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History Memories of T.A. Springer as a mid 20th century young mathematician

Back in the days

Shortly before his death about ten years ago, T.A. Springer (1926–2011) started writing down his memories. This is their first public appearance, in an English translation, accompanied by memories of his daughter Emmalien, as well as the (Dutch) text of a scene from one of Jan Beuving's shows about his encounter with Springer.

It is now about seventy years ago that I began to take a serious interest in mathematics and conceived the plan to continue in this field. But my plans for the future were hazy; I had no idea how far I could get. Looking back in 2011, I am a little surprised at my development, from a boy out of the Dutch lower bourgeoisie to a professional in mathematics. Via a somewhat winding road I ended up in international mathematics. It seems to me not uninteresting to follow that road a bit.

This is not a coherent autobiographical story. It is my intention to give an impression of the beginnings of my mathematical development and of the influence and help of others from which I have been able to benefit. Here and there, adding to the story, there are some personal or work-related digressions.

Family

My father Pieter Springer (1893–1955) was born in Delft, but his family came from Leiden. According to my father, the origin of the family name lies there: Springer means jumper in Dutch. In the time of the siege of Leiden in 1573 by the Spanish troops of Philips II, a 'Springer' was a Leiden citizen who managed to pass the Spanish lines by jumping over ditches with a pole, and that nickname has become a family name. I don't have documentation for this provenance of the name. But some facts support it: I could trace Leiden ancestors to 1689 and the surname Springer is still relatively common in Leiden.

My ancestors were simple people. About my grandfather Willem Frederik Springer (1859–1933) I only know that he was a cigar maker in Delft and that he later owned a grocery store in The Hague, in the Transvaal neighborhood. He married my grandmother Mathilda Henrica van Zwanenburg (1861–1927) in 1892. She was a widow who already had six children (two of whom had

T.A. (Tonny Albert) Springer (Scheveningen, 13 February 1926 – Zeist, 7 December 2011) studied mathematics in Leiden with Kloosterman, finishing *cum laude* with a PhD on symplectic transformations. He worked in Nancy and Leiden, before joining the Utrecht mathematical institute in 1955 as lector and from 1957 until his retirement in 1991 as full professor of 'algebra and analysis'. He supervised seventeen PhD students. In 1964, he was elected a member of the Netherlands Royal Academy and in 1983, he was awarded the Shell Prize. He was a speaker at the International Congress of Mathematicians in Stockholm (contributed) and Madrid (invited). For several years, he was an editor of *Inventiones Mathematicae*. In Nancy, Springer proved his eponymous theorem on quadratic forms. He was one of the founders of the theory of algebraic groups. With Steinberg, he worked on conjugacy classes of semi-simple algebraic groups. He is famous for his discovery of the action of the Weyl group as monodromy groups of certain fibrations associated to algebraic groups, now called 'Springer fibrations'. With Borel, he worked on rationality questions for algebraic groups. He also worked on Lusztig's theory of character sheaves, complex reflection groups, Hecke algebras and compactifications of symmetric spaces. He wrote very influential textbooks on linear algebraic groups, invariant theory, and Jordan algebras.

died). My father was the second child and the eldest son of my grandparents. They had eight children (two died early).

After elementary school my father had to go work, further education was out of the question. To get ahead, he reported at the age of sixteen to Schoonhoven to be trained as a non-commissioned officer in the Mounted Artillery. Towards the end of the mobilization period of 1914–1918 (the period of World War I) he was stationed in The Hague. There he met my mother, Emma Lina van Drumpt (1892–1946). She was born in the Betuwe municipality of Kesteren. Her father Teunis van Drumpt (1865–1944) was a carpenter and contractor there. He married Aaltje Aalbers (1859–1950). They had four children, and my mother was the second child. According to my father, my grandfather was well off at first, but later he fell on hard times. During the 1930s crisis, the grandparents lived in poor conditions in Wageningen, with an unemployed son. My grandfather died during the evacuation of Wageningen in 1944, after the airborne landings near Arnhem.

My mother was a good student at school. The teacher felt that she should continue her studies to become a teacher. But my grandfather disagreed and thought his daughter should find a job. So that is what happened. When she met my father, she worked as a live-in maid for a family in Scheveningen.

My parents married in 1917 and went to live in Scheveningen. They had two children. Their first child Willem Frederik was born in November 1918, in a period of important events: the end of the World War I and the height of the Spanish flu in the Netherlands (a younger brother of my father died of it). I was the second child Tonny Albert (named after my mother), born in Scheveningen on February 13, 1926.

Childhood

I seem to have been an easy child. Because my brother was much older and went his own way, I was left to my own devices at home, which didn't trouble me. I believe I was able to read early on, with the help of a teacher (Ms. Cor Langereis) who was our tenant. In my school days I became a fervent reader. First I went to school in The Hague. But in 1934 my father (by then a sergeant at the Mounted Artillery) was transferred to Ede. There I went through

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the upper levels of elementary school. From 1937 I was at secondary school. In 1939 we went back to The Hague.

The HBS

My father made sure that his sons received the secondary school education at the HBS that he himself had missed. The HBS (Higher Civil School), introduced in 1863, was a school type that existed for about a hundred years. Originally it was intended that there, students from the upper middle class attained a level of general knowledge as required for jobs in trade and industry. Later, the HBS became a popular type of school. It also proved to be a good preparation for a study in the exact sciences; prominent Dutch scholars of the early 20th century started at the HBS. The most important subjects in the curriculum were the exact subjects: mathematics, physics, chemistry; and modern languages: French, German, English and of course Dutch. The education was solid. Making the material enjoyable was not a goal.

In the first two years of my HBS period I was at the Wagenings Lyceum. After we moved to The Hague in 1939, I attended the Second Municipal Lyceum at the Stokroosplein. In 1942 I did my final exams there. (Shortly afterwards the school building and its surroundings were razed to the ground by order of German military authorities). I did not have much trouble with the examination material. Physics and chemistry fascinated me the most, more than mathematics. Drawing and 'state houskeeping' (= economics) were my worst subjects. The language lessons also bore fruit: I was still a fervent reader and I could indulge in foreign literature, which had become accessible through education. Later, too, I benefited greatly from the knowledge of languages that was taught to me at the HBS.

Via school I was also introduced to another part of culture: music. For secondary school students in The Hague, concerts were given by the Residentie-orkest, accompanied by explanations by the conductor. Through those concerts I became fascinated by music and that has remained so. I myself have never played an instrument, I am just a listener. ('Music' to me is 'classical' music, taken in a broad sense. I have never been interested in pop music and the like.) After I left school I listened to many concerts in The Hague. Regular musical life more or less continued until the last year of the war. After the end of the war there was a lot going on. Many famous conductors performed in The Hague (or Scheveningen) around 1950 (Ansermet, van Beinum, Furtwängler, Kubelik, Monteux, Walter). In later years this did not happen anymore. Although my concert attendance has declined over the years, I have benefited for many years, together with my wife, from the vigor of the Utrecht and Amsterdam music scene. In the 21st century, this unfortunately stalled.

