Proof-Theoretic Semantics: An Autobiographical Survey

Peter Schroeder-Heister

Abstract In this autobiographical sketch, which is followed by a bibliography of my writings, I try to relate my intellectual development to problems, ideas and results in proof-theoretic semantics on which I have worked and to which I have contributed.

1 The term: Proof-theoretic semantics

Proof-theoretic semantics, from various perspectives, has been a predominant occupation for me since my doctorate. The field itself was already in existence prior to any contribution of mine. It was created by Gerhard Gentzen by designing a logical calculus representing the "natural" way of deductive reasoning, and by claiming that its special features — in particular its way of handling assumptions, and its classification of inference rules into rules for the introduction and the elimination of logical symbols — give certain rules a 'definitional' status, thus equipping logical symbols with their meaning (Gentzen, 1935). It was only consequential that the German logician Franz von Kutschera called this approach "Gentzen semantics" (von Kutschera, 1968) and thus created a term in analogy to "Tarski semantics" which is often used for the dominant approach to denotational semantics established by Alfred Tarski. Similarly, I came up with the term "proof-theoretic semantics" as a systematic term in analogy to "model-theoretic semantics", which emanated from what Tarski had put forward.[1] In 1985, I first mentioned it as the title of a planned book in a letter to Dag Prawitz. Later I used the term in lectures I gave in Stockholm. It appeared in

Peter Schroeder-Heister
Department of Computer Science, University of Tübingen, Germany, e-mail: psh@uni-tuebingen.de

[1] For further reflections on the idea of proof-theoretic semantics see my *Stanford Encyclopedia of Philosophy* entry (E2012c) and, from slightly different perspectives, Wansing (2000) and Francez (2015).

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print, I think, first in 1991 in an abstract (A1991e)[2]. Nowadays it seems a 'natural' term, which has become a standard designation for a certain field of study and is often used without much consideration of its conceptual origin; something it shares with Robert Brandom's term "inferentialism" (Brandom, 1994, 2000). It suggests that we are talking about a semantics that is based on, or crucially uses the notion of "proof" or "proof theory".

2 Academic roots: Hasenjaeger's institute

I discovered logic relatively early. At the University of Bonn, where I started studying in 1971, I came into contact with the *Institute for Logic and Foundational Research*[3] during my first year of study, even though it had nothing to do with my official subjects, which at that time were Catholic theology and mathematics. The head of this institute was Gisbert Hasenjaeger (M2006c, Wirth, 2021), who, in the 1940s and 1950s, had studied, worked and published with Heinrich Scholz in Münster. He had modelled his institute on Scholz's "Institute for Mathematical Logic and Foundational Research", which was the first institute of its kind in Germany. Hasenjaeger — who, incidentally, was a sort of counterpart to Turing[4] — had a very liberal attitude towards what logic should be, so he did not follow a particular strand of logic exclusively. This attitude also applied to those whom he admitted to his group's seminars. He did not mind that I, as an undergraduate, attended seminars which were officially intended for graduates only. As early as late 1972, for a seminar directed by Wolfram Schwabhäuser in my third semester of study, I quite enthusiastically produced a detailed exposition of Gödel's incompleteness theorems. In that essay of nearly 50 pages I even included Y. Matiyasevich's result from two years prior that every recursively enumerable predicate is diophantine, from which the (negative) solution of Hilbert's tenth problem directly follows. Alexander Prestel, who later, as a professor in Konstanz, was to become instrumental for my career, had just completed his Habilitation in Bonn and, following his inaugural lecture on this topic, left me his notes. I had become acquainted with the issues of incompleteness and undecidability through Wolfgang Stegmüller's excellent book of 1959, on which I must have hit during my first semester of study, or even earlier. I remember Hasenjaeger, during one of my presentations, correcting my pronunciation of the name "Kleene", which I pronounced like the word "clean", as I had not listened to any logic lectures before and knew the name only from printed

[2] References of the form A⟨year⟩, P⟨year⟩, C⟨year⟩, D⟨year⟩, R⟨year⟩, E⟨year⟩, M⟨year⟩ refer to the corresponding sections of the bibliography at the end of this article.

