A passion for numbers

farewell lecture of

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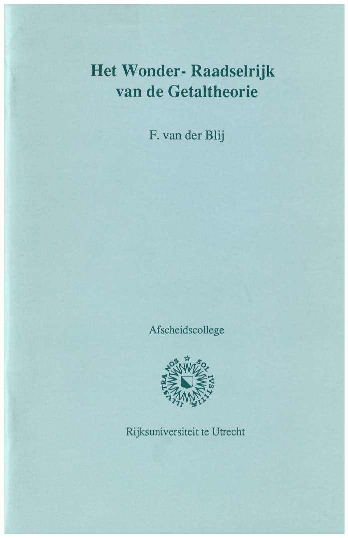
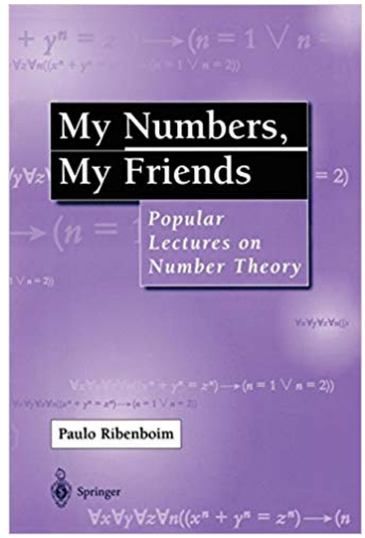
This lecture will be a colloquium style goodbye lecture. The 'goodbye' part means that the lecture is on the occassion of my retirement in which I look back on my past career as a mathematician. The 'colloquium style' part refers to the fact that this lecture will also contain some mathematics. I realize that part of the audience has no background in higher math. There is no reason for despair here, though. You will see some mathematics and formulas during this lecture, but I ask you to let it pass over your head. My hope is that you may remember some of the magic mathematical words that I am so excited about. Perhaps even beyond the end of this lecture.

I began my studies in mathematics in 1971 at Leiden University. Although I liked differential equations and theoretical physics, my attention was soon drawn towards number theory. The reason was that Robert Tijdeman had recently joined the Leiden math department and he started teaching number theory courses. Equally important for me, Robert Tijdeman was, and is, a very nice person who always showed a genuine interest in his students. So number theory became one of my major occupations. One of the things I remember from my student years is that I tried proving Fermat's last theorem. Unfortunately I failed, but these failed attempts turned out to be a good way to digest and use the number theory that I had been learning. In 1976 the director of the Leiden institute suggested that I hurry up and finish my studies because a PhD-position would be available. That is the moment when my mathematical career in number theory started. As a number theorist one naturally deals with numbers, mostly the integers, and in doing so one runs the risk of developing a relationship with certain special numbers that one encounters. This has happened to me, but also to others, as you can see from the following titles.





On the left you see the cover of the farewell lecture of Fred van der Blij in 1988, who was clearly inspired by the world of numbers. On the right you see the cover of a book by Paulo Ribenboim, a Brazilian algebraic number theorist. In it he explains the secrets of his favourite numbers, which are mostly classic integers. In this lecture I wish to do something similar, although the numbers will often not be integers.
Here is the first number.

$$\zeta(3)$$

It has played a crucial role in my career. But first let me explain what $\zeta(3)$ is. First look at $\zeta(2)$. It is the sum of inverses of the integers squared:

$$\zeta(2) = \frac{1}{1^2} + \frac{1}{2^2} + \frac{1}{3^2} + \frac{1}{4^2} + \cdots$$

It was an amazing discovery of Euler that the total sum is equal to $\frac{\pi^2}{6}$. I have seen this identity hundreds of times, but still find it thing of beauty. What do the squares of the integers have to do with π? But Euler continued and for example he found that

$$\zeta(4) = \frac{1}{1^4} + \frac{1}{2^4} + \frac{1}{3^4} + \frac{1}{4^4} + \cdots = \frac{\pi^4}{90}.$$

Similarly for the 6-th, 8-th powers etc.
Contrary to $\zeta(2)$ and $\zeta(4)$ the value of $\zeta(3)$ has always been a mystery, for a long time nothing was known about it. However, there are many more numbers with equally mysterious values so $\zeta(3)$ did not stand out as something

special. Untill 1978, when Roger Apéry proved that $\zeta(3)$ is irrational. That is, there is no fraction p/q such that $\zeta(3) = p/q$. The history of the proof of this theorem is a remarkable one.