Society

At the beginning of my school years social and political developments passed me by, secure as I was in the peace and quiet of a family. The crisis of the 1930s brought salary cuts for civil servants like my father, but no forced resignations. My parents had to be frugal but they were used to that.

In contrast to the situation of many Dutch contemporaries, religion did not play a major role for my parents. The 'verzuiling' (pillarization of religion in Dutch society), noticeable everywhere at that time, was not to their liking. I noticed it sometimes, with family and acquaintances, or when Catholic neighbor boys in Ede let me know that they were only allowed to play soccer with Catholic boys.

In Ede I also became aware of the social stratification of the Netherlands, when a classmate (son of an officer) told me that he really shouldn't play with a non-commissioned officer's son. Because I was such a smart student the contact was still permissible. Such expressions of social discrimination were not uncommon in those days. Later on, until the 1950s, I still encountered them in the university world, in remarks made by regents and by representatives of student fraternities. (After the cultural revolution of the years after 1968 this no longer happened; by then everyone had become progressive.) Such statements irritated me. However, I kept my irritations to myself. But I did get a critical (or perhaps even cynical) view of Dutch academic morals and habits. Later I have always distanced myself from the circles of University regents and student fraternities. The few times that I did find myself amongst them I felt out of place.

Back to school. The most far-reaching event in my school years was the German invasion of the Netherlands in 1940, the beginning of the war period 1940–1945. During those years I started to study mathematics. But before I talk about that, it should be noted that in that period, especially the later years, the woes of war always loomed in the background. Incidentally, my family got through those years reasonably well.

Mathematics

In my school years there was not much evidence of a special talent for mathematics. My highest grades were for physics and chemistry (9 or 10 on a scale of 10). For math it was never more than 8 (the teacher, a former officer, did not go higher). At the beginning of the last school year, I thought about studying chemistry at the Technical College in Delft. My interest, however, shifted to physics and astronomy and eventually to mathematics. But the mathematics education was boring. It came down to training for the final exam. I did try to learn a little more about mathematics, from popular science books I found in the library, but it didn't make me much wiser.

Snooping around the book market in The Hague (where there was a lot of everything) however, I found a 19th century French book with mathematical exercises: Frenet's *Recueil d'exercices sur le calcul infinitésimal*. Some of the exercises I could understand and even solve, but most of it was mysterious. If I am not mistaken, at the same time I came across a formula that greatly intrigued:

$$1+\frac{1}{4}+\frac{1}{9}+\frac{1}{16}+\cdots=\frac{\pi^2}{6}.$$

I began to suspect that there were interesting things to be found in mathematics and I wanted to know more about it. A study at a university did not seem possible. After consultation with my parents I decided to look for an office job and in addition to that, to start studying for a mathematical degree in teaching. I ended up with a job at the central government of PTT (Post Telegraph Telephone) in the Hague, as a non-tenured deputy commissioner.

KI and KV

I studied for non-university exams KI and KV to obtain the authority to teach mathematics at the HBS. They were held once a year in The Hague, with a written and an oral part. The material covered a large part of the material from the first years of a university mathematics study.

One prepared for the exams by self-study, or by following lessons with a 'trainer'. I did the latter. For the first KI exam I ended up with a teacher who showed me the ropes (Ms. C. Gerritsen, in early 1945 she was killed in the bombing of the The Hague Bezuidenhout district). An important subject was 'stelkunde', i.e., algebra. The subject matter was old-fashioned. It included, in addition to algebraic matters such as linear equations, some analysis, such as convergence criteria for infinite series. The most difficult subject was uniform convergence of series of functions. I had difficulty with it. I had even more difficulty with the subject of descriptive geometry. For that one had to make complicated drawings on the written exam. Because of my poor drawing skills this was a failure. Nevertheless I passed the KI exam in the fall of 1943, by the skin of my teeth.

For the second KV exam I took lessons from a well-known trainer in The Hague, Dr. M. Scheffer. I benefited greatly from his lessons. (He also taught me the principles of complex function theory). With the exam KV I had no trouble, not even with the descriptive geometry. I took it in December 1945; and by then I was already studying in Leiden.

KI and KV (continued)

For the benefit of those studying for the KI and KV exams, there arose, between 1900 and 1950, a small subculture. This involved textbooks (for example a series by the publisher Noordhoff) and a magazine (the *Nieuw Tijdschrift voor Wiskunde*).

Actively involved in all this was Pieter Wijdenes (1872–1972). Starting as a teacher he became a leading man in Dutch mathematics education, who published many textbooks, of various levels. He also provided training for the K-exams. I used his book *Middle Algebra* (1933) for the algebra part of the KI exam. It is a preparation for the more difficult *Higher Algebra*. For that, reference is made to the *Lessons on Higher Algebra* (from 1926) by Prof. Schuh, an adaptation of a book with the same title by R. Lobatto from 1845 (!). Fred. Schuh (1872–1966) was professor of mathematics at the Technical College in Delft for many years and his scientific output is not large, but he produced many textbooks which, although exact and precise, are mainly dry as dust. They have not stood the test of time. Still worthwhile though, is a more lighthearted book of his on mathematical games, published in English as *The Master Book of Mathematical Recreations* (Dover, 1968). Schuh provided radio commentaries, and partly because of that he was perhaps the most prominent Dutch mathematician in the 1920s and 1930s. The profound L. E. J. Brouwer was relatively unknown then. I only came across his name for the first time in February 1943 in an encyclopaedia article about D. Hilbert, which I consulted when his death was reported in the newspaper.

Mathematics in Leiden

In my civil service days I had saved some money. It became the basis for a study of mathematics at Leiden University, which I started in the fall of 1945. I continued to live in The Hague with my parents. In Leiden a new world opened up for me. I knew the larger part of the University mathematics material for the first few years, but that material was built into a bigger whole and the teaching was not aimed at exam training.

The very first lecture I attended immediately made a lasting impression. It was a (non-compulsory) lecture by H. D. Kloosterman about the number theory of quadratic forms. It began with elementary material that I knew from the KI exam, but presented in a superior fashion. Later he discussed the classical theory of binary quadratic forms.

The first academic exam (the candidacy exam) had two main subjects (for a mathematics student usually mathematics and physics) and an minor (usually astronomy). I wanted to pass the candidacy exam quickly, but there was a problem with a physics major: it required a two-year lab practicum to be taken. I therefore decided to take astronomy as my second major (and physics as a minor subject). I then succeeded in passing the exam in the summer of 1946.

My brief contact with astronomy has taught me a lot of respect for Leiden astronomy. I believe I even thought of switching to astronomy. But I stayed with mathematics: I noticed that I comprehended things more easily there. Also the side of astronomy involving instruments did not appeal to me.