[3] "Seminar für Logik und Grundlagenforschung"

[4] After being severely wounded in the war, he was, as a gifted mathematician (who had not yet started university studies), appointed to the cryptology department of the German military and tasked with testing the cryptological reliability of the Enigma machine. He was not able to discover those weaknesses that allowed Alan Turing and Gordon Welchman (based on ideas of Marian Rejewski and other Polish cryptologists) to decrypt the code it generated — "fortunately", as he said after the war, as otherwise the war might have lasted even longer.

sources. In 1973, still in my second year at Bonn, I wrote another essay on Gödel's functional interpretation of arithmetic (the *Dialectica* interpretation), which meant that by this point I had built up a significant background in logic. However, I regret that I did not take the chance of learning set theory or model theory in more depth. I would have had a perfect chance to do so, as besides Prestel, Keith Devlin, Ronald Björn Jensen, Sabine Koppelberg and Wolfram Schwabhäuser were all at the institute at the time. In any case, there was a spirit at the department that motivated young researchers like myself.

There was also funding available for students to attend meetings in mathematical logic; much of it from the Volkswagen Foundation. Already in early April of 1973, in my second undergraduate year, I had the chance to visit such a meeting in Tübingen, organized by my future colleague Walter Felscher, who had just become a professor of mathematical logic there. Solomon Feferman gave a two-day course on advanced proof theory, and I was able to meet quite a prominent (or soon-to-be prominent) section of the German logic scene including Wilfried Buchholz, Justus Diller, Ulrich Felgner, Wolfgang Maas, Gert Müller, Helmut Pfeiffer, Wolfram Pohlers, Kurt Schütte and Helmut Schwichtenberg. Very impressive was a week in January 1974 at the Mathematical Research Institute in Oberwolfach, where I had been invited to a meeting on set theory and model theory led by Felgner (another future Tübingen colleague), where many outstanding German logicians were present. Of the other conferences or schools that I was able to attend as an undergraduate, I remember one in Münster on intuitionistic logic with courses given by Anne Troelstra about choice sequences and Dirk van Dalen about intuitionistic logic in general.

With other students at the Hasenjaeger institute, notably Benedikt Peppinghaus and Hans Leiß, we embarked on all kinds of interesting ventures. Benedikt, for example, invited Eduard Wette, who was considered by many an enfant terrible of mathematical logic[5], to present his purported system-internal consistency proof of arithmetic which, by Gödel's second incompleteness theorem, would prove the inconsistency of arithmetic. I found this extremely stimulating, even though there were gaps in the proof he presented (as one might have expected). Again, it speaks for Hasenjaeger's liberal attitude that he permitted such things to take place. It is obvious that all this could pull somebody like me in the direction of proof theory and constructive logic. However, logic was only a part of my student life in Bonn between 1971 and 1977 (see Section 4), and I did not acquire any university degree in logic until my doctorate.

3 Family roots: Upbringing and school

I do not have an academic family background. My mother came from the household of a primary school teacher and was very well-educated despite the fact that due to the difficult circumstances of her time (including gender-based disadvantages) and family

[5] For a fair characterization of Wette see Paul Bernays's letter to Kurt Gödel of 24 January 1975 (Gödel, 2003).

obligations she was unable to attend high school. My father, who, like my mother, was well-educated and had a talent for mathematics, ran a business that produced stationery (exercise books, writing pads etc.), which he had taken over from his father. This prevented him from going to university after returning from America, where he had been a prisoner of war. Despite it not being his first-choice career, he was a very successful businessman. When he sold the company in the 1980s, he was able to establish, from the profits, a charitable foundation that built and ran an innovative local carehome. In our town of Düren[6], where I was born on 2 March 1953 as the eldest of four children, I attended the "humanist" high school with its emphasis on Greek and Latin, at the expense of modern languages and natural sciences, though provided with an excellent education in mathematics.

At school, I was always good at mathematics, and it was always assumed I would study mathematics at university (which I did). I otherwise became interested in philosophical and theological questions when I was fifteen or so, partly through the influence of a Catholic youth organization and its local leaders[7], who were pretty left-wing (it was 1968, after all). Through a friend — Lothar Stresius, who was four years older, a theology student at Bonn and Tübingen[8], and in many respects a role model — I had the chance, while still a high school student, to attend lectures in Tübingen, I think in 1970, by many famous German theologians of the time. These included Eberhard Jüngel (20 years later my colleague in the Department of Philosophy), Ernst Käsemann, Walter Kasper (today cardinal of the Roman Curia), Hans Küng and Jürgen Moltmann (Joseph Ratzinger — later pope Benedict XVI — had left Tübingen the year before). All this impressed me so much that, after my high school diploma in 1971, I enrolled in Bonn in Catholic theology, in addition to mathematics. After one year I replaced theology with philosophy, which I had been studying as part of the theology course, and which also interested me deeply.