Roger Apéry (1916 - 1994) was a French algebraic geometer of the old school. By old school I mean the classical Italian way of doing algebraic geometry. However, in the 1960's algebraic geometry was revolutionized towards a very abstract construction. Alexander Grothendieck was one of the main architects of this revolution. Simultaneously the whole of pure mathematics was formalized by Bourbaki, a collective of mainly French mathematicians who were active from the 1930's to the 1990's. Many of the French mathematicians sided with these new developments. Even to the point where the formal Bourbaki approach would be adopted by the French high school curriculum. Apéry was strongly opposed to the formal and abstract approach of algebraic geometry. Since he was a very outspoken personality he often came in conflict with his fellow mathematicians, who eventually came to see him as an old-fashioned curiosity. As a result he found himself isolated in French mathematics. Perhaps for this reason Apéry turned to number theory in the 1960's and worked on certain diophantine equations. Until in 1978, at the age of 62, he announced his irrationality proof of $\zeta(3)$. This happened during a Journées Arithmétiques meeting in Marseille. To give a flavour of the events I quote from a Mathematical Intelligencer article by Alf van de Poorten, 'A proof that Euler missed':

The board of programme changes informed us that R. Apéry (Caen) would speak Thursday, 14.00 "Sur l'irrationalité de $\zeta(3)$". Though there had been earlier rumours of his claiming a proof, scepticism was general. The lecture *tended to strengthen this view to rank disbelief. Those who listened casually, or who were afflicted with being non-Francophone, appeared to hear only a sequence of unlikely assertions.*

The lecture did not look like a math lecture at all. If anything, it was a direct confrontation between Apéry and his French colleagues, with Apéry provoking his audience. The lecture was so messy and ununderstandable that it seemed nothing would come from it. However, Henri Cohen had noticed a few formulas on the blackboard that looked verifiable and together with Hendrik Lenstra and Alf van der Poorten he convinced himself that there might be something in what Apéry had said. In a few weeks time Henri Cohen with the help of Don Zagier managed to turn Apéry's notes into a valid irrationality proof of $\zeta(3)$. This was the proof presented at the world conference of mathematics in Helsinki in 1978. It created quite a stir at the time and of course this was a great victory for Apéry over his French colleagues.

Here is an indication of the proof in a nutshell. Apéry constructed in a miraculous way two sequences of numbers

$$a_n: \quad 1, 5, 73, 1445, 33001, \cdots$$

and

$$b_n: \quad 0, 6, \frac{351}{4}, \frac{62531}{36}, \frac{11424695}{288}, \cdots$$

having the property that $a_n\zeta(3) - b_n$ tends to 0 very fast when n gets bigger. When asked by people how he found these sequences, Apéry told them he found them in his 'back garden'.

I am telling you all this because at the time I was a participant of the Marseille conference. As a young PhD-student this was my first international conference. I was very happy to see that Apéry was also there, because he had written two very short articles that I had come across during my thesis work on certain diophantine equations. Very soon I learned about Apéry's claim and attended his famous lecture. It was indeed a remarkable lecture. At the time I did not realize that this would also be a unique lecture with nothing coming close to it in the next 40 years. I came away with a feeling of undecidedness and followed the development made by Henri Cohen. To cut a long story short, a few months later I managed to give a proof inspired by Apéry's, but otherwise completely different and much simpler. Here is a three line abstract.