The academic year 1945–1946 was largely filled with studying astronomy and physics. I must have worked hard, but that was a relief after the bureaucratic job of the previous years. There was still time left to become acquainted with the foreign mathematical literature. It was accessible at the 'Bosscha reading room', housed in the Institute for Theoretical Physics, in a corner of the Kamerlingh Onnes Laboratory. The reading room had come into existence at the initiative of the Leiden theoretical physicist Paul Ehrenfest (1880–1933), who, following the example of the university in Göttingen, had set up a mathematical-physical library annex reading room, financially supported by the Dutch-Indian planter K.A.R. Bosscha (son of a Dutch physicist). Ehrenfest's successor, Kramers, kept the reading room going. After his death in 1951, the library was split.

I benefited greatly from the Leiden library facilities. I spent many hours in the reading room with books that I had discovered in the library. In that first year I rummaged through the library, mostly on my own, and became superficially acquainted with all kinds of subjects. Kloosterman's lecture led me to peek at books on number theory, where I found intriguing subjects (prime number theory, Riemann's conjecture…). I then (and a bit later on as well) also tried to learn a bit more about the work of 'classical' mathematicians, through their own publications. That's how I learned a thing or two from Gauss's *Disquisitiones Arithmeticae*. But other classical work, such as that of Riemann, was still out of reach for me.

Assistant in Delft

In the fall of 1946, I had to start looking for work or additional income. A fellow student told me that there were vacant assistant positions at the mathematics department of the Delft Technical College. I applied and was hired.

Around the same time I began to visit the monthly meetings of the Dutch association of mathematicians (the Wiskundig Genootschap whose motto is 'An untiring labor surpasses all'). At the time such a meeting took place in the Amsterdam hotel Krasnapolsky; and the major part was a scientific lecture. The first time I went there was a lecture by the young Delft professor N.G. de Bruijn about zeroes of polynomials. That first time, it must have been in January 1947, I witnessed the famous Brouwer. I distinctly remember our meeting. Before the beginning of that meeting I kept myself in the background, hardly knowing anyone of the attendants. At a certain moment an older gentlemen entered the room, including a tall person having a striking appearance. "That must be Brouwer", I thought and I was correct. To my surprise, he approached me, introduced himself and had a friendly chat with me.

In the meantime, I was an assistant at Delft. The assistants were assigned to one of the professors, and in my case, to my surprise, this was de Bruijn, whom I had seen for the first time shortly before at the Society. An assistant taught practicals for the subject descriptive geometry (which has since disappeared from the curriculum) and in addition did little jobs for his supervisor. This brought me into closer contact with de Bruijn. He is one of the most prominent Dutch mathematicians from the second half of the 20th century. Born in The Hague in 1918, where he also attended the HBS, he took the KI and KV exams as a self-taught person and went to study in Leiden in 1936. He came under the influence of H.D. Kloosterman and obtained his doctorate in 1943, on a subject related to Kloosterman's interests. When I became his assistant he had left this field.

Contact with such an original mathematician was a new experience for me. So far I had been merely receptive in mathematics. That you yourself could invent (or, if you will) find mathematics was new to me. In our conversations about mathematics, I came to be impressed by de Bruijn's knowledge and insight. But I also noticed that we approached mathematics differently. De Bruijn tackles problems of all kinds with original ideas. I am interested in problems coming from the mathematical tradition and in the tools from the new mathematical technology with which they are tackled. An illustration: when de Bruijn once saw me reading in H. Weyl's articles from the 1920s on representations of Lie groups, he asked me if I needed those things for something. I had no answer to that. I had come across those articles via some roundabout route and they intrigued me and incidentally, it took a long time before I grasped the content fully. In our conversations de Bruijn told me what he was working on. It turned out that we could answer a question that he had posed in his lecture to the Mathematical Society. That led to a joint publication (my first) [2]. Somewhat to my surprise; my contribution was only a small one I thought. De Bruijn had conjectured that its main result (an inequality for zeroes of polynomials) is a special case of a more general inequality. That conjecture has strangely lain unproven lurking for years in our article, and it was not proven until 2005, see [7].

My Delft assistantship was not of long duration. In the fall of 1947 I became an assistant of mathematics in Leiden. My contacts with de Bruijn did not end completely, but they did become fewer. Later we went our separate ways and there were minor collisions at crossroads sometimes. In our old age our relationship has become friendly again.

Assistant in Leiden

From September 1947 to October 1951 I was an assistant in Leiden. In those years my teaching task was to assist with H.D. Kloosterman's lecture on the infinitesimal calculus, by means of a practical problem solving class, and to assist with written examinations. Mathematicians did not have workrooms at the university. But if necessary (for appointments with students, for example) I could go somewhere in the Institute for Theoretical Physics, where Kloosterman also gave his elementary lectures. In this way I got to know the theoretical physicists of Leiden at the time: the professor H.A. Kramers, the curator J. Korringa and the assistant P.H.E. Meijer. This led to pleasant and (for me) instructive contacts. But of course I was most involved with the mathematicians in Leiden and with Kloosterman in the first place.

About H.D. Kloosterman

Born in 1900 he did his final exams at the HBS in The Hague and in 1918 he went to study mathematics in Leiden. If I am not mistaken, he had already taken the KI and KV exams during his HBS period. Ehrenfest made it possible for him to continue his studies abroad in 1922, after his doctoral examination. First in Copenhagen with H. Bohr and then in Oxford with G.H. Hardy.

Kloosterman's 1924 dissertation deals with an old problem from number theory: The splitting of an integer into a sum of squares of integer numbers. He tackles it with the, at that time brand new, analytic method of Hardy and Littlewood. A few years later he found a refinement of that method, introducing what are now called 'Kloosterman sums', ingredients of modern analytic number theory.

Kloosterman spent a number of years in Germany. In 1930 he returned to Leiden as a lecturer. In 1947 he became a professor there. Kloosterman was aware of what was happening in international mathematics, including outside his own field. In the 1930s he thus discussed many 'modern' subjects in (non-compulsory) lectures in Leiden. Kloosterman's activities at that time did not contribute to a modernization of the somewhat ossified University mathematics education, then firmly in the hands of an older generation. The same goes for the Amsterdam activities of another 'modern' mathematician at that time, H. Freudenthal.

Kloosterman's main teaching task was teaching analysis in the first two years for students of mathematics and physics. His teaching, in content and presentation, was at a high level. He took great care with it. I have sometimes wondered whether Kloosterman's teaching activities were not at the expense of his scientific production. During the forced closure of Leiden University during World War II, he (in his own words) found time to write a long article, which was published in 1946 [4]. He spoke about that work at the International Congress of Mathematicians of 1950 in Cambridge Mass. (as the only Dutch 'invited speaker'). In the academic treadmill, he would probably not have found an opportunity for long-term work.

