

Chapter 2

A Short Scientific Autobiography

Dag Prawitz

2.1 Childhood and School

Being born in 1936 in Stockholm, I have memories from the time of the Second World War. But Sweden was not involved, and my childhood was peaceful. One notable effect of the war was that even in the centre of Stockholm, where I grew up, there was very little automobile traffic. Goods were often transported by horse-drawn wagons. At the age of six we children could play in the streets and run to the nearby parks without the company of any adults.

One memory from this time happens to illustrate a theme that I was to be quite concerned with as an adult: the difference between canonical and non-canonical methods and the power of the latter. Children in Sweden normally do not begin school until the year they reach seven. Many learn to count before that. Some of us could count to one hundred and were even able to add small numbers together. We did this orally in the canonical way: To get the sum of 3 and 5, we counted 4, 5, 6 ... and kept track of the number of steps until they were 5, whereupon we proudly announced 8. To add big numbers was of course out of question, even if we understood how it could be done in principle: to add 30 and 50, we would have to start counting 31, 32, 33, and would loose count of the number of steps long before reaching 80. My father then told me of a non-canonical way: to add 30 and 50 is to add 5 tens to 3 tens, just like adding 5 apples to 3 apples. I still recall the faces of my friends when I started to practice this trick, a mixture of suspicion and admiration: how could I do so big sums so fast?

Otherwise, I was slow as a child. My father tried to teach me how to read, but I never managed words longer than one syllable before entering school. Having absorbed what I learned in school, I showed some pedagogic ambitions, however. At home I set up my own school where I took the role of the teacher of my three years younger sister Gunilla, trying to teach her to read and write. This was successful enough to get her accepted in the second form when she entered school at seven; it is another story that skipping the first form turned out to have many drawbacks.

D. Prawitz (✉)

Department of Philosophy, Stockholm University, 10691 Stockholm, Sweden
e-mail: dag.prawitz@philosophy.su.se

Summers were spent on an island in the Stockholm archipelago, a different world I loved very much. A renowned Hungarian mathematician, Marcel Riesz, professor at a Swedish university, often came to stay with us for a while. He had been the teacher of my father, and they used to discuss mathematics while sunbathing on the rocks at the seaside. When I entered secondary (middle) school at the age of eleven, he wanted to teach me something and tried to prove Pythagoras' theorem in a way that should enable me to take it in. I understood the general idea of proving theorems from axioms, but I questioned the axioms and grasped little of the proof. He also explained to me why a natural number is divisible by 3 if, and only if, the sum of the numbers that occur in its Arabic notation is divisible by 3. I understood his demonstration, I thought, but when he gave me the task of writing down the proof in my own words, it was too much for me. My mother was afraid that I would become overworked by such attempts, and asked Marcel to stop these exercises.

My early childhood seems to me now to have been rather dull. I entered gymnasium (high school) at 15, which was like taking a step into the world of adults; teachers showed us a new kind of respect, addressed us in a more formal way, and had more interesting things to say—this gave us more self-respect, and we wanted to do things that adults do. In my class, we were a group of friends who started to publish a magazine. It was stencilled and sold well within the whole school. As editor of the magazine, I was summoned to the headmaster one day. A poem written by another pupil that we had published was offensive when some of the words were taken in a sexual sense. The fact was that I was completely ignorant of that reading.

I was also active in several clubs that flourished at the gymnasium. I came to chair several of them: the school's rifle-club, its literary society, which then celebrated its 50th anniversary, and the pupils' council, then a fairly new invention. I was also a little involved in an association of similar councils in the Stockholm area that was formed at this time. In this connection, the idea of a joint school magazine came up. Together with some pupils from other schools, I started such a magazine, and it came to be sold in schools throughout the whole of Sweden. It was printed at one of the leading Swedish newspapers, and its layout was that of a newspaper. Soon I spent most of my time as editor in the composing room of this newspaper, as a reporter at different events, or as canvasser for advertisements to finance the project. It was a huge undertaking and filled up more than my spare time. The headmaster of my school (now another one) encouraged my work, and allowed me to take time off from school. My schoolwork was minimized accordingly.

The school subjects to which I devoted most energy were history and philosophy. Most of our teachers were very competent, and many were qualified to teach at the university. My philosophy teacher was especially qualified and had written a book about Nietzsche. He was also an inspiring teacher. Philosophy was an optional subject the last two years of gymnasium, and my interest in philosophy dates from this time.

2.2 Undergraduate Studies

When I finished school, I got a temporary job for the summer as journalist at a provincial newspaper. For the next year, I had been awarded a very generous scholarship to study at the University of Wisconsin, where I was accepted as a junior student (to have completed a Swedish high school at this time was considered comparable to having studied two years at an American university). There I studied journalism, rhetoric, and psychology. My plans for the future were quite unclear. I wanted to study at university for a few years. As possible careers afterwards I was thinking of journalism or politics. What I did not have the slightest idea of at the time was that the University of Wisconsin had a most eminent logician Stephen Kleene, though I would become very aware of this two years later.

Back at Stockholm I wanted to allow myself a short time of luxury, during which I would study philosophy, psychology, and mathematics for a first degree, before doing something more useful that could earn me a living. The normal time for graduating from Swedish universities was three years, but I was eager to get it done faster.

I started with theoretical philosophy, which is a separate subject at Swedish universities. It was taught at Stockholm University by only one professor, Anders Wedberg, and an assistant, Stig Kanger. Wedberg was an historian of philosophy, internationally known for his book *Plato's Philosophy of Mathematics*. But he had studied logic at Princeton at its glorious period at the end of the 30s, and had written two small booklets on modern logic intended for a general audience. They were written in a fluent style and explained not only the main ideas of sentential and predicate logic in an intuitive way but touched also on more advanced and fascinating themes such as Gödel's completeness and incompleteness theorems.

In my first semester, Wedberg lectured on logic. As a teacher, he taught us scrupulously the language of first order logic. This combination of intuition and exactitude fascinated me. It was thrilling to demonstrate rigorously by intuitive reasoning that a conclusion necessarily follows from given premisses, and it was mysterious how this could be possible at all. It seemed that modern logic was able to explain why such necessary connections hold, and it was impressive that this could be done so exactly and in such details that within certain areas, a machine could in principle always take over and find a proof, if the connection did hold.

In the second semester we were ready for Hilbert and Ackermann's book *Grundzüge der theoretischen Logik*. It was then in its third edition from 1949 (not to be confused with the completely changed fourth edition). Wedberg told us that the principle of substitution for second order logic was wrongly formulated in the first edition and that even the attempted emendation in the second edition had gone wrong. Let us now see if it has come right in the third edition, he suggested. I wondered myself how one was to decide such a question when even the experts had got it wrong. I imagined that one would have to read a lot in order to compare different formulations of the rule that could be found in the literature. Wedberg's intention was of course different: we should think out for ourselves how the rule must be formulated in order to be correct. He taught us how to reason, and gave us faith in our ability to do so.

At the end of this semester, Kanger was to defend his doctoral thesis at the kind of public disputation that is still in use in Sweden; I followed it with fascination. One of his results was a new completeness proof for first order logic that offered a seemingly feasible method for proving theorems. Even a computer could use it, Wedberg suggested, and so the possibility of a machine proving theorems could be realized in practice.

I now got the idea of devoting the approaching summer to such a project. I withdrew to a lonely country house in Denmark, bringing with me Kanger's dissertation *Provability in logic* and Beth's essay on semantic tableaux, *Semantic entailment and formal derivability*, which I had been told contained a completeness proof similar to Kanger's. The idea was to work out an algorithm for proving theorems that could be implemented on a computer. There were no programming languages at that time. Programs had to be written directly in the machine code for a particular machine. I knew no such codes, but I understood enough of the principles of mechanical manipulations to invent my own programming language. I defined a number of specific operations for syntactic transformations of formulas. In terms of these, I laid down instructions for how, given a sequence of formulas in a first order language as input, one was to make syntactic manipulations in a certain order and store the result in various specified memories. The outcome of following the instructions would be a proof that the last one of the input formulas was a logical consequence of the preceding ones, if this was in fact the case.

I presented and discussed my algorithm in an essay I submitted as a graduation paper when I returned to Stockholm in the autumn. My studies in psychology and mathematics had run parallel with the ones in theoretical philosophy, and later in the autumn I obtained my first university degree (corresponding to a BA). I wanted now to go in for theoretical philosophy. To my delight Wedberg proposed that I should continue as a graduate and offered me a scholarship. In January 1958, I enrolled and started to study Kleene's *Introduction to Metamathematics*.

2.3 Mechanical Proof Procedures

The first Swedish computer had been developed in the 50s, and for a short while it held the world record for speed. A successor to it now filled several big rooms in the former building of the Royal Institute of Technology in Stockholm. It so happened that my father used it for certain tasks and knew how to program it. He was kind enough to use part of his summer vacation to translate into the code for this machine the algorithm that I had written in my homemade programming language.

My father was a mathematician and did not know any modern logic; his contact with the subject amounted to having heard Hilbert delivering the lecture "Über das Unendliche" at the celebration in honour of Weierstrass in 1925 at Münster—having been sent there by Mittag-Leffler, whose assistant he was at that time. Strangely enough we seldom spoke about such things. He was usually quite busy, and after having translated my algorithm into a machine program he did not have more time to

devote to it. His work was continued in the autumn by a fellow-student of mine, Neri Voghera, who was an assistant at the institute responsible for the machine for which the program was written. He tested and modified the program, and in 1958 he could run it to prove simple theorems of predicate logic. It seems to have been the first full-scale theorem-prover for predicate logic implemented on a computer. I presented our work at the First International Conference on Information Processing at UNESCO in Paris in June 1959, and a joint paper by the three of us was published in 1960.

It was clear to me already in the summer of 1957 that the proof procedure I was working on was not very efficient after all. One of its main limitations was that instances of quantified sentences that had to be formed in the proof search were generated more or less at random. In the essay I wrote that summer, I discussed how to improve the procedure in that respect: the substitutions for quantified variables should be postponed to a stage at which one could see that the substitutions were useful for finding a proof. When I presented the essay in the autumn, it turned out that Stig Kanger had a similar idea.