4 Bonn 1971–1977: Undergraduate study

Philosophy and mathematics. My interest in philosophy dates back to when I was 16 or 17 and read, in addition to the fashionable philosophical literature of the time (Frankfurt school, especially Adorno and Horkheimer), some Popper (my earliest copy of the *Logik der Forschung* dates from 1970) and also some Kant. I was introduced to the latter by my excellent high school philosophy teacher Hubert Fackeldey, who later became a professor in Cologne and made significant contributions to philosophical aspects of deontic logic.

Philosophy in Bonn was a traditional department covering the whole history of the discipline from antiquity to modern times, where modern times meant essentially

[6] To mention its mathematical connection: Peter Gustav Lejeune Dirichlet (1805–1859), who propagated the notion of a mathematical function as an abstract (that is, operation-independent) mapping, was born there.

[7] Especially Paul Georg Meyer, who later became a linguistics professor at RWTH Aachen.

[8] Later, after a Ph.D. on Adorno, he became high school principal.

hermeneutics and Heidegger, but also some Wittgenstein and analytical philosophy. Even the normally underrepresented (if present at all) medieval philosophy was very strong. Most impressive in philosophy to me was the approach of the Kantian Gerold Prauss, who was an associate professor who later went to Cologne and then to Freiburg. He was working on a novel interpretation of Kant's concept of "thing-in-itself" and had just completed a book manuscript on it (Prauss, 1974). Although he did not know me he had so much trust in me that he lent me a carbon copy of it (not a photocopy — this technique was just about to enter wider use), and at some stage, because he unexpectedly needed it for the publisher, had to find out where I was living to have it returned.

Prauss later also supervised the philosophy part of my university degree, which I completed in 1977, writing a thesis on the concept of truth in natural languages (originally I had even considered writing something about Heidegger). Unlike its title might suggest from the modern point of view, it was a piece in philosophy of language in the Kantian spirit, which had nothing to do with formal natural language semantics. My final university degree was actually a combined teachers' degree in philosophy and mathematics. Like most humanities students at the time, I chose the teachers' degree simply because this gave one the possibility to enter the high school teaching profession if anything went wrong with the Ph.D.

Through Prauss I also got in contact with Günter Buhl, who had written an excellent dissertation on consequence and grounding in Bolzano (Buhl, 1961), the outstanding quality of which I only appreciated much later when working in proof-theoretic semantics, as he was the first to fully recognize the proof-theoretic aspects in Bolzano's work. Buhl taught elementary logic in the philosophy department using a system of natural deduction. He hired me as a student assistant, and I was able to give tutorials in formal logic, an experience that shaped all my later teaching of logic to philosophers.

I spent my student time until graduation essentially in Bonn, but also as a "guest student" in Cologne and Aachen. The initial choice of Aachen was chiefly due to the fact that my girlfriend Gabi was a student there. In Aachen, I attended seminars run by Christian Thiel, who later became the successor to Paul Lorenzen in Erlangen. I learned a lot from him. He drew my attention to Lorenzen's *Operative Logic* of 1955, which from today's perspective must be considered one of the milestones of proof-theoretic semantics[9], even though Lorenzen himself abandoned his operative approach in favour of dialogical semantics in the 1970s, something also appreciated by Thiel. Dialogical logic was a topic I became strongly interested in myself — much later we even had a research project on it (see Section 11). Outstanding was, of course, Thiel's knowledge and mastership as far as Gottlob Frege's work was concerned. In the German-speaking world he had initiated, through his doctoral dissertation on sense and denotation in Frege's logic (Thiel, 1965), the revival of interest in Frege's work and played the role which in the English-speaking world was taken by Michael Dummett and his monograph on Frege's philosophy of language (Dummett, 1973).

[9] See my later papers A2007b, A2007c, A2008a.