During my four years as an assistant in Leiden I had a lot of contact with Kloosterman, primarily related to a calculus problem session for first-year students. At the beginning he took it in hand himself, later more was left to me. I learned from him how to deal with students: in a friendly way, not overbearing or belittling, but with some distance. Through many conversations I got to know Kloosterman better. It did not come to really confidential conversations; Kloosterman was a closed person and the same applied to me. But I learned a lot from what he had to say about mathematics and mathematicians. His statements often had an ironic undertone. Perhaps there was melancholy behind the irony, or a personal problem maybe…

First years in Leiden

During the assistantship I could also prepare myself for the final doctoral examination. That didn't take much effort. Early 1949 I took the exam. Meanwhile I began to take an interest in group theory. I had already taken some things from van der Waerden's *Modern Algebra*. In Kloosterman's seminar on topological groups (on the basis of Pontryagin's *Topological Groups*) other aspects of group theory were discussed. I tried to understand something of Kloosterman's 1946 article. In this article he tackles a difficult algebraic problem (description of irreducible characters of the finite groups $\mathrm{SL}_2(\mathbb{Z}/p^n\mathbb{Z})$), with analytical tools. His analytical prowess somewhat overwhelmed me. But the algebraic problem was intriguing; it stimulated my interest in the character theory of finite groups.

At that time in the Netherlands, one was not very aware of what was going on in international mathematics. Every now and then something trickled through. From Kloosterman I heard about the optimal estimation of Kloosterman's sums, a consequence of the Riemann conjecture for function fields, proven by A. Weil (1906–1998) in 1939. The details did not appear until 1948, as applications of Weil's 1946 book *Foundations of Algebraic Geometry*. When I looked into the book I was impressed by the introduction and I thought "I have to read that book". It turned out not to be an easy read, I did not quite get through it, but my interest in algebraic geometry was awakened.

In 1948, for the first time I attended a lecture by a leading foreign mathematician in Leiden, namely the topologist Heinz Hopf (1894–1971). I was impressed by his lecture. He dealt with an elementary algebraic problem: understanding (not necessarily associative) fields over the real numbers. Hopf proved with tools from algebraic topology that the dimension of such an algebra is a power of 2. Those tools were foreign to me, but it was intriguing that here geometry (topology) was enlisted to help tackling an algebraic problem. Many years later it was proved with more subtle algebraic topology that only the powers 1, $2, 4, 8$ are to be considered. To my knowledge, an algebraic proof of this last result is still not available.

When I was dealing with quadratic forms in the 1950s, I remembered Hopf's lecture, and I saw a way to deal algebraically with the case of commutative (non-associative) fields over the real numbers. There, only dimensions 1 and 2 come into play; Hopf had already shown this via topology. Later I had some dealings with the interplay between algebra and geometry, which I encountered in Hopf's lecture for the first time.

To the PhD

Although I had no clear plans for the future it was clear to me I had to produce a thesis in order to obtain the doctoral degree. It was obvious that Kloosterman would be

Special Issue of Indagationes Mathematicae in memory of T.A. Springer

Indagationes Mathematicae, the scientific journal of the Royal Dutch Mathematical Society, has published a three-volume special issue in memory of T.A. Springer (edited by Gunther Cornelissen and Eric Opdam), containing the first (open access) English translation of his PhD thesis *On Symplectic Transformations*, https://doi.org/10.1016/j.indag.2021.12.003, as well as contributions by Bao, Bayer-Fluckiger, Brion, Carnovale, Ciubotaru, Cohen, Digne, Eberhardt, Esposito, Fenn, Fiebig, Gaitsgory, Garibaldi, Gille, Gross, Guralnick, Gurevich, He, Hoffmann, Howe, Jibladze, Juteau, Kac, Kato, Kazhdan, Knop, Lecouvey, Lusztig, Michel, Neher, Panyushev, Procesi, Rozenblyum, Serre, Solleveld, Sommers, Sorlin, Stokman, Stroppel, Taylor, Varshavsky, Yun, Ziegler, and van der Kallen.

the supervisor. He did not prescribe a PhD topic, but let me do my own thing. This is how it had been for his own doctorate as well. Kloosterman's analytical work on the character theory of the finite matrix group $\mathrm{SL}_2(\mathbb{Z}/p^n\mathbb{Z})$), led me to ask whether one could also approach that case algebraically. That led me to search the literature for what was known about irreducible characters of this kind of groups and turned out not to be very much. G. Frobenius (1849–1917), the father of character theory, had determined the irreducible characters of the groups $\mathrm{PSL}_2(\mathbb{Z}/p\mathbb{Z})$ (p a prime) and of the symmetric groups. I. Schur (1875–1941) had treated the groups $\mathrm{GL}_2(F)$ (F a finite field).

Could one do something for other finite classical groups such as $\mathrm{GL}_n(F)$, $\mathrm{SL}_n(F)$ or the finite orthogonal and symplectic groups? The name 'classical groups' had been introduced by H. Weyl (1885–1955), in the title of his book *Classical Groups* (from 1946). I knew that book, but found it rather difficult. However, it was clear to me that it offers views on technically subtle parts of mathematics, e.g. representation theory and invariant theory. Over the years, I have had much to gain from Weyl's book and also from other publications of his. I admire the way he approaches mathematics.

Useful to me was a, for that time, modern elementary introduction to the classical groups by J. Dieudonné (1906–1992), in his booklet *Sur les groupes classiques* (1948). At first I tried, following Schur, to find the irreducible characters of other linear groups, with $\mathrm{GL}_3(F)$ and $\mathrm{GL}_4(F)$ as the first to be considered. I succeeded in doing so in 1950. It seemed a suitable topic for a dissertation. But a little later I came across an article by Robert Steinberg which showed that he had already dealt with these groups in 1948, in his dissertation at the University of Toronto, with R. Brauer (1901–1977) as supervisor. I then tried to tackle another classical group: the symplectic group $\mathrm{Sp}_4(F)$ (F a finite field with characteristic $\neq 2$). That did not succeed entirely. But an explicit description of the characters of a finite group requires knowledge of its conjugacy classes. For the group $\mathrm{Sp}_4(F)$ these were already described by Dickson in 1901. I was able to extend Dickson's work to the case of $\mathrm{Sp}_{2n}(F)$, with F an arbitrary field of characteristic $\neq 2$. This did not seem to have been done in that generality and it became the subject of my dissertation. I wrote it in the summer of 1951; the defence took place in October. The thesis was a short paper: about 35 pages. In later years it became customary at Dutch universities for doctoral students in mathematics to produce thick dissertations. In my opinion, the obesity of dissertations is a symptom of pomposity of supervisors. A dissertation is a first test of scientific competence. I don't see that 150 pages are necessary when it can be done in 30. It only costs the poor PhD student extra time and money.