A year later, I was able to work out this idea in the form of a method that did not make any substitution until it was guaranteed that the substitution instances would generate a proof. The method was presented in a rather unwieldy paper called “An improved proof procedure” published in 1960. It would have benefited from some guidance, but to supervise graduate students was not the custom in Sweden at that time, at least not in philosophy. Anyhow, the ideas of this paper came to some use in the soon growing field of automated theorem proving, as was generously acknowledged by Martin Davis when the paper was later republished in the volume *Automation of Reasoning*. It also became my thesis for a second university degree (*filosofie licentiatexamen*), which was then a required step before one could go on to a doctorate. I now left this field for other interests that I wanted to pursue in a doctoral dissertation, and returned to it only a couple of times at the end of the 60s, when I described the main idea in a more mature and compressed way and also developed it somewhat further.

2.4 Towards the Doctoral Thesis—Normalizations of Natural Deductions

Before I could take up doctoral studies, I had to complete my military service. I had so far been conscripted for a few summer months as a recruit, and had been able to postpone the rest of the service, but now it had to be fulfilled. I was placed as a psychologist because of my earlier psychology studies. My duties were not without interest. I conducted hundreds of interviews with soldiers as part of the military selection process, and worked for several months at a psychiatric clinic with the aim of learning how to sort neurosis in the case of war. But I doubt that I should have been able to function very well as a military psychologist if it had really come to it.

In the summer of 1961, I returned to studies. I had been awarded a scholarship for four years in order to write a doctoral dissertation. Again I withdrew to a lonely place,

this time renting a summerhouse in the Stockholm archipelago. There I started to read works by logicians who had philosophical ideas about what matters in deductions.

One reason for leaving the field of automated theorem proving was that I had a wish to understand philosophically what it really was that made something a proof. I took for granted that this question must somehow be connected with the meaning of the sentences occurring in the proof. Stig Kanger, who was the closest senior logician in my surroundings, was sure that all such questions concerning meaning and all that really matters in logic were to be approached model-theoretically. To begin with, this seemed plausible to me. At least, it was not contradicted by my experience of how a completeness proof could generate a proof algorithm. It also seemed to be confirmed by other facts such as the one that Kanger had obtained in his dissertation a three-line proof of Gentzen's main result, the Hauptsatz, from a completeness proof. For a while, I was led to study model theory, but it soon left me unsatisfied, and when resuming studies in 1961, I turned to approaches that linked questions of meaning to deductive matters.

I began reading works by Paul Lorenzen and Haskell Curry. Curry had made several attempts at giving a kind of inferential interpretation of the logical constants, which attracted me, but I was repelled by his formalist outlook and his general way of writing, and never looked at his work on combinatory logic. Lorenzen related the meaning of the logical constants to statements about underlying logic free calculi like Curry, but he was more inspiring to read, although I was not yet quite ready for his constructivist attitude.

Then I reread Gerhard Gentzen's "Untersuchungen über das logische Schliessen". I had read this work before and like most people I had mostly paid attention to his sequent calculus. This time I was immediately struck by the depth of his idea that the introduction rules of the system of natural deduction determine the meaning of the logical constants and that other rules are justified by reduction procedures that eliminate certain uses of them. It seemed to give some kind of answer to my question what it is that makes something to a proof. I saw in a flash that by iterating these reductions it was possible to put the deductions in a certain normal form, which gave Gentzen's idea a sharper form.

The discovery of this normalization procedure filled me with great joy that summer in the Stockholm archipelago. Soon I also realized that this procedure corresponded to eliminating cuts in the sequent calculus, which gave the latter enterprise a greater significance. I was surprised that Gentzen had not stated this beautiful normalization theorem for natural deduction, but it seemed clear from his writings that he had seen the possibility of such a theorem and that his Hauptsatz had its source in this insight. Accordingly, I was not surprised when Jan von Plato a few years ago told me that he had found a proof of the normalization theorem for intuitionistic logic in Gentzen's unpublished Nachlass.

When I returned to the mainland in the autumn, I presented in outline the proof of the normalization theorem at a joint Stockholm-Uppsala seminar, which had just then been initiated. Wedberg pointed to a problem concerning the inductive measure that I employed. I could remove the problem shortly afterwards, and used the rest of the year to work out other details connected with normalization.

The next spring I went to the Institute for Mathematical Logic at Münster and stayed there for the summer. I attended lectures by Hans Hermes, Gisbert Hasenjaeger, and Wilhelm Ackermann. I was glad to see that the author of my old textbook of logic was still alive; Ackermann was now a retired schoolteacher, and came to the institute every second week to lecture on his notion of “strengte Implikation” and to stay for the joint seminars, which were led by Hermes. They gathered all at the institute and were its highlights. I was invited to present my results about normalization of natural deduction at one of its sessions. My presentation was well received on the whole, but Hasenjaeger wondered why I bothered with natural deduction when Gentzen had been so happy to leave it after having found his sequent calculus.

Hasenjaeger’s reaction was not uncommon. It took some time before the normalization theorem was accepted as a significant way of formulating the main idea behind Gentzen’s Hauptsatz. Certainly, for certain aims and from certain perspectives, sequent calculi are preferable to systems of natural deduction. But the philosophical essence of Gentzen’s result appears most clearly in natural deduction: here one sees how applications of introduction rules give rise to canonical forms of reasoning and how other forms of reasoning which proceed by applications of elimination rules are justified to the extent that they can be reduced to canonical form; the prerequisite of this justification by reduction was that the eliminations were the “inverses” of the introductions, to use a term that I had found in Lorenzen, and when this condition was satisfied, the prerequisite for proving the Hauptsatz for the corresponding calculus of sequents was also at hand. As is clear from the introduction to his second consistency proof, Gentzen himself saw this as a key to the understanding of his result. I am glad that Gentzen’s high appreciation of his normalization result has been further confirmed by von Plato’s recent investigations of Gentzen’s Nachlass; it appears that Gentzen even wanted to make it a cornerstone of a book on the foundations of mathematics that he was planning.

Most of what was to become my doctoral dissertation *Natural Deduction* three years later was ready for publication now after my first year as candidate for the doctorate. But my plans for the dissertation were more ambitious. There were several other themes that I was working on. One was the question of the best way to present classical logic as a natural deduction system. I wanted to have a more dual system than Gentzen’s, and experimented with several different ideas, for instance, letting the deduction trees branch downwards at disjunction eliminations or taking the nodes of the deduction trees as disjunctions.

Another theme was the extension of the normalization theorem to second order logic. I knew that Takeuti had conjectured that the Hauptsatz held for second order logic, but I had not yet digested his rather long proof of the claim that this result would yield the consistency of first order Peano arithmetic.

A third theme was the relation between classical logic and intuitionistic logic. A fourth was the interpretation of intuitionistic logic. I saw that one could map natural deductions into an extended typed lambda calculus. To each provable sentence in predicate logic I assigned a term in an extended lambda calculus, and took it as an interpretation of intuitionistic logic in terms of constructions. It intrigued me that this mapping of two natural deductions of which one reduced to the other resulted in

two terms in the lambda calculus that denoted the same construction. I presented this idea at a seminar in Stockholm in 1963 attended by Wedberg, who to my surprise turned out to be well versed in Church's lambda calculus; recalling his period of study at Princeton in the 30s, this should not have been surprising after all.

On the whole, however, there were few persons with whom I could speak about logical matters; supervision was still something unheard of. However, in 1963 I got to know Christer Lech, the only mathematician at Stockholm with an interest in logic at that time. We discussed various things including themes around my planned dissertation such as alternative systems of natural deduction for classical logic, and he then came up with helpful ideas about how to formulate the reductions if one based the system on the axiom of the excluded middle. Christer held a position as docent, which gave him plenty of time for research and the right to teach what he wanted. One semester he chose to lecture on Gentzen's second consistency proof. I felt that I understood the proof better than he because of knowing how to normalize natural deductions. This boosted my self-confidence, but the feeling was not really well grounded—it was not till a year ago that this understanding materialized in the form of a proof of a normalization theorem for Peano arithmetic.

For the spring term of 1964, I was invited to UCLA as visiting assistant professor. I was to be a substitute for David Kaplan who was on leave. The invitation came from Richard Montague, whom I had met at a conference at Åbo (Turku) in the summer of 1962. Some of the participants had attended the World Congress of Mathematics held immediately before in Stockholm and went together by boat from Stockholm to Åbo. In this connection, I met several persons whom I knew before only because of having read their works, like Church, Tarski, Mostowski, and Curry; the latter introduced me to Kripke, who accompanied him.

The invitation to teach at UCLA flattered me—I was only 27 and had not yet got my doctorate. My duties there were to give an introductory course in logic and to run a more advanced seminar in philosophy of logic. The textbook for the course was written by Kalish and Montague and used a system of natural deduction that they had recently developed. It had some pedagogical merits, but the authors had understood nothing of the proof-theoretical potentialities of a system of natural deduction.

As theme for my seminar, I chose to discuss various interpretations of intuitionistic logic, starting with the ones by Kolmogorov and Heyting, and continuing with my imbedding of intuitionistic natural deductions into an extended lambda calculus. Intuitionism was a rare thing at UCLA; Montague who attended a few of the sessions complained that some of the interpretations were too informal.

Every second week there was a joint logic colloquium for the whole Los Angeles area in which a number of very competent logicians participated; the most well known was perhaps Abraham Robinson. I was invited to present a paper and chose to talk about my ideas of a normalization theorem for a system of classical natural deduction where the deduction trees branch downwards at disjunction eliminations. The idea is quite natural but the precise formulation is delicate, as several people have discovered who have later tried to develop such a system. I had a simple solution, I believed, but was worried that it might be too simplistic. I started my presentation with a warning, saying that there might be something wrong with the proof since it

used only usual induction over the length of the deduction tree, which meant that the proof worked also for second order logic, but such a proof should not be possible in view of Takeuti's claim.

After a few minutes of lecturing, I felt very uneasy and started to see the situation from the outside: who am I, standing here in front of all these competent people, to present an obviously fallacious proof? I blushed, my heart was throbbing, and I felt as if my tongue was stuck in the mouth. But the lecture went on, and no one could see anything wrong with the proof. The presentation was generally considered to have been quite clear. My tension seemed not even to have been noticed; this was confirmed by some students who had attended the seminar and whom I told how I had felt during the talk. Montague was of the opinion that I should not worry about Takeuti's claim, which opinion he backed up by saying that people often make unfounded claims that so and so cannot be proved because of Gödel's result.

Not long afterwards I found the error in my proof, and I also understood how Takeuti's claim was obtained as an immediate corollary of three very easy lemmata; the full proof was later put in my dissertation on less than a page.