Consider the integral

$$I_n = \int_0^1 \int_0^1 \int_0^1 \frac{x^n(1-x)^n y^n(1-y)^n z^n(1-z)^n}{(1-(1-xy)z)^{n+1}} dx\ dy\ dz.$$

Then one can derive that

1. $I_n = c_n\zeta(3) - d_n$, where c_n, d_n are rational numbers with common denominator less than 27^n.

2. $0 < I_n < 1/30^n$.

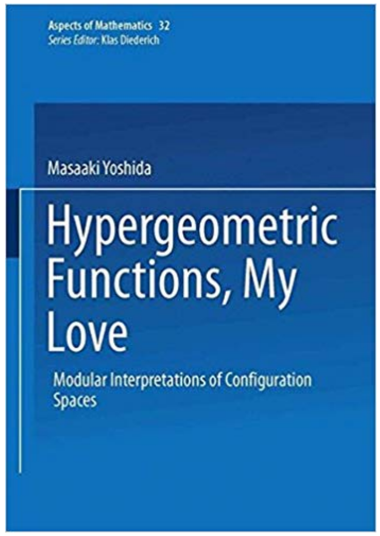
These two properties suffice to show that $\zeta(3)$ is irrational. The derivation of these properties requires no more than some second year calculus. Moreover, the numbers c_n, d_n are precisely Apéry's numbers, but that knowledge is not required for the proof.
This proof went 'viral' in the math community and ever since that time my name has been associated with $\zeta(3)$ and Apéry. For me, as a beginning PhD-student, this was a stroke of extremely good fortune. Having a result that stood out, certainly helped open doors for me. For example, it helped me to a Fullbright scholarship to stay for a year at the Institute for Advanced Study in Princeton.
This was the academic year 1979-1980 and for me it was a wonderful time. It opened my eyes to what was going on in mathematics at a high level. I had always experienced Leiden as a mathematically secluded place where I could prosper in relative quiet and peace. This was exactly the proper environment for me. Very often I see young PhD-students whose research takes place in some highly advanced subject. Which means they have to work the first one or two years almost day and night to master the narrow path towards that particular subject and then hopefully add a new result of their own, preferably a distinguishing result. There are PhD-students who succeed in this way and I envy them for it, for I am not sure if I would have survived such an approach.
After the Princeton year fortune was still on my side and I first got a position as assistent professor in Leiden and soon after that I met my very dear wife to be, Herma. In 1985 a few associate professor positions became available in Utrecht and both Frans Oort and Dirk Siersma indicated that I might apply for one of these. Which is what happened and ever since that time I have been working in Utrecht. At the time Utrecht had a large math department

with a great variety of fields of mathematics represented and a long string of internationally reknown visitors. At the time there was a weekly math colloquium on Thursday mornings at the sacred time of 11 AM. This was a sacred hour, no teaching was planned in that time slot. Jan Strooker was the colloquium chair who did not introduce the speakers but just told them to start. I remember that one of the visitors, Varadaradjan, commented that appararently Utrecht was such a prestigious place that colloquium speakers should already be honoured by their mere invitation. Such was the atmosphere, but I enjoyed the fact that Utrecht was an institute broad in its scope of mathematics. For many questions in algebraic geometry, algebra, Lie theory, analysis, etc I could simply turn to a colleague. I am still thankful to Frans and Dirk for their suggestion, and also for their continuous interest in my wellfare during my time in Utrecht.
Now it is time to show you some more numbers. Already in my PhD-thesis I employed the so-called hypergeometric method to solve certain diophantine equations. This was the beginning of my life long association with hypergeometric functions. They were introduced in the 18th century by Euler and they generalize the functions many people encountered in their school days, such as logarithm, sine and cosine. It is given by an infinite series of the form

$$1+\frac{ab}{c\cdot 1}\times z+\frac{a(a+1)b(b+1)}{c(c+1)\cdot 1\cdot 2}\times z^2+\frac{a(a+1)(a+2)b(b+1)(b+2)}{c(c+1)(c+2)\cdot 1\cdot 2\cdot 3}\times z^3+\cdots$$