There was something special going on with my doctorate. The Leiden Faculty of Mathematics and Physics had long ago put 'cum laude' on hold, allegedly because the famous H.A. Lorentz had not received 'cum laude' at his promotion. In 1951 it was taken out of the refrigerator: I seemed to be the first to receive it again. I myself felt that my dissertation did not deserve such an award. I was aware that it had little depth. I see now that I was a bit clumsy with my doctoral dissertation. The first thing is: I wrote the dissertation in Dutch, English would have been more efficient. I did intend to translate it. But during my stay in France after the PhD I did not get around to it. And after returning to the Netherlands I did not get around to it either.

A second awkwardness was that the conjugacy classes of other classical groups (orthogonal, unitary) had been left out. It would not have taken much trouble and space to say something about that too. Eventually an English exposé of my approach appeared in 'seminar notes' by Steinberg and myself in 1970 [10]. Therein the conjugacy classes were considered in the context of the theory of linear algebraic groups. But in 1951, that theory had not yet taken off.

Nancy

Kloosterman advised me to spend some time abroad after the doctorate and suggested a stay in France, at the Université de Nancy, to which Dieudonné was attached. This was made possible by a scholarship for the academic year 1951–1952 from the French CNRS (Centre National de Recherches Scientifiques).

I left for Nancy at the beginning of November 1951 and I acclimatized quite quickly. I found out that a lot was going on in the 1951–1952 academic year in mathematics. The most prominent professors were Jean Dieudonné and Laurent Schwartz (1915–2002). Dieudonné was someone with enormous work power, who at that time did a lot of work as 'secretary' of the mythical Nicolas Bourbaki, for example in the publication of new Bourbaki volumes (of which there were quite a few in the pipeline at the time). Recently, he had produced a initial text for a future Bourbaki volume on class field theory (which has still not appeared...) I was lucky that he lectured on this subject in 1951–1952. It gave me a background in algebraic number theory. At that time, the cohomological approach was not quite there yet, but the idèles of Chevalley and Weil were already playing a role. Dieudonné was an accessible person. Every week, at a fixed time, he had a chat with me, where I could come with questions and where he told me the latest news. Laurent Schwartz was a distinguished analyst. He had been awarded the Fields Medal for his work on distributions. He lectured on it in 1951–1952 and I followed his lectures with interest. Incidentally, in 1952 Dieudonné and Schwartz both left Nancy, for respectively the USA, and Paris.

Prominent foreign mathematicians such as C.L. Siegel and H. Whitney paid a short visit to Nancy and gave a lecture (in French). Several young mathematicians, like myself, spent some time in Nancy, from outside France there were, among others, Heinz Bauer (Germany), Paul Cohn (England), Heinz Jacobinski (Sweden), Paulo Ribenboim (Brazil). Then there were young Frenchmen (later prominent figures) who worked with Schwartz: Jacques-Louis Lions, Bernard Malgrange, Paul Malliavin. And of course there was Alexander Grothendieck, already a class act at that time. He was then working on his dissertation. I came into closer contact with several of them, mostly with Cohn and Jacobinski, and less with the French.

In the meantime I found out a few things about what the French mathematicians were working on. I heard about Leray's work in algebraic topology, for example his spectral sequences. And I learned that Serre and Grothendieck were considered the best of their generation. This kind of information did not penetrate into the Netherlands. I tried to find out more about several things. In Nancy the theory of topological vector spaces was in the air, because of its importance for distributions. But that theory did not appeal to me. I studied Chevalley's recently published book on algebraic groups, and that book did not suit me. It would be superseded in 1957 by Borel's work. In retrospect, I would have been better off occupying myself with editing my dissertation.

During my stay in Nancy I published one small article, in the Parisian *Comptes Rendus*, in which I answered a question of E. Witt (in an article on quadratic forms from 1936). The answer is sometimes called 'Springer's theorem'. A few years later I learned from an article by Witt that this name is actually not sound: the answer was already given in the 1930s by E. Artin (but not published), see [13]. An example of the well-known phenomenon that naming in mathematics is quite often careless.

Back to the Netherlands

I had vague plans, after the stay in France, to look around a bit further abroad, for instance in the United States. But before I could do so, I received an offer via Kloosterman of a job at the mathematics department in Leiden and I accepted. There was a real danger of failure if I stayed abroad: in Nancy I had noticed that my knowledge and insight were lacking. It seemed to me better not to try to penetrate into the higher regions of mathematics, but to continue as an amateur provincial.

So in the autumn of 1952 I was admitted to the mathematics staff of Leiden University, as a scientific officer 1st class. That staff was only small: three professors J. Droste (analysis), J. Haantjes (geometry), Kloosterman (analysis and number theory) and a few temporary assistants. I was entrusted with part of the teaching of the first two years, which had previously been done by the professors, and a new course in 'abstract' algebra. That was a considerable teaching task, but I did not find it difficult. I enjoyed teaching interested students. I also learned from it: to explain something clearly you mustn't avoid details, but you must also keep a clear distinction between main issues and side issues. I never became a brilliant teacher.

Quadratic forms

In these days in Leiden I ended up in the theory of quadratic forms. If my memory serves me correctly this happened by reading M. Eichler's book *Quadratische Formen und orthogonale Gruppen*, which had just

Photo: family photo album

Tonny and Tijnie Springer in 1953

appeared. I produced a couple of articles on the subject. One of them is about quadratic forms over a p-adic field L (with $p \neq 2$). Such a quadratic form is a polynomial $F(x_1,\dots,x_n) = \alpha_1 x_1^2 + \cdots + \alpha_n x_n^2$. I was interested in the case of anisotropic forms F, where for $\xi_1,\dots,\xi_n$ it holds that $F(\xi_1,\dots,\xi_n) \neq 0$ if not all ξ_i are zero. Guided by the analogy with definite quadratic forms over the real numbers, I could analyze this case. This leads to a description of the Witt group of L, see [5].

Another aspect of quadratic form theory with which I was concerned was what is now called Galois cohomology of orthogonal groups. Let L be any field (characteristic $\neq 2$), F a quadratic form over L. To F belongs an orthogonal group G, the group of linear transformations of variables with coefficients in an algebraic closure of L, which leave F invariant. Now Galois cohomology says that the isomorphism classes of n-dimensional quadratic forms over F are classified by a set $H^1(F,G)$ (which is defined for any algebraic group G over F). In my Leiden time I had found a result that is more or less equivalent to this. My starting point was an old result by Fueter from 1919 which says (in modern language) that $H^1(F,\mathrm{GL}_n)$ is trivial (a noncommutative version of 'Hilbert 90'). I considered my result formal and not very interesting and did not publish it until a few years later. It was in 1962 at Princeton where I was attending a seminar by Borel on Galois cohomology when I realized that those formalities were worthwhile after all.