My stay in Los Angeles was otherwise quite pleasant. I rented a small cottage in Brentwood with an orange tree outside. Socially I saw especially Don Kalish quite often, who among other things opened my eyes to what was happening in Vietnam. He also introduced me to Rudolf Carnap, who had recently retired from UCLA and now lived in Santa Monica.

This was a time when in my opinion it was still preferable to cross the Atlantic by boat, and in connection with the two voyages I stayed in New York for quite a while. I also went to New York to attend a meeting of the Association for Symbolic Logic, at which I presented a proof of the normalization theorem for classical logic, choosing this time a formulation with the law of the excluded middle as an axiom. I included some applications to ideas suggested by Fitch. In this connection, Church taught me a lesson: never *postulate* proofs to be normal—what use could there be of a system without free access to *modus ponens*; I think he was very right.

In New York I was kindly invited to Martin Davis's house. There I also met Raymond Smullyan, who told me enthusiastically that he was very taken with my modification of Beth's semantic tableaux used in my first paper on mechanical proof procedures and intended to develop it further in the text book that he was planning. This topic was now very far from my mind, and I was sorry that I could not really share his enthusiasm.

Back in Stockholm in the autumn, it was high time for me to publish my dissertation and to get my degree—there was only a year left of my scholarship. I put together what I had ready, including several extensions of my results to other languages than the ordinary first order ones. The dissertation had to be available in printed form in good time before it was to be defended at the public disputation, and it would take some time to get it printed with the technique of that day. But I was still hesitating about what form I should choose for the system of classical logic. Shortly before the deadline for handing in the manuscript to the printer, I changed the system from one where the nodes of the deduction tree were assigned sequences of formulas interpreted disjunctively to the one that appears in the monograph. This was certainly

a wise move, although it does not do justice to the duality of the logical constants when interpreted classically.

The disputation took place on the very last day of the spring term of 1965. Kanger was the faculty opponent. To reduce the tension of this public event, he told me the main points of his opposition the day before. But he left one surprise. In summing up his evaluation, he made the complaint that I had not seen, or had chosen not to see, that my main result could very easily be obtained from Gentzen's Hauptsatz for the sequent calculus. At this point, my future first wife, who was sitting with Christer Lech in the audience at the first row, pointed emphatically at the beginning of the dissertation, obviously wanting to encourage me to refer to it. But I was dumb with astonishment. Not only had I indeed written on the first page of the preface that the normalization theorem was equivalent to the Hauptsatz, but in an appendix, I had even shown in sufficient detail how it could be derived from the Hauptsatz. Kanger's accusation was too absurd, and in addition, it revealed that he had missed or did not want to accept what I considered to be the main point of my thesis. It was not the custom to respond to what the opponent said in his summing up. So I remained silent. But sometimes I still regret that I did not give a quick, crushing reply.

2.5 Docent at Stockholm and Lund—Visiting Professor in US

The Swedish doctorate at this time corresponded to a German Habilitation or a French doctorat d'État. Provided that the dissertation was considered to be sufficiently good, it gave the doctor the desirable position as docent for six years, if there was such a vacant position at the time in question. The docentship in theoretical philosophy was already occupied by Jan Berg, who had got this position for his dissertation on Bolzano a couple of years earlier. But the one in practical philosophy was free, and I was allowed to "borrow" it as long as there was no one qualified for the position in that subject.

I could do so for two years. Then my good friend Lars Bergström had completed his dissertation in practical philosophy. He asked me to be his opponent at the disputation; according to an old tradition there should be, besides the faculty opponent, a second opponent chosen by the author. This was the immediate reason for my engagement in a critical study of utilitarianism, which resulted in several papers in the next years. He got his doctorate and took over my position. To my luck, Sören Halldén, who had recently been appointed to a professorship at Lund University, then offered me to become a docent there.

The position gave "protected space", to use a vogue term in education policy for naming a phenomenon that is now rare. The holder of the docentship could engage in research at will without the need to write any further applications or reports. The teaching load was small and could be fulfilled as seminars connected with the research.

This was an opportunity to move to the countryside. I married Louise Dubois, whom I had known for many years and to whom I had dedicated my dissertation.

We bought a country house located in a rural area south of Stockholm at a distance of less than two hours travel by train or car, and settled down there. It was a grand villa, built as the main building of an estate, but now partitioned from it. There were a few neighbours, a lake, which the villa had a splendid view of, open fields at one side, and deep forests at the back—a perfect place for a peaceful life as a docent in philosophy. I needed to go to Stockholm only for one day a week. When I got attached to Lund, to which the distance was greater, I went there occasionally for short periods.

I now took up themes that I had worked on as a doctoral student but had not been able to include in my dissertation. I first turned to questions concerning various relations between classical and intuitionistic logic that I found useful to approach proof-theoretically. As for the translation of intuitionistic logic into classical logic, it seemed likely that the Gödel-McKinsey-Tarski interpretation of intuitionistic sentential logic in classical S4 could be extended to predicate logic by using the normal form theorem that I had established in my dissertation for classical S4 with quantifiers. I gave this problem as a task to Per-Erik Malmnäs, an able student whom I had got to know when I taught logic courses as a graduate student. He wanted to write a graduation paper, and solved the problem quite quickly. We decided to write a joint paper “A survey of some connections between classical, intuitionistic, and minimal logic”; it should be said that Per-Erik did the hard work. I presented part of the paper at a logic colloquium at Hannover in August 1966 organized by Kurt Schütte, who invited us to include the full paper in a volume that he edited. From the conference I otherwise recall Peter Aczel, the only person there of my age, with whom I was glad to speak.

Next I returned to the problem of extending the normal form theorem to second order logic. Having no idea of how the reductions could be shown to terminate, I chose to approach the problem model-theoretically, which was most conveniently done by trying to prove the Hauptsatz for second order sequent calculus; in other words, Takeuti’s conjecture. As had been noted by Kanger and Schütte, the Hauptsatz for first order logic follows immediately from their kind of completeness proof. They had both tried to extend their results to second order logic but had not succeeded in that attempt so far (Kanger had announced such a result, but no proof had been forthcoming). In a vaguely nominalist spirit, I had interested myself in models where the second order variables range not over all relations between individuals but only over definable ones. In these terms, the problem that appeared when trying to extend Kanger’s or Schütte’s result to second order logic could be described by saying that the counter model that one gets for an unprovable sentence by taking an infinite branch in an attempted cut-free proof might not be closed with respect to definability. It had seemed to me for some time that the natural way to overcome this problem should be to extend the second order domains of the obtained counter model by adding the relations that could be defined in terms of the relations already contained in the domain. This operation might have to be repeated a number of times along an initial segment of ordinals, but eventually the domain must become closed under definability.

When I finally tried this idea in the spring of 1967, it worked out fairly immediately to my surprise; I had expected greater difficulty in view of the fact that Takeuti’s conjecture had been open for quite a long time. First I was afraid that I had overlooked

something, but soon I had a complete proof, and in June I had written it up in a paper “Completeness and Hauptsatz for second order logic”, which I submitted to *Theoria*.

The question now arose whether this result could be extended to higher logic. Later in the summer, I took up this question, and it turned out that the same construction did not quite work for higher types. To overcome the new problems, I made the interpretation non-extensional, and found a way to extend the given model in order to make it closed under definability by a more complicated operation performed once, instead of repeating an operation a transcendental number of times as in the previous proof. With some modifications like this, the proof went through. Quite satisfied with this, I wrote a new paper, “Hauptsatz for Higher Order Logic”, which I sent to Schütte and submitted to the *Journal of Symbolic Logic*.

Shortly afterwards, I went to the third International Congress in Logic, Methodology and Philosophy of Science (LMPS), which was held in Amsterdam at the end of August. There I found out that in the autumn a proof of the Hauptsatz for second order logic, using a method different from mine, had appeared in a paper by William Tait. Furthermore, I met Schütte who told me that a few months earlier he had received an unpublished paper from a young Japanese logician Moto-o Takahashi containing essentially the same proof as mine of the Hauptsatz for higher order logic. Schütte nevertheless recommended my paper for publication in view of some sufficiently interesting differences between the proofs. He also arranged that I could present my proof at a session that had a gap.

The entire Congress took place at a hotel in the centre of Amsterdam, Krasnapolsky, and most of the participants also stayed in that hotel, which facilitated exchanges and created a nice feeling of homeliness. There I met Georg Kreisel who urged me to investigate whether a similar proof of the Hauptsatz could be worked out for second order intuitionistic logic. I was only moderately interested in this problem but looked at it anyway when I had returned home. It turned out to be fairly easy to give a proof that followed the same pattern as the one for classical logic, using Beth’s interpretation of intuitionistic logic generalized to second order, which turned out to be a special case of Kripke’s interpretation when similarly generalized. I presented the result at the Conference on Intuitionism and Proof Theory, held at Buffalo in the United States in August 1968.

I was more interested in taking up a third theme from my pre-doctoral period, the idea of interpreting intuitionistic logic in terms of constructions denoted by terms in an extended lambda calculus. I had not worked on it after presenting it at my seminar series in Los Angeles in 1964. But now in September of 1968, there was yet another conference, the First Scandinavian Logic Symposium, held at Åbo in Finland; one of Stig Kanger’s many good initiatives. It seemed to be a suitable occasion to present this idea, but I regretted that I had not paid more attention to it in previous years and that I had not succeeded in giving it the presentation I had wanted and that it deserved. I returned to the subject in a little more detail two years later at the Second Scandinavian Logic Symposium held at Oslo, but then it would be another 30 years before I took it up again in a different context.

The second semester of the academic year 1968–69, I spent as visiting professor at Michigan University in Ann Arbor. One may wonder why, and I have no real answer

to that question. The invitation came from Irving Copi, whose system of natural deduction I had written a very short note about. But this was a very minor thing, and the department at Ann Arbor did certainly not have its strength in logic. Its staff had a good reputation though, especially for their competence in moral philosophy, which I had some interest in. The philosophical dedication of the staff was impressive, demonstrated among other things by their gathering at the house of a member of the faculty every Sunday afternoon to listen to a paper one of them had newly written.

The subsequent academic year, I was to spend at Stanford by invitation of Kreisel. I had bought a car in Ann Arbor, and at the beginning of the summer I drove from there to Palo Alto. In Chicago I picked up Per Martin-Löf. I had met Per in December 1965 for the first time on the initiative of Christer Lech, who had asked me to attend a session of his seminar where Per was to make a presentation. After that we saw each other very occasionally to begin with. But Per had a growing interest in logic. We went together to the LMPS Congress in Amsterdam and to the Buffalo conference, and we soon exchanged ideas quite frequently. Now he accompanied me on the drive to Stanford, and stayed with me there for the summer.