and is denoted by $F(a,b,c|z)$. The numbers a,b,c are parameters and z is the (complex) variable. Nowadays there exist also generalizations in one and several variables. Hypergeometric functions occur everywhere in mathematics and mathematical physics, and they played a crucial role in Riemann's ideas on analytic continuation. Over the past 50 years we have seen the rise of a small but succesful Dutch school in hypergeometric functions. It started with Levelt's 1960 PhD-thesis on monodromy. Around 1987 Gert Heckman and Eric Opdam started their famous work on hypergeometric functions associated to root systems. In 1989 Gert Heckman and I published our extension of the well-known Schwarz list for algebraic hypergeometric functions of higher order. Gert Heckman is a friend and colleague of mine from Nijmegen. In our time in Leiden we had adjacent offices, where we shared our budding interest for hypergeometric functions. I remember we once made a kind of pilgrimage to Nijmegen to talk to Ton Levelt. Beside these things and two joint papers we also have our retirement date in common.

The following picture indicates that there were more people who liked hypergeometric functions.



To go to numbers I promised, hypergeometric functions turn out to be a wonderful source of many strange identities and evaluations. Here is one example,

$$F\left(\frac{1}{12},\frac{5}{12},\frac{1}{2}\,\middle|\,\frac{1323}{1331}\right)=\frac{3}{4}\sqrt[4]{11}$$

This is an evaluation that is not supposed to occur. It is a common expectation that the value of a well-behaved transcendental function at a rational point is a transcendental number. In this instance that is not the case, the function value is an algebraic number. Even more strongly, Jürgen Wolfart proved in 1988 that there is a dense set of algebraic arguments where this function assumes algebraic values. Equalities such as above cannot be proven by direct manupilation of infinite series. They require the deep underlying mechanism of elliptic curves with complex multiplication and their periods. In number theory there are different ways of defining the size of a rational number. For example, we know that $\frac{1323}{1331}<1$, a 'small' number, which is what makes the convergence in the above equality possible. However, it is also small in a different sense. Notice that $1323=3^3\times 7^2$. We now a declare a number which is divisible by a high power of 7 to be small. The higher the power, the smaller the number. We call this the 7-adic norm of a number. In general, when p is a prime number, we speak of the p-adic norm. So 1323, hence $\frac{1323}{1331}$ is 7-adically small because it is divisible by 7^2. The left hand side of the above equality also converges 7-adically. An experiment of mine around 1990 indicated that

$$F\left(\frac{1}{12}, \frac{5}{12}, \frac{1}{2} \middle| \frac{1323}{1331}\right)_7 = \frac{1}{4}\sqrt[4]{11}$$

The subscript 7 indicates that we consider 7-adic convergence. So very similar to the previous, but its proof is very different. Once again we use elliptic curves with complex multiplication, but consider so-called Serre-Tate parameters instead of periods.

Now that we entered the world of p-adic numbers I like to introduce one of my mathematical heroes, Bernard Dwork (1923-1998).

He entered mathematics in a very non-standard way. Dwork had an education in electrical engineering and served in the US army in WW II from 1944-46. He was stationed in Seoul, Korea. After that period he worked for ITT, the Atomic Energy Commission and the Radiological Lab in Columbia Medical Center, NY. Meanwhile Dwork also followed evening classes in mathematics at Columbia University. One of his teachers was Emil Artin. Dwork soon got hooked on mathematics. He gave up his job and studied full time mathematics. Five years after his PhD, in 1959, he astounded the mathematical world by proving the first part of the notorious Weil conjectures in arithmetic algebraic geometry. The way in which this was done was along a very different line than what was generally expected at the time. He used p-adic methods as opposed to l-adic étale cohomology. Dwork continued to work in this direction. He founded the theory p-adic differential equations