To Utrecht

In the summer of 1955 I was offered the position of 'lector' (lecturer) at Utrecht University. The lector rank was abolished in 1980. It was something like an 'assistant professorship'. A lector taught but had no administrative obligations. I accepted the offer and started in Utrecht in the fall of 1955. I thus somewhat offended the Leiden mathematicians, in particular Kloosterman. For in Leiden there was also a lecturership on the horizon and Leiden had and has a higher status than Utrecht with some people in the Netherlands. Maybe the reason I preferred Utrecht — my memory is a bit diffuse — is because the future looked less sluggish in Utrecht than it did in Leiden. In Utrecht Freudenthal was an active member of the older generation and from my generation one had van der Blij and van Est, whose interests were close to mine. But what happened in the future could not be foreseen. Indeed van der Blij and I worked together for a while. However, van Est left already in 1956 to Leiden, as successor of the geometry professor J. Haantjes, who had died unexpectedly.

Freudenthal

In 1955, at the Utrecht Mathematical Institute, Hans Freudenthal (1905–1990) was the dominant figure. Born in the vicinity of Berlin, Freudenthal studied at Berlin University in the 1920s, where he received his doctorate in mathematics in 1930, with Heinz Hopf as his first supervisor. He was educated by several (then and later) luminaries. He had become well versed in the modern mathematics of the time. At the invitation of L.E.J. Brouwer, he came to Amsterdam University, as an assistant in mathematics. There he developed great activity, resulting among other things in mathematical publications of a high level. He was one of the most prominent young topologists of the 1930s. In 1946 he was appointed professor at Utrecht University, where he set to work with great energy.

I have known Freudenthal in Utrecht for many years and I usually got along well with him. But we did not have intensive contacts. However, I did become interested in subjects that were in the air in Utrecht because Freudenthal was working on them: octaves (Cayley algebras) and the geometry of exceptional Lie groups. I was not yet familiar with Lie groups but Freudenthal's work also dealt with algebraic issues that I had some insight into. Hence the algebra of the octaves (on which I worked with van der Blij) and of the exceptional Jordan algebras (for which I found a new approach).

I have always remained interested in these matters, as evidenced by a booklet of much later date [11]. Freudenthal always kept his interest in mathematics, as well as his good mathematical taste. But in my later years in Utrecht his activities lay mainly in the, unfamiliar to me, world of mathematics education. I gradually began to find myself in modern parts of mathematics that Freudenthal did not deal with.

First years in Utrecht

Teaching first- and second year students in Utrecht was the domain of Freudenthal and van der Blij. My teaching task in Utrecht was in the later years of study, with subjects from algebra and analysis. More or less fixed lecture topics became Galois theory, complex function theory and Riemann surfaces. In addition, every now and then a special subject. I remember a rather advanced lecture in 1960–1961 on algebraic geometry, for an audience that partly consisted of staff members.

In the meantime, I had continued to dabble around. It became clear to me that knowledge of algebraic groups was necessary for a better understanding of the topics I had been working on. A first observation was that in the problem of my dissertation, over a finite ground field, good use could be made of a result of Serge Lang, which he had needed for other purposes, see [6]. It also became clear to me that I needed to become familiar with the work of Armand Borel (1923–2003) on linear algebraic groups from 1957, completed by Claude Chevalley (1909–1984) (their work could be found in the exposés of [3]). I also tried to get somewhat up to speed on what else was going on in mathematics. Very useful was an interuniversity colloquium (the 'Sheaves Colloquium') set up by N.H. Kuiper.

N.H. Kuiper

Nico Kuiper (1920–1994) studied mathematics at Leiden University from 1937–1941. In 1946 he received his PhD there on a subject from classical differential geometry, with W. van der Woude. During 1947–1949 he was assistant of O. Veblen at the Institute for Advanced Study (Princeton, NJ). In 1950 he was appointed professor of mathematics at the Agricultural College in Wageningen and in 1961 as professor at the University of Amsterdam. From 1971–1985 he was director of the Institut des Hautes Études Scientifiques (IHES) at Bures-sur-Yvette near Paris. Kuiper developed into an internationally prominent geometer, with important contributions to global differential geometry. He was someone who easily made contacts and maintained friendly relations with many prominent mathematicians. He was well informed about recent developments and he tried to make them known in the Netherlands as well. The 'official' Dutch mathematics, at that time embodied in the Mathematical Centre Foundation, did not, however, make use of Kuiper's abilities. Many visitors stopped by Kuiper's house and were hospitably received by him and his wife Agnete.

The 'Sheaves Colloquium' was set up by Kuiper around 1955. The intention was to keep abreast of new developments.

What are Springer fibers?

To study 2×2-matrices up to conjugation, one can look at their trace and determinant. This gives a map

$$\mathfrak{gl}_2 \to \mathbb{C}^2 : M \mapsto (\mathrm{Tr}\, M, \det M)$$

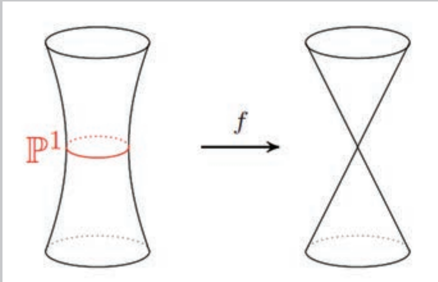
for which the fibers can be written as

$$F_{(b,c)} := \left\{ \begin{bmatrix} \frac{b}{2}+X & -Y \\ Z & \frac{b}{2}-X \end{bmatrix} : X^2 - YZ = \frac{b^2}{4} - c \right\}.$$

This is a smooth quadric when $b^2-4c \neq 0$. In that case all matrices in the fiber have the same two different eigenvalues and hence they are conjugate. If $b^2-4c=0$ the fiber is a cone whose singularity correspond to a scalar matrix, while the rest are nondiagonalizable matrices.

The singularity of a cone $C=\{(x,y,z) \mid x^2-yz=0\}$ can be resolved by looking at pairs of a point and a line on the cone on which it lies:

$$f : \tilde{C} = \{(p,L) \mid p \in C, L \subset C \text{ and } p \in L\} \to C.$$



For every point outside the origin there is a unique such line through p, so apart from the origin this map is one-to-one. Through the origin there is a line on C for every point at infinity, so the fiber is the projective conic $\{(X:Y:Z) \mid X^2-YZ=0\}$, which is a $\mathbb{P}^1$.

A more conceptual approach uses the notion of a Borel subalgebra. This is a subalgebra $\mathfrak{b} \subset \mathfrak{gl}_2$ that is conjugate to the upper triangular matrices. Every matrix can be brought in upper triangular form, so the map

$$\psi : \widetilde{\mathfrak{gl}_2} := \{(M,\mathfrak{b}) \mid M \in \mathfrak{b}\} \to \mathfrak{gl}_2 : (M,\mathfrak{b}) \mapsto M$$

is a surjection. It is a nice exercise to check that the fiber $\psi^{-1}(M)$ consists of

- just one point if M is not diagonalizable,
- two points if it has two different eigenvalues, or
- $\mathbb{P}^1$ if M is a scalar.

Furthermore $\psi|_{\psi^{-1}(F_{(b,c)})}$ is a resolution when $F_{(b,c)}$ is singular.