Per had been a year at the University of Illinois in Chicago by invitation of Bill Tait. There he had also got to know Bill Howard and had learned about his idea of formulas-as-types. Howard had first connected his idea with Gentzen's sequent calculus, but Per saw that it was more fruitful to relate it to my work on natural deduction that I had told him about. By slightly modifying Howard's idea, one got an isomorphic imbedding of intuitionistic natural deductions into an extended typed lambda calculus, whereas mine was homomorphic.

From this time, Per was a great supporter of my perspective on Gentzen's work, and he soon came to develop it further. One of his first contributions was to carry over Tait's notion of convertibility to natural deduction. It gave a new method for proving that the reductions that I had defined terminate in normal deductions. A second contribution was that he extended the idea of introduction and elimination rules to concern not only the logical constants but also the concept of natural number and more generally concepts defined by induction or by generalized induction, and obtained a normalizability result for the theory of such concepts. He came to present these results the next summer at the Second Scandinavian Logic Symposium in Oslo. A third idea of Per's concerned questions about the identity of proofs that we discussed together. If a deduction reduced to another, the corresponding lambda terms denoted the same construction, as I had already noted, and it seemed reasonable to think that the deductions then denoted the same proof. Per suggested that something like the converse of this was also true, more precisely, that two deductions represent the same proof only if one could be obtained from the other by a sequence of reductions and their converses. I liked this idea and came to refer to it as a conjecture of Martin-Löf.

At Stanford I was glad to get to know Solomon Feferman, who attended a series of seminars on my work on natural deduction that I gave during the summer term. Georg Kreisel came to Stanford in the autumn. My contacts with him had been limited, but we were soon on very good terms, had a joint seminar, and saw each other regularly for hours several times a week. He, too, now became convinced of my perspective on

Gentzen's work and became an enthusiastic supporter. As he put it when he wrote a long, critical review in the *Journal of Philosophy* of Szabo's translation into English of Gentzen's collected papers: "I was slow in taking in this part of Gentzen's work. ... As I see it now, guided by D. Prawitz's reading of Gentzen, the single most striking element of Gentzen's work occurs already in his doctoral dissertation". This striking element consisted in "informal ideas on a theory of proofs, where proofs are principal objects of analysis, and not a mere tool".

He was especially interested in the significance of the reductions by which a deduction d is transformed to a normal deduction $|d|$ and in the ideas about the identity of proofs that I had discussed with Per. To give what he called "the flavour of the potentialities of a theory of proofs", he referred to my result about normalizability, which he wanted to give the following "succinct formulation":

To every deduction d there is a normal deduction $|d|$ that *expresses the same proof as d* .
(Thus normal derivations provide canonical representations, roughly as the numerals provide canonical representations for the natural numbers.)

Of course, Kreisel also enriched my perspective and fused it with his own elaborated logical views. His reflections on these themes were likewise to be presented at the Second Scandinavian Logic Symposium in a long paper, "A Survey of Proof Theory II". Here he expressed certain reservations concerning the idea of the introduction rules as meaning constitutive, but nevertheless a main theme was the significance of the reductions in natural deduction.

These discussions with Martin-Löf and Kreisel were a great stimulus to me and made me again focus on the theme of my doctoral dissertation. During my time at Stanford I started to write a paper that would bring that theme up to date and give a survey of what I was now calling general proof theory, as opposed to reductive proof theory. It came to be called "Ideas and results in proof theory", and it too came to be presented at the Second Scandinavian Logic Symposium. The greater part of it was finished when I left Stanford in March to go to my country house in Sweden.

Kreisel came there at the beginning of June to stay with me before we went on to Oslo for the symposium. He brought news about a problem that we had much discussed at Stanford and that I had also discussed with Per: how, if at all, could one establish for second order logic, not only that to any deduction there is one in normal form that proves the same, but also that this normal deduction is obtainable by the standard reduction procedure—to use a terminology that Kreisel suggested, one wanted a *normalization theorem* not only a *normal form theorem*. Kreisel's good intuition had led him to think that a graduate student in Paris, Jean-Yves Girard, whom he had been supervising in the spring, was working on something that was relevant to this question and now had a result that may yield a positive solution of our open problem. He had actually sent me a paper by Girard in advance, which I had misplaced and could not now find. Unfortunately, Kreisel could not explain Girard's idea, so we could not settle its applicability.

The symposium in Oslo, organized by Jens-Erik Fenstad, was given a wider format than the First Scandinavian Logic Symposium since several non-Scandinavians

were also invited. It became an important event in proof theory because of the many contributions in that field. Girard was not coming to the symposium but another of his supervisors, Igor Reznikoff, was present and volunteered to explain Girard's idea. Unfortunately no one understood much of the explanation, except that Girard had made some kind of construction using impredicative quantification which by instantiation gave a computability predicate of the right kind for the system of terms that he had constructed. When I returned to my country house after the symposium, Igor accompanied me, and staying with me for a while, he tried to explain Girard's idea a little further. The idea was still mysterious to me, but the idea of an impredicative construction was suggestive. When Igor had left I sat down and was able to work out a proof of the normalization theorem. Very satisfied that this old problem had now been solved, I wrote a letter to Per at the beginning of July telling him what I had found. He was making a sailing trip, but back at home he replied that he had found the same proof. I included my proof as an appendix to my paper in the proceedings from the Oslo symposium, while Per presented his proof in a separate paper that was also included in the proceedings. Girard contributed a paper to the proceedings where the application of his idea to natural deduction was explicitly worked out.

Among the achievements in general proof theory during these years, I valued most highly the new method for proving normalizability of natural deductions that was obtained by Martin-Löf carrying over Tait's notion of convertibility to them. I modified the method so as to obtain strong normalizability, that is, the result that any sequent of reductions regardless of the order in which they are made terminate in a normal deduction. What I especially liked about this notion, which Martin-Löf had called computability, was that it seemed to allow one to make precise the idea that reasoning which proceeds by introductions preserves validity in virtue of the meaning of the logical constant in question, while reasoning by eliminations is justified or valid because of being reducible to valid reasoning as defined by introductions. In accordance with this view, which was presented in an appendix to my paper to the Oslo symposium, I renamed the notion and called it validity.

In my Oslo paper, validity, or rather several different notions called validity, was defined for derivations of various formal systems. However, this was not what I really wanted. The desideratum was to define validity for reasoning in general—to define it for derivations, all of which then turned out to be valid, was like defining truth for provable sentences instead of for sentences in general.

In the academic year after the Oslo symposium I was back as docent at Stockholm University; Jan Berg had got a professorship at the Technische Hochschule in München, and I could take over his position as docent. I now wanted to extend the definitional domain of the notion of validity so as to cover reasoning in general, which I represented by what I called arguments. They were made up of arbitrary inference steps arranged in tree form. To each inference step that was not claimed to be meaning constitutive there was to be assigned an alleged justification in the form of an arbitrary reduction procedure. The notion of validity was now worked out for such arguments in my paper "Towards a foundation of a general proof theory". It was presented the next summer at the 4th LMPs Congress, which was held in August-September at Bucharest, and to which I had been invited to give a lecture.

That summer my term of six years as docent was at an end. My future as an academic philosopher would then have been very uncertain, if I had not just won a permanent position as professor of philosophy at Oslo University.

2.6 Professor of Philosophy at Oslo University—Fellow at Oxford

The appointment of a professor at a Scandinavian university was a serious affair at that time, particularly in Norway, and especially if it was a professorship in philosophy. The procedure started with appointing a committee, which worked for about a year to rank the applicants. Then the whole faculty, in this case the faculty of Humanities, was to give its opinion, the Rector of the university was to make his decision, and finally the government was to make the appointment. In my case, the committee was deeply divided about whom to rank first. When it had delivered its publicly available report, thick as a book, a general dispute broke out, which took place among other things in the main daily newspapers of the country.

The position to which I had applied was vacant after Arne Næss, who had been *the* professor of philosophy at the university for 30 years; he was a kind of icon, held in high esteem. A number of people were of the opinion that I was not worthy to succeed him. I was too specialized and too young; in one newspaper it was said that a quickly raised broiler should not succeed Næss. Altogether there were 21 long newspaper articles that discussed the issue. Of them 18 were against and 3 were in favour of my appointment. Nevertheless, the faculty put me first and I was appointed. Many were indignant. I had a former girl friend, now living in Oslo, who warned me about accepting the position in view of the general opinion.

I ignored the warning, and in the summer I went to a big meeting held for several days in the Norwegian mountains and organized by students. Most of them had been active in the student revolts at the end of the 60s, and most of them were against my appointment. I survived unhurt, and when I later took office at the philosophy department all were friendly. As I happened to find out later by reading a protocol, one professor had suggested at a meeting of the department held after I had been appointed that they should write to the faculty to say that they had no room for me because Næss's office had to be converted to a tearoom. But soon he was to propose that we should run a series of seminars together, and so we did. My great support at the department was Dagfinn Føllesdal, who had proposed that I should apply for the position, and with whom I had many joint seminars in philosophy of language.

I liked living in Oslo but it was a new life for me in many respects. My wife did not accompany me to Oslo, and we were eventually to separate and to divorce. As professor I had several new obligations. Philosophy was a bigger and more important subject in Norway than in Sweden, which had its ground in the fact that all students in all faculties had to study philosophy in their first year at the university. Being a non-native of Norway, I was not expected to know how things were run at the university, and I cannot say that I was burdened with many administrative duties. But to come up to expectations I found it important to cultivate other interests that I had

in philosophy besides logic. One such interest was the concept of cause, about which I wrote some essays, stimulated by writings of Georg Henrik von Wright, who even visited the department for a month. I also wrote long reviews of philosophy books for a daily newspaper; for instance, I recall devoting much time to John Rawls, *A Theory of Justice*.

