

## LOOKING BACK

There are at least two reasons for NOT saying "in the old days, everything was better", namely (a) because the editors asked me not to, and (b) because it is not true. I intended to write a rambling, disorganised, amiable piece, as a pleasant change of composing carefully organised mathematical texts - but in the end it turned out, after all, to be more or less chronological.

After my final examination in 1964, I became a mathematics instructor, which meant that I had to teach students in their first or second year how to do exercises in Analysis, Linear Algebra etc. The teaching was a, not unpleasant, routine; but now and then a task a little out of the ordinary came along. At one time there was a little epidemic of 'trisectionists', people who believed they had found a construction to divide an angle into three equal parts by means of ruler and compasses only. Mathematicians know that this is impossible; but some amateurs do not. It fell to my lot to answer their letters. When the second letter arrived, I decided to compose a standard answer, which ran as follows: "Dear Sir, I checked your solution and found it to be entirely correct. In fact, already years ago I found a solution myself, but did not dare to publish it because of the jealousy and the stubbornness of my fellow-mathematicians. Therefore I advise you to do like me, and keep quiet. Yours etc." But when the third trisectionist came along, I could not use it, because he came in person, sent on by the Amsterdam tourist office.

In June 1966, I got my doctorate (Ph.D.) under Heyting; the thesis was a contribution to 'Brouwer's programme', i.e., the development of mathematics according to the principles of L.E.J. Brouwer's intuitionistic philosophy of mathematics. After my thesis, I did not want to do more intuitionistic mathematics, I had become interested in the metamathematics of choice sequences, a basic concept of intuitionism. I obtained a stipend from ZWO (the precursor of NWO) and went for a year to Stanford, where Georg Kreisel resided, expert on choice sequences.

My host at Stanford had a deserved reputation as a top logician, and also, perhaps undeserved, as a womaniser. When I came to his office for the first time, he talked for a long time, and I was thankful for my previous training in looking intelligent while not understanding. From the corner of my eye I scanned some booktitles on a shelf; one title was 'Seduction'. Afterwards I understood it had been 'Deduction', and since then I never needed convincing that our perceptions are influenced by our expectations.

I never felt so stupid as during my year at Stanford. My training in formal logic in Amsterdam had been very meager indeed, and Stanford at that time was bursting with clever Ph.D. students, Ken Kunen and Jon Barwise among them. I had a lot to catch up with. However, towards the end of that year I had some results. After that, back to the Mathematical Institute in Amsterdam. The next year, 1968, I was asked to become lecturer in Mathematical Analysis. My first reaction was to say NO! I don't want to do research in analysis, I want to do logic. But Heyting told me that I could do research in logic, provided I taught in analysis, and that if I remained an instructor, life would not always remain pleasant. So I accepted; but my first course in analysis was not a success. In the same year Heyting retired, and there was an interregnum filled by guests, the most notable of these being Dana Scott, with whom I shared for a year Heyting's former office.

In 1970 I became Heyting's successor; and to prove that indeed not everything was better in the old days, I only have to think of the extremely lightweight procedure for appointing professors in those days (maybe my luck). Around that time, Dirk van Dalen was appointed at Utrecht; together we built up a regular curriculum in Mathematical Logic, where all the basic subjects were represented. At that time there were many mathematics students, and they stayed longer. They could afford to take a course now and then not for credit, but to satisfy their curiosity. So there was an audience for our logic courses. Since then things have changed dramatically: a shortened curriculum, more financial pressure on the students, ever fewer students. As a result, I decided this year not to give an introduction to constructivism, a subject which I had been offering each year in one form or another since 1968.

Up till 1980, roughly, my own research centred round choice sequences and Kleene's realizability and all its variants. My interest in realizability led to a Springer Lecture Notes in 1973, with chapters from Craig Smorynski and Jeff Zucker and myself, where much information on intuitionistic metamathematics was brought together. After 1980, choice sequences seemed to be played out: the research, one might say, had been too successful, and there were few open questions remaining, or so it

seemed then. Some of this is continued and vastly generalised in topos semantics. However, I still recommend choice sequences as an interesting example to philosophers of mathematics. Realizability, on the other hand, is 'alive and kicking', and has penetrated into computer science.

In 1980 Heyting died, and he left his Nachlass to the Mathematical Institute. This caused me a lot of 'invisible' work: the ordering and cataloguing, with the help of several students as assistants, of Heyting's papers. The work we put in has been invisible in the sense that it did not result in lectures or papers in journals, only in a report 'Index of the Heyting Nachlass'. Recently the Heyting archive has been moved to the State Archive at Haarlem. The archivists paid me the compliment of saying that this was the best ordered and indexed personal archive they had seen in their career, and I am really a little proud of that. You might say this has been a labour of love.

Writing, with van Dalen, a lengthy two-volume introduction to constructivism, dealing with all the main techniques and results, was a way of taking stock of what had been achieved so far. The book came out in 1988.

After this lengthy introduction, what next? A brief interlude with linear logic followed. But after 1993 I left linear logic alone and started writing an exposition of elementary proof theory, a collection of results and techniques widely used, in mathematical logic as well as in computer science, but also widely scattered in the literature. It was intended as a book I could use for my courses. The subject was not completely new to me, since I had already encountered it in intuitionistic metamathematics. The second, revised edition of the resulting book, with Helmut Schwichtenberg as co-author, has been finished recently.

As the foregoing makes clear, I started as a mathematician, and that is what I still am. Switching from the Mathematics Institute to a group 'Logic and Theoretical Computer Science', and then to (the margin of) the ILLC made very little difference to me personally. Actually, moving to another building, not residing any longer on the same floor as the mathematicians, made more of a difference, at least socially: I saw less of my colleagues in mathematics.

What did bring something new was the Master-of-Logic programme. It was interesting and stimulating to meet students from such diverse backgrounds and nationalities. It also made it worthwhile to teach some subjects for which otherwise there would have been not enough interest.

It almost seems to me that since I moved to the Euclides building there has been a paucity of memorable anecdotes. But I recall a fairly recent one. Some years ago I received a visit, one afternoon, of a member of the "Bund zur Verbreitung unerwünschter Einsichten", the Association for the Dissemination of undesirable Insights. This Association turned to be a most curious left-over from the turbulent sixties, a reservation for elderly activists, said to exist at the time only at Karlsruhe (still?). They were lobbying for international support in their quarrel with the University. Of course I withheld support, since, as everyone knows, I am a staunch supporter of the establishment.

The most rewarding experience of my career? Undoubtedly the contact with my Ph.D. students. (The least rewarding experience I try very hard to forget.)

Why am I leaving at 61? First and foremost, for health reasons; I feel that cannot any longer fulfil my tasks as they ought to be fulfilled. But there is more. Thinking of our young researchers, I feel that they are motivated by intellectual challenges. But when I listen to politicians, captains of industry, decision-makers and the like, the only message which gets through to me, regardless of the packaging, is that science, even fundamental research is exclusively justified by results which bring material benefits one way or another. It looks as if we are a society of spoiled children, who want more, and More, and MORE ... This is the background against which we ask for funding for our projects. Applying for outside funding shows commitment to one's projects, I am told. But I don't fit into this, and I want out!

And afterwards? The intellectual void? No fear!