This whole story can be generalized to arbitrary semisimple Lie algebras: the Springer fiber S_x of $x \in \mathfrak{g}$ is the space of all Borel subalgebras $\mathfrak{b} \subset \mathfrak{g}$ that contain x. T. A. Springer [9] has constructed an action of the Weyl group W on the cohomology of the fiber S_x (in our example $W \cong \mathfrak{S}_2$ is the group of permutation matrices). This is an important contribution to the representation theory of Weyl groups. Springer fibers and their variants have been studied extensively ever since. For instance, 'affine Springer fibers' (Springer fibers in a context with power series) are crucial in the work of Ngo Bao Châu on the so-called 'Fundamental Lemma' in the Langlands programme, for which he got a Fields Medal.

I have no documentation about it, but I do have the dates of the meetings from 1960–1969 (I could not find anything anymore in 1970). These took place every two weeks on Saturdays, in different places, first Utrecht and Amsterdam, later also Leiden and Nijmegen. Most of the participants were from the younger generation, although Freudenthal also came to listen on occasion. Some of the topics that were discussed were: oscillation theory and applications, complex and Kähler varieties, automorphic forms, index theorem. I have good, but unfortunately hazy, memories of our meetings.

Princeton

In the fall of 1960, I was surprised by an invitation from Borel for a stay in the upper echelons of mathematics, as a 'member' of the Institute for Advanced Study at Princeton. The benevolent position of Utrecht University authorities made it possible for me to accept the invitation. Thus I found myself in the United States from September 1961 to June 1962 with my family. Adapting to American life did not cost us much effort. Adjusting to the mathematical entourage cost me a little more trouble. I still considered myself a mathematical amateur and I entered an unknown world of mathematical professionals. I had to get used to that. The great mathematicians with whom I came into contact inspired awe in me.

There was a lot of news in the air and I tried to absorb some of it. Borel, in his 'seminar', covered recent work on algebraic groups over arbitrary fields, a prelude to his later work with Tits, and also discussed his work with Serre on noncommutative Galois cohomology. A great mathematician of an older generation with whom I had superficial contact was Weil. What I knew of his work impressed me. At the Institute he gave some lectures and at Princeton University he organized a weekly 'current literature seminar', which I attended. Weil had a sharp tongue which was not appreciated by everyone. I liked his sharp remarks, they were always to the point.

At the Institute I attended a 'seminar' by Harish-Chandra (1923–1983). He was then a 'member'; a few years later he became a professor at the Institute. He discussed his construction of the discrete series of representations of a non-compact Lie group, one of the highlights of the representation theory of those groups. I was not familiar with the technical aspects of semi-simple Lie groups and I could therefore follow Harish-Chandra only partially. Nevertheless, I stayed onboard his seminar and later found out that some of it had stuck after all.

I got to know many contemporaries at Princeton. Some of them, with whom I stayed in touch with later on were Nagayoshi Iwahori (Tokyo), K. G. Ramanathan (Bombay), Hans Reiter (then in Newcastle, later for a number of years in Utrecht, then in Vienna) and especially Robert Steinberg (Los Angeles).

Mathematics at Princeton

I gradually began to become familiar with linear algebraic groups. The work of my dissertation naturally led to problems about conjugacy classes in such groups. It turned out that Robert Steinberg was also interested in such problems. We got to talking about a question about conjugacy classes in finite Lie groups, which emerged in his work on modular representations of those groups and to which he did not yet know the answer. I saw that Lang's theorem about algebraic groups over a finite ground field, which I had come across earlier, could be brought into play with Steinberg's problem. With that, the problem was related to the question of the connectedness of centralizers in a semi-simple algebraic group. During an afternoon tea at the Institute we presented that question to Borel, who referred us to a recent article of his which answered an analogous question in the case of compact Lie groups. That put us on the right track and Steinberg's problem could be solved, see his article [12].

The course of events illustrates the usefulness of informal contacts. I owe a lot to the contacts with great mathematicians of my generation, such as Armand Borel, Jean-Pierre Serre, Jacques Tits. But I will not elaborate on them here. I do want to say something about another great mathematician: Alexander Grothendieck. My personal contact with him was of short duration, but it stimulated me.

Grothendieck

During my stay in Nancy I had already gotten to know Grothendieck superficially. About his work in algebraic geometry I heard from Nico Kuiper in 1957, if I am not mistaken, who had attended the first 'Arbeitstagung' in Bonn, where Grothendieck had presented his Riemann–Roch Theorem.

Congratulated by Rudy Kousbroek with being awarded the Shell Prize in 1983

Memories of my father: 1957–1970, by Emmalien Springer

One of my first memories is waking up in the middle of the night after we moved to Maarn in the beginning of 1957, I was 2 years old and scared by our new surroundings. My father took me on his arm, and we looked at the sky with the stars and the moon. He told me that the universe was infinite and expanding and that we were all part, albeit infinitesimally small, of this galaxy. That reassured me and it still does, as a matter of fact. Another memory from around that time is me drawing something and my father telling me that a line was a series of infinitely many points. For me this was just as normal as having a home and a life filled with books, papers, magazines, art, culture, classical music, games, the outdoors, lots of interesting and strange people and being interested in everything.

When we lived in Princeton in 1961–1962, we experienced a different kind of lifestyle: lots of parties and dinners where children were always welcome. Halloween, Christmas, Easter were exuberantly celebrated at the Institute and there were outings and dinners (with the Borels, who had two daughters, Dominique and Ann, my age, the Dworks, our neighbours: the Pottashes, later to go to Groningen, and the Truongs, later to go to Paris, and many others). I remember Andre Weil's still very French accent and John Nash walking the grounds surrounding the Institute hands clasped behind his back, my father telling me he was a smart and interesting man. It was a very open, creative and exhilarating atmosphere and I expect my father enjoyed our stay there: in his last will he left the Institute the money that came with the Shell Prize he got in 1983.

We came back to Holland in 1962; my father took the train to Utrecht, where the faculty was in the Boothstraat, a very cozy, peaceful and beautiful place. Freudenthal was always around, and the secretary gave us cookies and lemonade. When I started to notice that my parents were quite strange compared to the other folks in Maarn, we went, again by boat, to America in 1965, starting with a road trip from New York to a summer conference in Boulder, Colorado. After the conference, one of my fathers PhD students, Hans Mars, came along with us in our car on our way to Los Angeles and some of the national parks. After that trip we settled in Santa Monica and my father worked at UCLA. We got to see the lovely Steinberg couple a lot that year and went on trips to everything there was to see, as we were used to doing, every weekend and every holiday. After the summer break in 1966 we went back to Holland. Very much later, I must have been around 50, my father told me that it would probably have been better for all of us to have stayed in the States, but that he had the idea at that time that the schools were better in Holland (I started high school in 1966).