For all that, I still pursued logic but now with less intensity. In 1974, I gave a course in proof theory at the Summer Institute and Logic Colloquium at Kiel. I also presented a paper at a concurrent symposium in honour of Kurt Schütte in which I looked at Gentzen's symmetrical sequent calculus from a classical semantic perspective. The point was that the calculus could be seen as defining a semantic notion in such a way that the completeness property could be expected as a feature of this notion. But I tried to do too many things in the paper, and the point was lost in a multitude of definitions and results. Part of the paper had been stimulated by discussions with Georg Kreisel, with whom I continued to have good contact. Our roads sometimes more or less crossed in various parts of Europe, and he then suggested that I visit him. Occasionally, we spoke German with each other. Since as a Swede I found it strange to use the formal *Sie* when knowing each other so well, we agreed to use the singular *du* and our first names when addressing each other, an informality that he practiced with very few persons, he said. We continued reading and commenting on each other's papers. But it was clear that his former enthusiasm, not to say over-enthusiasm, about my work gradually cooled off. Some time after my six years at Oslo, his comments started to be rude. Knowing his character, I was not offended, but I found it better to drop the contact. We seldom saw each other after that. An exception occurred much later when he came up to Oxford a couple of times to visit me when I was staying there for the summer of 1995, borrowing Daniel Isaacson's house.

Two new contacts outside of Norway that I made during these years were especially important to me. One was with Oxford and Michael Dummett, and the other was with Italy and Italian logic. I was invited to give a talk at a meeting at Santa Margherita Ligure in June 1972, where I presented in a more accessible form some of the ideas of a general proof theory that I had set out at the LMPS Congress in Bucharest the year before. It was a small Italian conference—the other foreign guests were Arend Heyting, Charles Parsons, and Gert Müller—and was my first real contact with Italian philosophy, besides having got to know Ettore Casari in my visits to Münster ten years earlier. Casari came to the meeting with some of his students, among them Daniela Montelatici and Giovanna Corsi. From Florence there was also Marisa Dalla Chiara, and from Rome came Carlo Cellucci. All of them I came to know very well in the succeeding years. I had not been aware before of the great interest in logic within Italian philosophy. It was an interest that continued growing, and it embraced proof theory of the kind that engaged me. I came often to return to Italy for this reason.

The conference in Santa Margherita was one of these nice, relaxed, and yet fruitful meetings that Italian philosophers often organize so well according to my experience: there was generous time for presentations and discussion, but there was also time for excursions at sea to Portofino, for walks, for hearing Marisa singing together with Giuliano Toraldo di Francia, and for a splendid dinner at the home of Evandro Agazzi, who was the organizer of the meeting.

But the meeting is most memorable to me for a personal reason. There I met my future second wife, Daniela Montelatici, who at that time was at the end of her university studies. She was the main reason for me to return to Italy frequently. Three years later we went together to Oxford for a year.

I was allowed to take a sabbatical from Oslo for the academic year 1975–76, and chose to spend the year at Oxford. Michael Dummett and Robin Gandy had very warmly welcomed me coming there when I told them about my intention. I had met Robin Gandy at the conference in Hannover in 1966. He was to me then just a tall man with a strong Oxford accent—when passing me in the hallway of the venue, he said very briefly but encouragingly enough: “hello, I liked your *Natural Deduction*, that is the way to present Gentzen”. Later he invited me to Oxford several times, and now we got to know each other very well.

Michael Dummett was a new acquaintance. I had first met him at a conference at Oberwolfach in 1974. At that time I did not know anything about his work, but I soon understood that we had converging interests. Some of his ideas of a theory of meaning could be seen as a generalization of Gentzen’s idea of natural deduction; in particular, his talk of two aspects of language, the conditions for appropriately making a statement and the conclusions that could be appropriately drawn from a statement, that must be in harmony with each other, as Dummett put it. This idea amounts within logic to nothing other than what I had called the inversion principle (following Lorenzen) for introduction and elimination rules, on which I had based the proof of the normalization theorem for natural deduction. My reason for wanting to go to Oxford was especially that I wanted to learn more about what I saw as Dummett’s extension to language in general of a logical theme that I had been engaged in.

It was arranged that I should be a fellow at Wolfson College since it was considered important to have an affiliation of this kind. To begin with we rented Philippa Foot’s charming three-store house at Walton Street while she was in California for most of the year. When she returned, Robin Gandy was going away for the rest of the academic year and offered us his apartment at Wolfson College.

The time for my stay in Oxford was very appropriate. Some of Dummett’s main contributions to the field that I was interested in, “What is a theory of meaning? (II)” and “The philosophical basis of intuitionistic logic”, were published that academic year. Furthermore, Per Martin-Löf also came to Oxford for the autumn of 1975 with a fresh and intensive interest in theory of meaning. He gave a series of well attended seminars at All Souls at which he rejected the idea that introduction rules or conditions for making a statement are meaning constitutive. Instead, it is the elimination rules or the rules for drawing conclusions from a statement that are meaning constitutive, he argued, and outlined an entire meaning theory for a substantial part of mathematics on this basis. Thus, there were a lot of issues to discuss at Oxford.

I was very impressed by Dummett’s papers although I did not agree with all of his ideas. My occupation with them gave my philosophical thinking a new direction. Per’s seminars were also quite impressive, but I stuck to the opposite view that it is the introduction rules that determine the meaning of the logical constants and that this idea was the one that was to be extended to mathematics and language in general. I started to work on my own version of this view. A first result was the paper “Meaning

and proofs”, which was finished before I left Oxford. Per came a little later to realize that the meaning theory that he had presented in his seminars did not work. When he worked out his type theory in detail, he concurred with me that it is the introduction rules that have to be meaning constitutive in a meaning theory for mathematics.

Michael Dummett had discussed two possible meaning theories, one that identified the meaning of a sentence with what it takes to verify its truth, and one that identified it with what conclusions can be drawn from its truth, calling the first verificationism and the second pragmatism. In the spring of 1976 we ran a series of joint seminars discussing this and other meaning theoretical issues. Our discussions were to continue in subsequent years at a number of conferences, in proceedings, in three books devoted to Dummett’s philosophy, and in one volume devoted to philosophical ideas of mine. I visited Oxford again many times, and Michael accepted several invitations to come to Stockholm after I had returned there from Oslo. As to the choice between verificationism and pragmatism, he came to lean on the whole to the former, later relabeling it justificationism in which the basic concept was justification of an assertion rather than verification of the truth of a sentence. In *The Logical Basis of Metaphysics* published 1991, he devoted some chapters to what he called proof-theoretical justifications of logical laws, which he wrote originally soon after my stay in Oxford. There he discussed critically, but in the end mainly with approval, meaning theories of the kind that I had tried to base on the notion of a valid argument taking introduction rules as meaning constitutive. But he also returned to a notion of valid argument based on elimination rules, somewhat surprisingly referring to Martin-Löf, not noticing that he had given up that project because the idea does not work if one tries to extend it beyond first order predicate logic.

When I was in Oxford, Anders Wedberg was granted a pension a few years before the regular retirement age because of bad health, and his chair was advertised vacant. I applied and at the end of 1976 I was appointed. The appointment procedure was less dramatic than the one at Oslo, although Per Martin-Löf, who had also applied for the position, was a strong candidate. I stayed in Oslo for the academic year 1976–77, not to leave my position there too abruptly. Daniela stayed in Florence. In the spring we visited Belgium and France together. A group at the University of Leuven had invited me to give a series of lectures and treated us with great hospitality, showing us around in Belgium. We also saw the newly erected Université de Louvain at Louvain-la-Neuve and were shocked by the effects of language conflicts. We then went to Paris. Jules Vuillemin had invited me to give a lecture at Collège de France. I had not understood before what a prestigious institution this was. At the reception afterwards, at which I was given a medal, I felt that some of the faculty members considered me too young for this honour; if I had known before of this medal, I might have been able to give a better lecture in order to feel worthy of the medal.

This year I engaged myself in the on-going discussion about atomic energy, which was especially intensive in Sweden because of a decision that was to be made about whether to build further nuclear power stations. Together with Jon Elster I arranged a series of seminars about the philosophy of risks. I came to be member of a subgroup of the Energy Commission that the Swedish government appointed and that had the task to give a foundation for a national policy on current issues about energy production.

Our task was to study principles of balancing different kinds of risks, especially when it was a question of events whose occurrence had low probability but whose effects would be catastrophic. We were impressed by a principle for decision-making referred to in insurance business as the notion of Maximum Probable Loss, which we understood as the principle that one should not always choose the alternative with the highest expected utility but should refrain from such an action if there were non-negligible probabilities for negative consequences of a magnitude above a certain limit, for instance leading to the non-survival of the agent. In my opinion, an application of this principle, which I found most reasonable, gave the result that one should not build nuclear power stations of the kind available at that time. Arguing for this view, I came to participate a little in the subsequent political debates in Sweden. Today I am less sure of this view because of the risk of climate catastrophes when using alternative energy sources.

2.7 Back to Stockholm—Chairman of the Department

I moved to Stockholm in the autumn of 1977. Daniela made the big decision to leave Florence and to move to Stockholm with me. We married, and after some years we had three children in rapid succession: Camilla, Erik, and Livia. Daniela decided that it was enough with one philosopher in the family and switched her subject to psychology. Parallel with giving birth to three children, she started a new, long university education to get a degree in psychology. To this she added a considerable period of training to become authorized as a psychoanalyst. In the 90s she could open her own psychoanalytical clinic, in which she is still working. I am glad to have been able thanks to my previous acquaintance with psychology to follow her professional career a little, and that she has been able to understand something of what I have been doing thanks to her previous studies in philosophy.

The Department of Philosophy at Stockholm University, in particular Theoretical Philosophy, had suffered a period of great weakness in the years before I got back there. No one had taken a doctorate in Theoretical Philosophy after I had done so in 1965. Anders Wedberg had been absent for long periods because of illness and because of a research project that he was engaged in. The number of students had grown a lot in the 70s. However, most of the teaching was done by a group of graduate students who had been doctoral students for years without seemingly getting any closer to a degree, much to the amazement of the rest of the Faculty of Arts; even the director of studies was such a graduate student. In addition there had been difficult conflicts between these students and the few permanent teachers. The latter tried to stop the graduate students from governing the department, which was not easy, among other reasons because of the so-called student democracy that had been introduced in the 70s. Students had been given a great say at all levels of the university, in particular at the departmental level, and together with the representatives of the administrative staff, they could even have a majority vote on the board of the department. I was told horror stories about meetings of the board, which could go on for more than ten

hours with a short adjournment for dinner. The director of studies, who was in fact sometimes governing the department, and who had a seat at the faculty level too, even tried to stop my appointment, proposing that the position should be replaced by less expensive positions at a lower level. Gaining no hearing for that proposal, he left the department before I arrived.