Music and musicology. I had a modest background in musical performance (piano) and therefore a corresponding interest in music and its theory. For three semesters I studied musicology in Bonn as an additional subject, which was possible at that time. Of the many courses I took, most stimulating were theories of harmony, which was only natural given my mathematical inclinations. A lasting impression on me was made by Martin Vogel, who had developed a systematic approach to the topic, which very much differed from the historical attitude typical for German musicology. Together with a small group of students, some of whom came from mathematics, he developed a theory of harmony based on pure tuning, mathematically modelled as (an extension of) Leonhard Euler's web of tones and based on approaches by the physicists Hermann von Helmholtz and Arthur von Oettingen (Vogel, 1975). Had I continued musicology, I probably would have followed this path of study myself, in particular as my later occupation with computer science would have presented the right environment and interesting practical applications for it.

Private life. The summer 1974 changed my life, when I met Gabi (Gabriele Heister), who was to become my wife five years later. Gabi was studying in Aachen for two degrees at the same time, one in psychology and one joint degree in German and education. I met her on a holiday trip to the Netherlands in a larger group of students. Apart from sharing my interest in music (she had at one point trained as a soprano), she also influenced my general scientific views. Whereas I was very much a humanities scholar (including logic and mathematics), she had a thorough grasp of empirical research. Moreover, through experimental psychology she was concerned with statistical analysis of data, which was completely new to me, even though I knew some probability theory and mathematical statistics. Discussing these topics with Gabi broadened my intellectual views significantly. Drawing up hypotheses, looking at the outcome of experiments, and using statistical methods in the evaluation of data — the absolutely normal approach in the sciences — was something I was not used to.

5 Konstanz 1978–1981: Encyclopedia and doctorate

What next? When I had completed my teacher's degree in May 1977, the question was: what to do now. I wanted to do a doctorate but I was not keen to continue with the topic of my undergraduate thesis, and apart from that there was no job in sight to finance it. As it turned out, Gabi, who had completed her diploma in psychology around the same time, had been offered several positions, including one at the University of Konstanz for the year. With no other options for me in sight, we decided to go. In November 1977 we found a flat in Allensbach near Konstanz, and in December we moved. For Gabi the working conditions were less than ideal, to put it mildly. For me, the move turned out to be one of the best decisions of my life. There was no job initially, but that was soon to change.

Gereon Wolters, the Dingler Nachlass, and the Encyclopedia. The opportunity to start an academic career in Konstanz I owe first to Gereon Wolters and then to

Jürgen Mittelstraß. In early 1978, I walked into Wolters's office. He had completed his Ph.D. the year before and was affiliated with Mittelstraß, who held one of Konstanz's three chairs in philosophy. It turned out that Wolters had just successfully applied for a grant to make the Nachlass (literary estate) of the philosopher Hugo Dingler accessible. There was a part-time position available for 10 months on this project with no candidate yet nominated. Wolters hired me on the spot. He even gave me an office of my own, the window of which offered a magnificent view over Lake Constance and the Isle of Mainau. As the university was still under development, it could be relatively generous with respect to office space. My first publication resulted from this job: a bibliography of the works of Hugo Dingler (A1981a). It was a piece of meticulous work, of which I am still proud.

Through Wolters, I joined the team assisting Jürgen Mittelstraß in his editorial work on the *Encyclopedia of Philosophy and Philosophy of Science* (1980–2018), a voluminous work that he had started some years ago, with the first volume covering the letters A–G due to be completed in spring 1979. A logician with interests in adjacent fields was a very welcome addition to the team. When in 1979 there was a two-year position available, I received a part-time appointment to work on the Encyclopedia, being free to work on my dissertation during the rest of my time. Over the years (the eighth and final volume of the second edition appeared in 2018, the year before my retirement) I wrote around 200 articles for it (E1980–2018); tiny ones such as *a* (the letter mnemotechnically representing universal affirmative judgements in traditional syllogistics), but also longer ones such as *Popper, Karl Raimund* (E1995a). We also had a lot of fun creating a number of fictitious entries ('Nihilartikel', 'mountweazels') — a long tradition in encyclopedic works, which reached a new level of sophistication under Mittelstraß.

Linguistics and mathematics. In Bonn I had come into contact with logic almost exclusively from the mathematical perspective. As to philosophical logic, I was only acquainted with topics relating to the foundations of mathematics, such as the dispute about classical versus intuitionistic logic or the discussion of the logical and set-theoretical paradoxes. I was not aware of extra-mathematical applications of logic.