A vivid memory is of the French speaking mathematicians Verdier, Serre, and some others, being happy and doing acrobatics on the grass after dinner at the conference Local Fields (organized by Monna and my father) in Driebergen in the summer of 1966, after we had just returned from LA. I remember being there every day. My father enjoyed himself hugely just like my mother, going out with the ladies, and us, the kids, seeing our American friends. I remember how in '67, '68 and '69, he would also enjoy the company and the intellectual atmosphere of the Tagungen in Oberwolfach.

After the summer of 1966 my father settled as a mathematician in Utrecht, and as a family we settled in Maarn. We had a fruit and vegetable garden, we would eat our own potatoes, beans, strawberries and other vegetables and fruits all year round. After we came back from LA my father started to cultivate asparagus beds, and we ate a lot of asparagus and to this day I am still not wild about them. My parents both liked to walk and hike wherever they were: I remember the cyclical gathering of brambles, chestnuts, mushrooms, an interest he shared with Corrado de Concini, and other edible and nonedible things you could find in the woods near Maarn. In Utrecht he would invite lots of mathematicians at the faculty, now in a new building on the Uithof. Then and untill his death, he loved to travel all over the world, preferably in the company of my mother, to share ideas with his fellow mathematicians. Later on I would joke that my father's mathematics took them abroad for about half of each year.

My dad was a convinced pacifist, like Grothendieck, and a very kindhearted man: he always wanted to help. All of his PhD students, and later my friends, came to our house for drinks, dinner and parties and were treated as equals, which was uncommon at that time, the sixties, in Holland: Hans Mars, Jan Strooker, Wilberd van der Kallen, Gerrit van Dijk, Arjeh Cohen and Henk Barendregt; I will have undoubtedly have forgotten some. My mother would organise drinks, dinners and parties for the people my father invited to Utrecht and for their friends, mostly scientists: Els Hornix, the families van der Blij, van Dalen, Kuiper, Laman, Murre, Monna, van der Sluis, Seidel, Reiter and Tits come to mind. My mother was a big part of my father's career: she edited his work, they talked a lot about everything, she founded the International Neighbour Group in Utrecht, found houses for visiting lecturers and organized outings and activities for the partners. Later on she became an archeologist, and would go to excavations, whilst my father would take care of us: very uncommon in Holland at that time.

My dad was not the run-of-the-mill father: he hated injustice, and fought, on many levels where he had influence, for justice and peace, he was humble and sensitive, could get impatient and short tempered, he showed me the shortsightedness of others ("not stupid, Emmalien," he would say, "just shortsighted"), he was the most morally sound person I have known (later on I found that he was right about a lot of things) and had strong opinions but tried not to judge, he loved good music, art, food, drink, conversation and discussion and was interested in almost anything. He died on his morning walk with my mother of an aneurism of the heart, still working on an article with Lusztig, I think. An extraordinary man with a kind heart, a good soul and a great mind.

Grothendieck gave a lecture around 1960, invited by Kuiper, at the Wageningen Agricultural College, on his new approach to algebraic geometry. This was its introduction to the Netherlands. In the years that followed, I heard a little more about Grothendieck's work in algebraic geometry from Jaap Murre, who was developing into a connoisseur of that work. In early 1964, Murre had invited Grothendieck to give a lecture at the University of Leiden. He discussed his new results on linear algebraic groups, over arbitrary ground fields. Surprising to me was that he needed Lie algebras in the proofs; they had moved somewhat to the background at that time.

In June 1964, at Grothendieck's invitation, I visited the IHES During my stay I worked out calculations in variants of simple Lie algebras over the integers. This involved explicit arithmetic information about the adjoint action of a regular nilpotent element (elementary divisors and the like). The intention was to use that information in a proof of the existence of regular unipotent elements in a simple algebraic group in arbitrary characteristic. (Steinberg had already done that via another way.) I did not quite manage to prove what I wanted; there is a problem if the characteristic is 'bad'. But in a bad characteristic p something else emerged: numerical coincidences of arithmetic information and information about the cohomology modulo p of the corresponding compact Lie group.

I told these matters to Grothendieck and through his intervention it came to a publication [8]. Without his encouragement this probably would not have happened: I found my work to be a rather trivial calculation. Moreover, for me the process of writing things down usually goes hand in hand with some reluctance. In this case, some nice things emerged while writing it up. It turned out to be worthwhile to transfer the Jordan decomposition from an algebraic group over to its Lie algebra. This became an ingredient for a different approach to Grothendieck's new results (from his lecture in Leiden). I didn't quite get there, but when I (at a conference in Boulder in the summer of 1965) discussed the approach with Borel, he saw how to proceed.

A later by-product of the article was a bijection, in good characteristic, between the unipotent variety of a simple algebraic group and the nilpotent variety of its Lie algebra. This was the theme of my talk at the conference on algebraic geometry at the Tata Institute in Bombay in early 1968. It was the last time I spoke with Grothendieck. He may have benefited from my lecture. One ingredient of it was the resolution of the unipotent variety of a simple algebraic group. A year or so later, Grothendieck's 'simultaneous resolution' appeared, a kind of unfolding of the first resolution. That simultaneous resolution was of importance in Brieskorn's work, in which he put simple singularities inside simple Lie algebras, see [1]. In the background of the simple singularities float the Platonic solids. That we encounter these classical objects again in esoteric modern mathematics is a remarkable fact that deserves some reflection.

So much for the digression on Grothendieck.

Een herinnering aan T.A. Springer van Jan Beuving (uit het theaterprogramma 'Rotatie')
Van Professor Springer heb ik nooit les gehad — hij was al geëmeriteerd toen ik met studeren begon. Toch heb ik een strak uitgelijnde herinnering aan hem. In de bibliotheek van het wiskundegebouw stonden twee identieke stoelen. In een van die twee zat ik op een dag een wiskundeboek te lezen — een zeldzame gebeurtenis in mijn studententijd — toen professor Springer binnenschuifelde. Professor Springer, wist ik, was een van de grootste wiskundigen die onze universiteit ooit had voortgebracht, en al over de tachtig, maar nog iedere dag op het instituut. Gekleed in een pantalon met kaarsrechte vouw en een keurig colbertje. Zijn hand ging langs de ruggen van de boeken in de kasten die hij zo goed kende, tot hij op zeker moment het juiste vond, er even in bladerde, en er vervolgens mee in de andere stoel ging zitten. Daar zaten we: de twee intellectuele uitersten die het mathematisch instituut in haar geschiedenis had voortgebracht, zaten naast elkaar de wiskunde te bedrijven. Ik weet nog dat ik toen dacht: als er nu een buitenstaander binnen zou lopen, en ons zou zien zitten, dan kon die niet zien wie van ons nu het genie was, en wie de schlemiel. Dichter bij de Nobelprijs ben ik nooit gekomen.

Conclusion

Meanwhile, I have ended up in 1970. By then I had metamorphosed from an amateur provincial into a mathematical professional. I don't want to discuss the comings and goings of that professional any further. Now is a good time to end the story.

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