Obviously I had to take on the responsibility for revitalizing theoretical philosophy at Stockholm University. I took on the task as chairman of the department for the first ten years. Everyone at the department was tired of the long period of conflicts, and the department meetings now ran fairly smoothly; after a while the problem was rather to get the elected representatives to participate in the meetings. The administrative duties consumed time but were not otherwise a problem. The challenge was to extend the teaching staff and to get the doctoral students to take their doctorates.

The regular teaching staff in theoretical philosophy was still very small. There was one position as docent, held by Alexander Orłowski, who had succeeded me, and a half position as lecturer besides my own position. Orłowski had been docent for almost six years and got another position when he reached this time limit. The position as docent came to be filled by a succession of very competent people from other universities, one after the other. They were in order Mats Furberg, Dick Haglund, Göran Sundholm, and Dag Westerståhl. After some years in Stockholm, each of them became professor at another university. The eminent institution of docentship was then unfortunately abolished, but at that time our staff had grown considerably.

The system of higher education was reformed in Sweden in the 70s. Among other things the former doctoral degree was gradually replaced with a doctoral exam, which was meant to be less demanding than the former degree. Many teachers were critical of the reform and saw it as a lowering of standards. However, another important ingredient in the reform was that dissertations for the doctoral exam were to be assessed only with respect to whether they were accepted or rejected, while the ones for the degree had been graded essentially along the old Latin scale of honours. The requirement for getting a low, passing grade had not been very high, and it is doubtful whether it had been higher than the requirement for passing the new doctoral exam. But to get such a low grade had been considered to be a failure, worse than not to take the degree at all, and this was certainly a major reason why candidates postponed the presentation of their dissertations. In view of the situation at my department, the reform seemed to me reasonable.

But most graduate students are self critical, and in spite of the reform, it was not very easy to get candidates to take their doctorates. New candidates with grants sometimes succeeded in taking their exam in almost the expected time of four years. Among the first of them was Luiz Carlo Pereira, who came from Brazil to study proof theory with me, and then returned to Brazil where he later got a professorship. Another was Lars Hallnäs, whose dissertation extended the normalization theorem to a version of set theory, and a third was Torkel Franzén, who wrote a dissertation defending realism in mathematics. The two got positions at a newly started research institute for applied computer science, where I was also engaged as a consultant for some years. Slightly later Peter Pagin wrote a doctoral dissertation on rules and their place in the theory of meaning. He stayed at the department and became lecturer.

For older candidates who had already used their grants it was often more difficult to find time to complete the dissertation at the side of other work. One of the first of them to take his exam was my old student Per-Erik Malmnäs, who had switched from proof theory to the philosophy of probability while I was in Oslo. Closely following him came Gunnar Svensson, whose dissertation was on Wittgenstein's later philosophy. Both of them got positions as lecturers in the department. Gunnar Svensson was also appointed director of studies and assumed the responsibility for a lot of administrative business, which was a great help to me.

At about the end of my first 10 years at the department there had been altogether ten doctoral exams; not a great number perhaps, but a definite improvement. At this stage the teaching staff had grown considerably, and gradually I could share supervision of graduate students with colleagues. My own teaching was mostly within logic and philosophy of language in the form of courses at undergraduate level as well as seminars at graduate level. I was especially pleased to be able to gather most of the people working in theoretical philosophy at a joint higher seminar once a week. It was satisfying to see the department growing and to regain health, but it did not give me much time for other things.

2.8 New Tasks

My old friend and fellow-student Lars Bergström now came back to the department as professor of Practical Philosophy and took over my task as chairman. Numerous other duties and undertakings were added instead. The department had for a long time been quite isolated within the faculty and had been looked upon with suspicion. To change the situation I engaged myself at the faculty level too, and was elected Vice Dean for two periods; I steered away from being a candidate for the position as Dean, but was instead on the Board of the University for one period. A more demanding task that I took on was to be a member for six years of the Swedish Research Council for the Humanities and Social Sciences. I was the chairman of two priority committees that had to evaluate applications for research projects within certain areas, and for a period I was vice president of the entire council.

Another demanding undertaking was to organize the 9th International Congress of Logic, Methodology and Philosophy of Science in 1991. Stig Kanger had volunteered to organize this congress and this was confirmed at the 8th LMPS Congress held in Moscow in 1987, but he died tragically in 1988 without having taken any measures to make such a congress possible. I had not much choice but to take over the responsibility—I had participated in all the LMPS Congresses since the one in Amsterdam in 1967 except one, and on the whole it seemed to me a worthwhile institution. The first challenge was to raise the considerable amount of money that was needed. I was fairly successful in this—it helped that I had become a member of the Royal Swedish Academy of Sciences and of the Royal Swedish Academy of Letters, History and Antiquities, and of course my affiliation with the Research Council also helped. It remained to actually organize the congress, a task that I was

glad to share with Dag Westerståhl, who did much of the hard work as secretary of the organization committee.

Among the most time consuming tasks were commissions as expert in conjunction with appointments. Swedish philosophy and the Swedish university system in general were in rapid expansion for several decades in the last century, but the number of professors in most subjects, in any case in philosophy, was small and constant. The demanding procedure that was applied in the Nordic countries for filling academic appointments (and is still more or less in place) required a committee of professors who evaluated all the works done by the applicants, an evaluation that was to be openly accounted for in a lengthy, public document. There was always a new or vacant position to be filled, and the few professors were constantly called upon as experts. Even when I was abroad on a sabbatical, large boxes full of books and papers could arrive that had to be evaluated for some appointment.

There were two extensive tasks that I quite willingly took on during these years. One was to be the director of the publishing house Thales. Swedish philosophy had been given a quite generous donation to be used for translations of philosophical literature into Swedish. Money was allotted to translators by decision of all professors of philosophy in the country at a joint meeting. We had found however that in spite of the translation being paid for in this way, commercial publishing houses were often unwilling to publish the book in an acceptable way. We were also dissatisfied with their failure to keep books in stock that we wanted to use at the universities. To improve the situation we decided bravely to take the matter in our own hands. On the proposal of Stig Kanger, we formed a foundation that was to carry on a non-profit-making publishing house. It was named Thales, and I assumed the task as its director.

Thales started in 1985 with a negligible initial capital. To register a foundation a donation is necessary, so we six present when the foundation was formed donated 100 crowns each, and this was our whole capital! Business was accordingly very slight to begin with. But we were able to get some small grants from other sources and could publish some of the translations that were already paid for. Furthermore, we were allowed to take over from commercial publishing house the rights of some translations that they did not want to republish. Some of these continued to sell very well, such as Wittgenstein's *Tractatus* and *Philosophical Investigations*, which Anders Wedberg had translated in the 60s and 70s. Thanks to a bank loan we were also able to take over philosophy books from another publishing house that had gone bankrupt. After ten years we had published around 50 titles and were selling 40 additional titles that we had taken over.

I stayed as director for an additional ten years, and the publishing house continued to grow. We translated a great number of classical texts, from Aristotle to Frege and Husserl. My last year as publisher coincided with a Kant anniversary, 200 years having passed since his death, at which we came out with translations of all of Kant's critical works; they had not appeared in Swedish before. But we also translated contemporary philosophers like Derrida, Dummett and Davidson, brought out original works by Swedish philosophers, and published three journals.

In the last years my work as director really amounted to a part time job. I liked the varied activities as a publisher of philosophical books. In a small publishing house

like Thales they were often of a very practical nature. It was not only a question of accepting manuscripts and finding good translators and editors. It was equally important to choose suitable printers, to develop a good system of distribution, and above all to make both ends meet. Economically this non-profit-making enterprise was very successful: the year I left, its net income was 1,000 times the initial capital. The point of the activities was of course to make classical and modern philosophy available in the Swedish language, and thereby contribute to maintaining and developing a Swedish philosophical vocabulary. In a small language area like the Swedish-speaking one, this does not come about by itself, but is of great importance, it seems to me, since thinking is most efficiently done in one's native language.

Another task that I quite willingly took on was to be president of the Rolf Schock Foundation. Rolf Schock and I were fellow-students from 1961. Both of us worked on doctoral dissertations in logic in the Stockholm philosophy department, but he presented his dissertation at Uppsala University some years after I got my degree. We were on quite good terms. He liked to take up philosophical questions of a logical kind for discussion. I was the Faculty Opponent when he was to defend his dissertation. Then our ways parted, and when I came back to Stockholm I had the unpleasant task of assessing his work when he applied for various positions, which he never got, and which always led to his appeal to higher levels, and to a certain bitterness against me from his side. He lived a quite simple life, taking various jobs, and I felt bad about not being able to offer him an academic position. In 1986 he died in an accident. To the surprise of most of us it turned out that he had a considerable fortune, half of which he wanted to donate to prizes that were to be given by Swedish academies, among them the Swedish Academy of Sciences. A foundation was formed for this aim and I was asked to represent the Academy on the board of the foundation. In this capacity I also became president of the foundation in accordance with its rules.

The first thing we had to do was to bring the capital, which came from the sale of real estates in Berlin, to Sweden. However, before we were able to do so, Rolf's mother succeeded in freezing the money in Germany. She was very rich and was one of the heirs according to Rolf's will, but she demanded a greater share of the inheritance. She sued the foundation and the other heirs, and this started a long lawsuit in two courts. The mother won in the first court, and when the outcome in the second court seemed to go in the same direction, there was a settlement, essentially on her conditions.

A big part of the fortune was thereby lost, but the returns on the capital that came to the foundation were still enough to award four prizes of 400,000 crowns each, every second year. According to Rolf's will, one prize was to be given in logic and philosophy, one in mathematics, one in art, and one in music. Rolf wanted the first two prizes to be awarded by the Royal Swedish Academy of Sciences, the third by the Royal Academy of Fine Arts, and the fourth by the Royal Swedish Academy of Music. The question was how this was to be organized. Some were in favour of letting each academy arrange this as it pleased, but to me it seemed much nicer to let the prizes lend lustre to each other and to arrange a joint prize ceremony. This also became the decision of the board, and the first four prizes were awarded in 1993 at an elegant, yet fairly relaxed ceremony, at which the King of Sweden handed over

the prizes, followed by a buffet supper. Rolf had often seemed quite rebellious, but he had asked very established institutions to award the prizes, and I think that he would have liked the form of the prize ceremony that we decided on.

