algebra, elementary algebra, advanced algebra and so on before you embark on difficult applications or deep abstract material. The draw back of this is that students loose contact with what's going on. Why is it interesting? Why do you want to do linear algebra? Many months of elementary material, without indication why you would need this. We see both methods have a draw back. The solution is that you teach basic courses by building up gradually the techniques you need; at the same time you give talks to students on whatever you like, on applications, on practical things, on logic, on pure thought, on biology, and so on, which is completely hap-hazard across all these fields; this gives students the view on what you could do, triggers their interests.

To let the students feel this is a beautiful but difficult problem. How do I make a schedule for all the trains and so on? The problem of the raveling salesman. This is a really hard problem. Do not try to solve it with your bare hands. All kind of computer people work on it. All kinds of logic people work on it. As a student you get one hour talk on this. That's interesting. You get excited. They try to understand the problem, and they see that a direct approach is too hard; the student longs for more mathematical tools, and that is the motivation for what they will learn in basic courses. They will have the motivation to go on. My prime number talk will be of this kind[61]. I will mention certain problems. It turns out that about half of these is trivial and half of the problems is much too hard, at least at the moment, for all mathematicians. In those cases we even don't know where to start. That's typical for mathematics: some problems are easy, some are very hard. This way of presenting material to students had my interest me when I was thinking about changing from Amsterdam to Utrecht then. It's this kind of view on education and on mathematics.

CHAI: That's wonderful. I am hoping that this will plant some seeds.

OORT: Is this an answer to the question you are asking?

CHAI: To some extent, yes. I mean certainly teaching I think is

[61] Frans Oort - *Prime numbers.* To appear in the ICCM Notices

important. Although here at the Institute we do not teach but I am secretly hoping to bring in some seeds and some days later it will bloom. I don't know.

OORT: But did I answer your question?

CHAI: Yes, you have answered in what you did. I think people they maybe looking at big answers. You answered it in a very practical way and it was very helpful.

OORT: I had a very good fortune that I had with my students. I had several sequences of very good people. They were usually not of the same age. We put them together in the one room, in one office. Usually there was one more senior student with another younger student and they would learn a lot from each other. Much more than from me, maybe. That was a very good system.

CHAI: That sort of things people do here.

OORT: Also it depends on the atmosphere. I will give you a different example. You may contradict me. When I was at Harvard long ago, there was a kind of general attitudes that you were suppose to give a difficult talk to show you are a good mathematician. If you tell something very advanced, not caring whether people can understand it or not, then people are impressed by you and think you are a very good mathematician.

CHAI: What year was this?

OORT: The 80s. Then much later I was there. Moonen[62] was with me. He was one of my PhD students. He gave a talk which was completely non-Harvard in the sense that all details carefully explained, all notations were coherent. Perhaps you would say this is not Harvard style.

CHAI: Well, my personal experience is different.

OORT: Different? Namely?

[62] Ben Moonen, Dutch mathematician, works in fields of algebraic geometry and arithmetic algebraic geometry.

CHAI: When I was a graduate student of course I went to the algebraic geometry seminars. Then I could not understand anything for two years. I think that is almost true during my entire stay. After my third year a fellow graduate student who understood all these things, said "This is not your fault because everybody who comes here has only one person in the audience in mind. That they were trying to impress." That I understood.

OORT: But that fits very well with my story.

CHAI: I don't know whether that's in general Harvard or at least certainly in algebraic geometry at my time that was certainly true. The only purpose, the single point in giving a talk at the Harvard's algebraic geometry seminar was to impress one person.

OORT: Exactly. It was different in my seminars at home.

CHAI: That is a good point.

OORT: In my seminars, students knew after the talk there would be a discussion between the students and me about contents and quality of the talk.

CHAI: That is something we shall learn here. Then you will hit on their heads with a ruler.

OORT: Well, not physically. One student came to my office after a talk and said "OK, Frans tell me." Knowing that certain things were not right . I said "well, it's a very nice talk. Good ideas." "Yes, but?" and I explained another way of arranging and presenting exactly the same material in such a way that people in the audience would understand better the beauty of his results. You can educate people on that level. Why not?

CHAI: I don't know whether the osmosis worked for me. Sometimes I can conclude there was also a reverse osmosis effect. I think here that would be a disaster.

OORT: Let me tell you a joke. I gave a talk about your 95 paper at Harvard. That's the famous paper by Ching-Li. There is an obvious question: can you prove this result by reduction of mod p? I said "This question will come up and I have a little exercise for you." Things on reduction of mod p and taking the Zariski closure. Do these operations commute? Some people in the audience didn't see the answer in first second. If the answer would be 'yes' then Ching-Li's theorem would be trivial because we know the result in characteristic zero and this would imply Ching-Li's result i characteristic p. But these operations do not commute, and you do not have a "cheap" proof this way. Also my question shows the essence of the problem.

CHAI: But this was in the 90s. There was a certain person not in the audience of the talk, and would have solved immediately the "little exercise".

OORT: Of course. Did I ever tell you this story about David Mumford? I was at Harvard and Johan de Jong and Ben Moonen were with me there. I usually took my graduate students along when I was on sabbatical. I was sitting in this computer office where you have a glass window with a view place on a space, part of a corridor, were people could discuss mathematics. Johan and Ben were discussing curves constructed by Mumford in $\mathcal{A}_4$. These we had baptized Mumford curves, each is a Shimura variety of dimension one in $\mathcal{A}_4$, but the term "Shimura curve" already has a different meaning. It was unknown (and still is unknown) whether any of the Mumford curves is contained in the Torelli locus (and it is still an open problem). They were discussing that question and I saw them. All of a sudden a person walked through that place, hearing about "Mumford curves"; for one moment he stares at the blackboard. Then he understands and walks on. Later I said to my students '"there was somebody walking along. Do you know who that was?" No, they had no idea. That was David Mumford who didn't want to discuss algebraic geometry any more. Usually the term "Mumford curves" is something different. For a second he looked puzzled, then understood and walked on.

Indeed, alas, Mumford was not in the audience in the 90's at Harvard. He would understand before I would finish my sentence. He would have an answer. Oh sure. It's such a beautiful theorem and I was happy to be able to report on it.

CHAI: I think this has been a tremendous productive interview. Just to demonstrate I was not even looking at my watch.

OORT: Neither did I.

CHAI: Thank you so much. I think this will be an extraordinary interview.

OORT: Thank you very much for this opportunity.