Making interdisciplinary contacts was very easy in Konstanz. All disciplines were under the same roof in one huge building complex, where one would frequently run into people from other departments in the corridor, in the cafeteria and in the large entrance lounge of the compound. People in general linguistics were natural partners to whom one would speak. In Konstanz, general linguistics had a strong leaning towards logic-based semantics. It was through this that I engaged more seriously with Montague grammar[10], higher-level linguistic type theory and similar issues. I discovered that general linguists were all doing logic in a very original fashion and at an advanced level that went beyond the sort of mathematical logic I was used to. This was essentially due to the intensional phenomena one has to deal with in linguistics and that do not play a role in mathematics, which is essentially (though not throughout) extensional. My main contacts on the professorial side were Urs Egli and

[10] Had I followed Hans Leiß's initiative, I would have studied this subject already together with him in Bonn.

Arnim von Stechow (who later became my colleague in Tübingen), and at the level of the postdocs Thomas Ede Zimmermann (who was later in Tübingen and Stuttgart, and then became professor in Frankfurt) and Wolfgang Sternefeld (later a colleague in Tübingen). I learned a great deal that I would probably never have encountered at a less communicative university with a more conventional department structure.

Konstanz had a strong mathematics department, and also a strong group in mathematical logic, headed by Alexander Prestel, whom I knew from Bonn. Prestel was a set theorist and model theorist and held a weekly seminar which I regularly attended. A special role was played by Ulf Friedrichsdorf, a permanent member of Prestel's group. He was a hardcore mathematical logician, but with an incredibly wide range of interests — he co-authored textbooks on model theory and set theory with Prestel — which extended in particular into philosophical logic and into linguistics, exemplified, for example, by his brilliant textbook on classical and intensional logics (Friedrichsdorf, 1992), which even covered the arithmetical completeness of derivability logic proved by Solovay some time ago. I have frequently used it for teaching. In Konstanz I realized for the first time what a collaboration between philosophers, mathematicians and linguists could achieve.[11]

Teaching. From 1981 to 1989 I held full-time positions in Konstanz, made possible by Jürgen Mittelstraß. This meant that I was obliged to teach, and in Konstanz I was often able to choose which subjects to teach, which elsewhere would not have been possible at this early stage of my career. I taught logic courses, but also other topics including Frege, Husserl, Carnap, Popper, ontology, epistemology, general philosophy of science, and later also logic programming and automated theorem proving etc. I learned a lot from teaching, and I had quite a number of excellent students. As mentioned above, the architecture of the university encouraged interdisciplinary activities, and there were always students from mathematics, linguistics and other subjects in my courses.

Marriage and children. Gabi and I got married in December 1979. This is how I arrived at my double-barrelled surname: "Schroeder-Heister", adding Gabi's surname to my birth name, "Schroeder". As the ability of husbands to take their wives' names had only just been introduced in Germany, the administration was not used to it. The registrar visited us at home after the ceremony to fill in critical signatures they had forgotten at the ceremony. Our children were born a couple of years later. Paula in 1984, and Justin in 1987; both in Münsterlingen, on the Swiss side of the lake. This did not give them Swiss citizenship, however: it is not that easy to become Swiss.

A Ph.D. without a supervisor. I took the risk of doing my Ph.D. without a formal supervisor. There was no "natural" supervisor in Konstanz for my field of interest. In fact, for a long time I did not know what to do contentwise. What would become the final topic of my thesis only emerged about twelve to eighteen months before I submitted it. Until then, it was not even clear whether it would be mathematical or philosophical logic, and how strong its philosophical component would be.

I was working in all sorts of directions, many of which I no longer remember.

[11] For the history of logic at Konstanz University see Buldt (2022).

What I do remember is that I somehow hit onto von Kutschera's 1968 article, in which he coined the term "Gentzen semantics" (see Section 1). There he showed, for intuitionistic propositional logic, the expressive completeness of the four standard connectives conjunction, disjunction, implication and absurdity, corresponding to the classical idea of functional completeness. This was established relative to a general schema of rules characterizing arbitrary n-ary connectives. Crucial was the idea of introducing some iteration of a sort of structural implication leading to what he called "S-formulas", which played a critical role in the interpretation of implication as a connective. I also at the time hit upon Prawitz's 1979 article, which had the same aim of proving expressive completeness. Whereas von Kutschera worked in a sequent-style framework, Prawitz used the apparatus of natural deduction that he had put forward in his monograph (Prawitz, 1965). It turned out that Prawitz's paper contained an error, as he could not give a proper schema for implication without presupposing it. My idea was to combine von Kutschera's idea of S-formulas with Prawitz's natural deduction schema. To achieve this, I developed a system of rules of higher levels, that is, of rules depending on rules, and generalized elimination rules using this tool. This provided a general framework to deal with arbitrary connectives. The introduction and elimination rules for logical connectives were understood as telling that connectives express a system of inference rules, which gave the connectives a semantics in terms of rules, thus a sort of proof-theoretic semantics (although I did not yet use this term in my thesis). In the second part of the thesis, following some ideas of von Kutschera (1969) I extended this approach to a system with an operation of denial and corresponding refutation rules, something which nowadays would be called bilateralist, but which I never published afterwards. I developed this very quickly, and my thesis was essentially written in the first half of 1980.