Of greater importance was of course which laureates were chosen. For the prize in logic and philosophy there was a committee of five persons, which I chaired—the other members were Lars Bergström, Dagfinn Føllesdal, Per Martin-Löf, and Georg Henrik von Wright. Our first problem was how to interpret the phrase “logic and philosophy”. To understand it as an intersection would make the field very narrow compared to the fields of the other three prizes. But it did not seem right, and it was probably not the intention of Rolf, to understand it simply as a union. Our solution was to say that the prize should be given for a contribution to logic that was of philosophical relevance or to philosophy that had some bearing on logic. There were obviously a great number of people who had made important contributions that fitted this description. We invited a group of people to make nominations and our choice was finally W.V. Quine. He arrived in Stockholm very alert, pleased to get the prize, and delivered a lecture about the notion of object, which attracted a big audience. Even Davidson and Dreben came to Stockholm to honour Quine on this occasion and we arranged seminars with all three of them on the day before the prizes were awarded. Two years later Michael Dummett was awarded the prize. I stayed in charge of the Foundation for 10 years and as chairman of the committee for the prize in logic and philosophy for four years. Then others took over.

I found it difficult to combine concentrated work in logic or philosophy with all these activities. The summers were more relaxed and were spent with the family. For many years we spend the summers in Italy. We then bought a small farm south of Stockholm. I liked to grow vegetables, and did a lot of that, supplying our own and some other family's entire needs for the whole year. A former student of mine, Gunnar Stålmärck, who had started a quite successful business based on a patent he had got for a hardware used to solve problems in sentential logic very efficiently, and who for a while employed the majority of the Swedish logicians, introduced me to sheep-breeding. It required a lot of fencing, because one had to change ground quite often to avoid intestinal worms, but otherwise the sheep took care of themselves. We started in a modest scale, slaughtered the lamb rams in the autumn, but kept the ewes. Soon they were quite a flock of sheep—numbering 52 in all, when we sold the farm since the children had grown up and were less interested in spending all their free time there.

2.9 Philosophy in Spare Time

Most of the papers in logic and philosophy that I did produce in these years were written for conferences in Italy or in connection with longer stays in Italy. For the Italian National Congress of Logic in 1979, which was held for a whole week in Montecatini, I worked out more systematically proofs of the normalization theorems for first and second order intuitionistic and classical logic using the notion of validity.

The paper was published in the proceedings of the congress, which was not very well circulated, and seems to have remained rather unknown. I learned the other day that several otherwise very competent and knowledgeable logicians thought that the problem of normalizing deductions in second order classical logic remained open.

In 1981, shortly after our first child Camilla was born, we spent six months at a vineyard in Chianti. At that time Marisa Dalla Chiara organized a conference on the foundations of mathematics. It was a good and quite lively conference. Among those who came from abroad were Kreisel, Putnam, Takeuti, Feferman, and Girard. I have a vivid memory of the conference among other things because we invited all its participants to a supper on our terrace of the vineyard; a risky undertaking for a great thunderstorm was looming up, which would have completely spoiled the whole supper—it came on shortly after the guests had left.

At the conference, I presented a paper where I compared and discussed some approaches to the concept of logical consequence. Besides the classical one by Tarski and the one that I tried to develop on the basis of my notion of valid argument from 1971, now revised in important respects, I discussed a notion based on ideas in a dissertation by Peter Schroeder-Heister. I had recently been asked to assess his dissertation, which I found interesting. It led later on to his staying for some time in Stockholm followed by further cooperation and to a long friendship. Among other things, he came to contribute to the further development of the notion of valid argument.

I was able to take leave from Stockholm University also for two other longer periods spent in Italy. In 1983, when our second child Erik was still a baby, we spent ten months in Rome at the invitation of Carlo Cellucci. I was attached to Rome University, La Sapienza, as “Professore a contratto”. There I gave a course in proof theory with lecture notes, which were carefully edited in Italian by Cesare Cozzo; my intention was that they could be the germ to a book on general proof theory.

I did not do much to implement that idea. When my dissertation was brought out, I had taken the risk on my own expense to print it in a greater number of copies than demanded by the university, but it had anyway been sold out fairly quickly. The question then arose whether to print a new edition. I preferred to write instead a new book that would bring the material of the dissertation up to date. But since this idea was not realized, the question of printing a new edition of the dissertation came back now and then, and when Dover Publications asked for the permission to make reprint as late as 2005, I finally decided to agree to that idea.

During my stay in Rome, I also took up once more a study of Dummett’s theory of meaning, and contributed a paper to the first volume that came out about his philosophy. While in Italy, I also gave some lectures in Siena, and at a meeting there I presented a further development of Hallnäs’s result on normalizing deductions in set theory, which was included in another virtually unknown proceedings.

In 1990–91, I had another sabbatical leave, and we then stayed in Florence for the whole academic year, sending our children to Italian schools. It was not the best time for leave since Dag Westerståhl and I were still preparing the details for the LMPS Congress that was to be held in August. Communications were not yet by e-mails but took place by fax. I recall how I was constantly going back and forth to my brother-in-law’s office that had a fax machine.

That year I managed to took up a problem I had lectured on in Stockholm in 1979 and in Oxford in 1980 concerning how to prove a normalization theorem for arithmetic by transfinite induction. It seemed likely that the ideas used by Gentzen in his second consistency proof could be used for this purpose. I had specific ideas about how this could be done by adding a rule of explicit substitution to natural deduction. Making some progress in this project, I presented the ideas at Ettore Casari's Saturday seminars, which I often visited that year; my presentation was in Italian—it was high time, it seemed to me, that I started speaking Italian on such occasions. But I was not able to bring the project to a successful conclusion.

Among other memorable conferences in Italy that I have participated in there were two held in the small Sicilian town of Mussomeli. They were memorable not only because of their philosophical content but also because of the friendliness, interest, and hospitality shown by officials and ordinary people. I do not know what they got out of the very specialized conferences that were held in their town. The first one, held in September 1991, was organized by Brian McGuinness and Gianluigi Oliveri and was devoted to "The Philosophy of Michael Dummett", which was also the title of the book that came out as a result. Dummett was honorary president of the second conference held in 1995, which had the title "Truth in Mathematics". In my paper to the latter conference I discussed Dummett's view of truth and presented my own, different view.

Both Dummett and I presented papers discussing the notion of truth at a conference held in Santiago de Compostela in January 1996, and the discussion between us continued at another conference "Logic and Meaning: Themes in the work of Dag Prawitz" held in June the same year in Stockholm. The latter was a conference arranged by Peter Pagin and Göran Sundholm as a celebration of my 60th birthday. It resulted in a special issue of *Theoria* with papers partly different from the ones presented at the conference but addressing themes that I had worked on. I was asked to write responses to the papers, which took me considerable time. Several of the papers seemed to me to require sophisticated replies, and I struggled with them for several summers to the frustration of Peter who was editing the volume; the issue came out as part 2–3 of *Theoria* for the year 1998, but was actually printed in 2000.

From the middle of the 90s, I led an interdisciplinary research project "Meaning and Interpretation". A group of people from philosophy, theory of literature, and linguistics had taken the initiative in this. It came to involve 22 researchers altogether from different disciplines and ran for six years, partly financed by grants from a research foundation. We arranged a large number of seminars and several conferences with invited guests from abroad. Several publications came out as a result. It gave me a little time for research, during which I wrote some minor papers, but it was not enough for more concentrated work on my part.

Thanks to this research project and other research grants, we could employ a greater number of philosophers than our ordinary university budget allowed. At the end of the 90s there were altogether seven lecturers in theoretical philosophy. The grants were sufficient to support a number of doctoral students too. In the 90s there was also another group of students interested in phenomenology, and I was able to arrange matters so that we received a grant from a private foundation that could be

used for the support of some of them. The group was led lead by Alexander Orłowski, and we also had sufficient resources to enable us to invite Dagfinn Føllesdal to participate in some of the activities. Among the doctors emanating from the group were Hans Ruin, who would form his own group at another university in the Stockholm area, and Daniel Birnbaum, who would come to have a career as a director of art museums in Germany and Sweden.

Among the dissertations that I supervised myself during the 90s, I especially appreciated the ones by Cesare Cozzo and Filip Widebäck. Widebäck's dissertation *Identity of Proofs* took up the conjecture that I had been discussing with Martin-Löf and Kreisel in 1969 and showed, independently of some similar results established by others at about the same time, that in the case of the implication fragment of sentential logic the proposed identity criterion is maximal in the sense that adding some further identities between proofs makes the relation collapse, that is, all proofs would become identical.

Cesare Cozzo's dissertation *Meaning and Argument* was a thoughtful contribution to what soon came to be known as inferentialism, a view that advocated a theory of meaning based on inference rules, taken in a wide sense. Ever since I met Cesare during my stay in Rome in 1983, I have greatly appreciated discussing various things with him, and it was a joy that he, after taking his doctorate at Florence, decided to write a second doctoral dissertation with me.

Superficially the kind of theory of meaning that I had tried to develop starting from Gentzen's idea of introduction rules as meaning constitutive could seem to be a kind of inferentialism. But when the matter is considered more closely an essential difference is obvious. The notion of validity of an argument that I had made the basis of my attempted theory of meaning is in general not a recursively enumerable predicate, whereas to be provable according to a fixed set of inference rules is such a predicate. This is an important difference, because in view of Gödel's incompleteness theorem we must be ready to extend our inference rules, for instance by bringing in concepts of higher order. We shall then be able to prove assertions in the original language that were not provable before the extension. If the meaning of asserted sentences is tied to a set of inference rules and their truth is tied to what can be proved by these rules, it is difficult to avoid the conclusion that the meaning and the significance of the assertions change when we extend the language, but this is counter intuitive because it clashes with our natural inclination to take the assertions to remain the same when the language is extended. In contrast, the notion of the validity of an argument and a concept of truth based on this notion are not defined with reference to a particular formal system and are consequently not affected by this kind of problem. (By the way, Cozzo escapes the problem by defining the meaning of a sentence by reference not to all inference rules that are in force, but only to some of them that concern the sentence in a qualified sense, and by explicating truth not in terms of what is provable in a language but in a more complicated way in terms of possible rational extensions of languages).

My meaning-theoretical use of the notion of valid argument, which I did not adequately separate from the proof-theoretical use until the beginning of the 80s, did not attract much attention to begin with. I was glad to be followed later in my

endeavour to explicate a notion of valid reasoning in this way by Michael Dummett in his book *Logical Basis of Metaphysics* from 1991 and by Peter Schroeder-Heister at the conference *Proof-Theoretic Semantics* that he arranged in 1999 and in the subsequent volume with the same title; there Peter made very clear how and why the notion of validity must differ depending on whether it is used as a basic semantic notion or for the end of proving normalizability. However, I always considered this project as a tentative one, and at a conference on natural deduction that Luiz Carlo Pereira arranged in Rio de Janeiro in 2001, I considered a notion of valid proof term that had a greater affinity with the usual notion of intuitionistic proof. More recently my interest has turned to the question what it is that gives a proof its epistemic force, and since this question cannot be answered in terms of valid arguments or valid proof terms, I am now more concerned with a notion of legitimate inference, which I consider more basic. But this belongs to another chapter of my life.