and p-adic cohomologies and had many followers in the USA, France and Italy. I remember the very lively meetings I had in Paris in the 1980's with the Study group in ultrametric analysis where Dwork also featured. Dwork was also interested in p-adic properties of hypergeometric functions and this is where our interests met. More about this later.
In 1991 I was at a Taniguchi workshop in Japan and there I learnt about A-hypergeometric functions introduced by Gel'fond, Kapranov and Zelevinski. This was a theory of hypergeometric functions in several variables which subsumed all classical examples of hypergeometric functions that were introduced in the 19th and 20th centuries. Using their combinatorial properties it was possible to make sense of the confusing zoo of complicated formulas that the subject of hypergeometric functions was notorious for. Two years later, during a month in Berkeley, I tried to read a then-recent book by Dwork on generalized hypergeometric functions with emphasis on their p-adic properties. I say 'tried' because Dwork's books on hypergeometric functions are extremely hard to read. They consist mostly of formulas and lots of notation. The list of notations of the book I was reading covered over 4 pages. From the things that I did understand I was awe-struck by the similarities between the A-hypergeometric approach and Dwork's theory.
So I made a promise to myself that I would fathom both theories and lay the link between them. That would be a great project. But unfortunately, other duties in Utrecht demanded more time and attention and this promise was not to be fulfilled in the foreseeable future. In the year 2000 I became full professor in Utrecht, which also increased the number of responsibilities. This culminated in the periode 2006-2009 when I was head of department. For me, and also the department, this was a strainful period. The department of mathematics was being incorporated in the newly formed Faculty of Science, covering mathematics to pharmaceutical sciences. Suddenly we were not 'our own boss' anymore and more than half of our supporting staff was transferred to the central Faculty. The future of our department also became uncertain, the criteria 'earning capacity' and valorization became increasingly important, and would we be able to compete with the other departments in the Science Faculty? It is my feeling that by now we have grown accostumed to the role of one of the departments. One thing we will probably never get used to is the continuous stream of counter productive directives from Faculty and central University on how to organize our way of teaching. A positive aspect of being department chair in a big Faculty is that one gets an opportunity to see the excellent research that is being

done in the other departments. Before the transition to the large Faculty we mathematicians tended to be very content with ourselves. We had excellent mathematicians in Utrecht and mathematics was after all the 'fine fleur' of science.
During the time of my chairmanship I had a half-finished manuscript on A-hypergeometric functions in the drawer of my desk. Only in 2010 I could devote myself to finishing that manuscript and to the promise I had made myself in the 1990's. I finished two articles on A-hypergeometric functions, learned about hypergeometric motives and recently, in a project with Masha Vlasenko, we worked on a simplication and extension of some of Dwork's work. So there is a mathematical life after a life as department head!
To end the story I will give you a further example of life with numbers. Recall the evaluation

$$F\left(\frac{1}{12},\frac{5}{12},\frac{1}{2}\middle|\,\frac{1323}{1331}\right)=\frac{3}{4}\sqrt[4]{11}$$

This holds in the ordinary convergence and something similar in the 7-adic convergence. For any other prime $p>7$ the number $\frac{1323}{1331}$ has p-adic absolute value 1 so the hypergeometric series does not converge. Enters Dwork. He showed that if F is a hypergeometric function then $F(z)/F(z^p)$ can be continued in a meaningful way to the domain with $|z|_p=1$. Dwork's famous discovery is that the values of this quotient allows us to compute the zeta-function of families of algebraic varieties in an analytic way. Let us now abbreviate $F(z)/F(z^p)$ by $F(z)_p$ if $p>7$. Out of curiosity I tested numerically what the above evaluation would give us in the p-adic case for $p>7$. Here is the conjecture based on these tests.

Conjecture: For any prime p with $p\equiv 1 (\mathrm{mod}\ 4)$ we have

$$F\left(\frac{1}{12},\frac{5}{12},\frac{1}{2}\middle|\,\frac{1323}{1331}\right)_p^4=1$$

Replace $\frac{1323}{1331}$ by any other rational number and the statement will be completely false.