How to proceed formally towards a Ph.D.? The thesis was very much done in the spirit of Dag Prawitz, whose works I admired, in particular his combination of formal investigations with philosophical interpretation. However, I did not know him personally, and had not seen him at any conferences. In fact, Prawitz to me was somebody of such high status that I did not dare approach him. Help came from Alexander Prestel. He encouraged me to contact Prawitz after all, and actually sent him a photocopy of my thesis draft recommending me to him. With Prestel I also discussed the idea of finally submitting the thesis in Bonn with Hasenjaeger as examiner, even though he had not been involved with my thesis at all. From his time in Bonn, Prestel knew Hasenjaeger very well and was aware that I had left a good impression on him years ago. The arrangement with Hasenjaeger finally worked out. After seeing my draft, and in light of my earlier achievements as an undergraduate student, he accepted me. The final plan was, in addition to Hasenjaeger as the first examiner, to get Prawitz appointed as external examiner, which the philosophical faculty agreed to.

The only problem was that I had no reply from Prawitz, who at the time was on a research stay in Italy. Per Martin-Löf, who knew from Prawitz about my thesis draft supported me and encouraged him to respond to my request. I met Martin-Löf for the first time in Munich in early 1981 at a meeting organized by Helmut Schwichtenberg, where he presented his constructive type theory, and where Peter Aczel presented

his version of intuitionistic set theory. This meeting impressed me enormously, in particular the way in which Martin-Löf presented his system. It was very philosophical; he argued for the rules he was giving from first principles.

Suddenly, in mid-January 1981, after having heard nothing from Prawitz for months, there came a 25-page handwritten letter, in which he discussed my thesis draft in detail. This was extremely helpful, in particular as he pointed out to me certain problems associated with introducing rules as structural entities, as this essentially meant the duplication of implication. However, in principle he was quite happy with the approach. This allowed me to revise and finish the thesis very quickly and to formally submit it, so that in May 1981 my oral doctoral exam ("Rigorosum") took place in the form of three examinations on one day, in logic as the main subject, and philosophy and musicology as minor subjects. Due to the existence of Hasenjaeger's institute (Section 2) the formal degree subject of the doctorate was "Logic and Foundational Research" (rather than "Philosophy"). I only met Prawitz personally the year after. Thus, he had been the hidden supervisor of my thesis.

Part of my thesis (A1981b), which itself was written in German, appeared in English in revised form under the title "A natural extension of natural deduction" (A1984a) in the *Journal of Symbolic Logic*[12]. The title goes back to comments made by Göran Sundholm, who acted as a reviewer for the *JSL*[13]. It was to become my most-cited paper. Perhaps this confirms the view that for normal researchers, the work for their doctorate is often the most substantial work they ever do, with all the rest being incremental extensions of it. Somebody like Gödel had several different and independent fundamental ideas in his lifetime, but most people are no Gödel.

6 Konstanz 1981–1989: Postdoctoral research and Habilitation

Being free after achieving the Ph.D., I continued my research in various ways on topics that occupy me still today.

Semantical completeness. My dissertation gave a proof-theoretic semantics of propositional logics and proved the *expressive completeness* of connectives with respect to this semantics. What I tried in the sequel was to prove Prawitz's completeness conjecture of 1971 (Prawitz, 1971, p. 257), namely that the formalism of intuitionistic logic is *semantically complete* with respect to a certain sort of proof-theoretic semantics. I published two papers on this issue (A1983c, A1985b) with practically no feedback. Only some 35 years later I came back to these issues in joint work with Thomas Piecha, where we showed the limitations of the goal to prove completeness — see below (Section 10). Today, on the background of the modern discussion (see Piecha, 2016), this topic has regained some interest through work by Tor Sandqvist (2015), David Pym and others (2022).