2.10 Retirement

At the turn of the century there were three more semesters before I turned 65, which was the stipulated age of retirement. There were several things I wanted to do before I was pensioned off. Among other things, I regretted that I had never lectured on the history of philosophy, and I planned to do so in the autumn of 2000. As usual there were too many things that had to be done. I was preparing the lectures, I was reading proofs of a second edition of a Swedish textbook in logic that I had written long ago and that was now published by Thales, I had promised the Research Council evaluations of some applications for research projects, and so on. Daniela said that I was leading an especially hectic existence at this time. Anyway, on the 11th of September, I bicycled from my home in the centre of Stockholm to the university campus, situated a few kilometres north of the city centre; usually a pleasant ride, the end of which went through woodland scenery. At an earlier point where the path crossed a two-lane street, I was hit by a car. It was a clearly marked bicycle crossing, in which the traffic should give way to bicycles, at least those coming from the right, and the cars in the first lane rightly stopped. The driver of a small lorry in the second lane, seeing a gap in front of him, speeded up, and hit me when I came to that lane. From a legal point of view the driver was to blame, but I should of course have been more careful.

My first reaction was “how silly to fall down like this, now I just have to continue”. But people around me came running, saying that I should just stay where I was, and in fact I could not move very much, having received a double fracture to my pelvis. I was taken by ambulance to the hospital. Before being operated on, I made two phone calls, one to Ulf Jacobsen, who was the main editor of Thales and with whom I had almost daily contact, telling him that I had a small problem but would call him again soon, and one to Daniela, to whose answering machine I said that I was in the hospital and asked her to inform the Research Council that I would be a few days late with my report. The operation went well anatomically but it gave rise to internal bleeding which was difficult to stop. I had been quite conscious up to the time of the

operation and had felt relatively well under the circumstances. But since the internal bleeding did not want to stop, the doctors continued to give me anaesthetics after the operation, and I remained anaesthetized, deeply unconscious, in the intensive care unit for two weeks. The bleeding soon threatened the kidneys and the lungs, and the situation was critical for a while. It was a strange feeling to wake up after two weeks. The situation improved gradually but slowly. After about half a year, my first sick leave as an employee, I was back at work, and in late spring, shortly before my retirement in June, my colleagues said that they recognized me as I used to be.

In the summer I went to Brazil for one month with Daniela and the two of our children who wanted to go there with us. The conference that Luiz Carlo Pereira arranged in Rio lasted a week. The rest of the month we spent at various places. We were especially fond of the coast in northern Brazil, east of Fortaleza, where a colleague lent us a hut for a week.

The next two years I had a part time appointment in the department, which allowed me to finish in a less abrupt way various things in which I had been engaged, such as the research project “Meaning and Interpretation”. I also continued as director of Thales to the autumn of 2004, at which point my real retirement started, but one could also say that I retired in September 2001 at the date of my accident.

At this time there was a reform at the Swedish universities allowing competent lecturers to be promoted as professors. Six lecturers in theoretical philosophy became professors in this way at about the time I retired. Three of them I have already mentioned: Peter Pagin, who had been a main participant in the project Meaning and Interpretation and with whom I share many interests, Per-Erik Malmnäs, who had also taken up an old interest in Greek philosophy which I would soon benefit from, and Gunnar Svensson, who had taken over as chairman of the department and in whose care it has continued to thrive. The other three had been recruited externally: Staffan Carlshamre, who also took over my work as publisher and has continued to develop Thales, Dugald Murdoch, who had written his dissertation at Oxford on philosophical aspects of the Copenhagen interpretation of quantum mechanics and with whom I shared an interest in the epistemic aspects of inferences, and Paul Needham, who also has a British background and is working mainly in the philosophy of science. It was considered to be enough with six professors of theoretical philosophy. They could be said to be my successors, since my position was not advertised as vacant.

At home there were also big changes. The children left home and went to live for themselves, eventually with partners, and now there are even grandchildren. I am glad to have survived my accident to see my children enter on their careers.

I see my retirement as a very happy change. I am now free to engage in whatever I like, and can for instance concentrate again more whole-heartedly on philosophical questions of my choice. It is like being back at the time when I was a student or docent, but now being less anxious to produce something. I still have a room at the department and go there for some seminars and colloquiums—for instance, in the Logic, Language, and Mind Seminars that Peter Pagin organizes—or when I feel like it, but I have no obligations.

From the autumn of 2004, I have declined administrative commissions with a few exceptions. I led a committee for assessing the Swedish philosophy departments,

commissioned by the National Agency for Higher Education, took part in a second round of such assessments, and participated in a committee for assessing Norwegian philosophy. I now enjoy enormously the freedom from all such duties.

Instead I can accept invitations to speak at conferences or at universities, which previously I often had to decline. In 2006, I was invited to give what is called the Kant lectures at Stanford. It was a pleasure to see again several old friends: Solomon Feferman, whom after my time in Stanford I had seen on numerous occasions in Stockholm and other places in Europe, Patrick Suppes, who was as vital as ever and had more energy to discuss my lectures than I had myself, and Gregori Mints, whom I had first met in Moscow in 1974.

That same year, I gave a talk at a memorial symposium in honour of Georg Henrik von Wright in Åbo on “Logical Determinism and The Principle of Bivalence”. An extensive series of seminars where Per Martin-Löf went through Aristotle’s logical work had inspired me to take up Aristotle’s Sea Battle. Per-Erik Malmnäs, also active at these seminars, inspired me to study the ancient commentaries to Aristotle’s work—a new kind of experience for me.

Still the same year, I was glad to speak on “Validity of Inferences” at a symposium in Bern, when Dagfinn Føllesdal was awarded the Lauener Prize, and on the same topic at another symposium arranged in Stockholm by Peter Pagin to celebrate my birthday.

For a couple of months in 2007, I was fellow at the Institute of Advanced Studies at Bologna on the invitation of Giovanna Corsi, a friend since my first Italian conference in 1972. I was even given the Medal for Science that the Institute awards each year, and gave also a course at the Philosophy Department of Bologna University.

Among other events in the last years, I gave the lecture at Michael Detlefsen’s inauguration ceremony as “Professeur d’Excellence” in Paris 2008. I was back in Paris for a longer visit in 2009 to lecture at several conferences and at Collège de France, this time at the invitation of Ann Fagot, a friend since we were both at Stanford in 1969–70. I was in Italy to give talks at two conferences in 2010, “Logic and Knowledge” in Rome and “Anti-realistic Notions of Truth” in Siena, and a course at a summer school for doctoral students in Siena organized by Gabriele Usberti in 2011. Later that year I replaced Saul Kripke as a plenary speaker at the LMPS Congress at Nancy, speaking about “Is there a general notion of proof?”. In the autumn the same year, I gave the Burman lectures at Umeå by the invitation of Sten Lindström, and gave a talk at a conference on “Evidence in Mathematics” arranged by Dagfinn Føllesdal. Last summer I was again in Rio de Janeiro to give a series of lectures.

Many of the talks on these occasions have had to do with the epistemic force of deductive inferences. Perhaps I have spoken too often of this problem, but it is one that intrigues me and that has been neglected in contemporary logic. Modern logic has on the whole ignored dynamic aspects. I am not fond of vogue words like “dynamic”, but I think it is appropriate here; an inference is first of all a matter of getting to know something that one did not know before. This epistemic phenomenon cannot be explained in terms of truth preservation under various interpretations in the way logical consequence is usually explicated. It is not easy to say how it is to

be explained. Aristotle had at least a name, perfect syllogism, for an inference that justifies its conclusion. For an inference to be legitimately used in a deductive proof it must provide us with a ground for the conclusion given that we had grounds for the premisses. Although the premisses of such a legitimate inference cannot assert a true proposition while the conclusion asserts a false one, it is obvious that truth preservation is not a sufficient condition for being legitimate. Nor can legitimacy be explained in terms of my notion of valid argument, as I have already remarked. It seems to me that to account for what makes an inference legitimate we have to rethink what an inference is. This is the topic of my paper in this volume, and I shall not speak more about it here.

Another question that has engaged me in the last one and a half years is one that I left in 1991, namely, how to prove a normalization theorem for first order arithmetic using transfinite induction. It had remained an open problem, and I took up it up again when asked if I could contribute something to a volume planned in connection with the 100th anniversary of Gentzen's birth. We are many who expected it to be possible to carry over Gentzen's second consistency proof to natural deduction and to extend it by using his methods so as to get a normalization theorem. Gentzen had obviously intended to prove this more general result, but had met with difficulties and had then confined himself to proving that cuts could be eliminated from an imagined proof of a contradiction in the sequent calculus. With the knowledge that we have now, more than half a century later, it should be possible to establish the more general result. But those of us who have tried to do so have found it surprisingly difficult. In my new, more relaxed situation after retirement, I was able to concentrate on the problem for some weeks, and then I saw where my previous attempt had gone wrong. After some additional weeks, when I was swinging between thinking that I knew how to do it and being in despair of finding a solution, I was able to modify the strategy in the right way so that all the pieces fell into place. At least, so it seemed, and it was very satisfying that this old problem now seemed to be solved. It remained to polish the proof and correct various oversights, but I now trust that the paper, having been read by several colleagues, is in order.

It is a curious thing to write an autobiography. In a way it is fun to recall old memories. I have written down some episodes as they have come to mind without any real plan, making selections with respect to whether they really mattered to me and were relevant in the "short scientific biography" that I have been asked to write for this volume. But when I check them, I find that my memory is not always as accurate as I thought and would like it to be. And when I look at the result, I sometimes wonder what person I am picturing. Needless to say, there are many other people than those mentioned here who have been important to me personally or professionally, and probably there are many other stories that I would have found more relevant to retell if I had thought about them.



<http://www.springer.com/978-3-319-11040-0>

Dag Prawitz on Proofs and Meaning

Wansing, H. (Ed.)

2015, XIII, 458 p. 6 illus., Hardcover

ISBN: 978-3-319-11040-0