[12] A transfer to quantifier logic appeared in A1984c.

[13] The original paper submitted to the *JSL*, which differs substantially from the final printed version, is still worth reading. It is attached to the online version of my thesis A1981b.

Popper. One of the outstanding experiences of my intellectual life was meeting with Karl Popper. I had already once been in contact with him when I asked him for possible information about Kurt Grelling, on whom in 1980 I had to write an article for the *Encyclopedia*. I was extremely impressed that he replied immediately to my request with a letter in his beautiful handwriting. Shortly after my doctorate, I came, more or less accidentally, across his papers on deductive logic that he had published in the late 1940s after finishing his *Open Society* (Popper, 1945). These papers were very different from the topics that one would expect from Popper. They had received a mixed reception in the logic community. Even though Popper was one of the leading philosophers of the twentieth century, his contribution to deductive logic remained largely unknown.

I immediately saw the significance of these papers for proof-theoretic semantics. They attempted to provide an inferentialist foundation for logic. What I discovered was that Popper's papers provided an original contribution towards the problem of the logicality of sentence operations. I tried to work out this idea in a long paper, which appeared in *History and Philosophy of Logic* (A1984d). A later paper in the *Popper Centenary* volume (A2006b) attempted to change the ignorance regarding Popper's logical theory, as did papers by my students and colleagues David Binder and Thomas Piecha a decade later (2017, 2021). Recently, the three of us completed a volume containing Popper's logical papers as well as manuscripts from his Nachlass (estate) on these topics, including exchanges of letters related to logic, as well as a detailed introduction (C2022b, A2022a)[14]. We look forward to seeing the reaction from the logic community to Popper's ideas.

In July 1982, I sent the first version of my manuscript (of A1984d) to Popper. I immediately received a reply, followed by another one with more detailed comments a few days later. This extended into an exchange of letters (M2022). Popper started this exchange with me, somebody who was a complete no-name to him, simply because he was interested in the topic and in what I had done on it. Some time later Gabi and I met Popper in person. We were staying in Edinburgh, where I had a fellowship at the Institute for Advanced Studies in the Humanities for three months, and Popper was the keynote speaker at a meeting in Leicester in spring 1983 that we attended. He greeted us there, and I had the chance to speak with him about the paper. Soon after we had another exchange of letters on inductive probability and his joint paper with David Miller (Popper and Miller, 1983)[15]. Years later, after a lecture in St. Gallen in June 1989, with his characteristic sense of humour, he greeted me with "Ach, Herr Schroeder, ich hätte Sie fast nicht erkannt, Sie sind aber dick geworden" ("Ah, Mr Schroeder, you have put on weight — I almost didn't recognize you").

My interest in the foundations of probability theory and the notion of randomness that I had maintained since my mathematical studies in Bonn was also, in part, due to Popper. The chapter on probability in the *Logik der Forschung* presented a frequentist definition of probability which was highly original and, being a definition of randomness for finite sequences, anticipated certain aspects of A. N. Kolmogorov's

[14] For a summary of (our view of) Popper's ideas on deduction see Piecha (2023).

[15] In Popper and Miller (1987), they explicitly acknowledged comments of mine on the possible dualization of their argument, which was also quite rewarding at this stage of my career.

later definition in terms of algorithmic complexity. I studied some of these theories, in particular by R. von Mises, A. Wald, Martin-Löf, C. P. Schnorr and Kolmogorov, but have never been able to turn this into a research topic — too many excellent researchers were already involved in it, and I was fully occupied with other issues. However, a presentation of Popper's approach in a collection of essays on the *Logik der Forschung* resulted from it (A1998b).

Psychology. Gabi completed her Ph.D. in experimental neuropsychology in 1985. Through following Gabi's work, I found this type of research more and more interesting and gradually started looking into it myself. Her experiments were based on reaction times upon tachistoscopically presented stimuli. She realized that work in general psychology in the area of perception played a significant role in the effects observed, and her work shifted somewhat towards that realm. She became interested in spatial stimulus-response compatibility: the phenomenon that in most situations spatial features of a stimulus correspond to spacial features of the response in terms of shorter reaction times. This finding allows certain insights into the way spatial information is processed and how cognitive processing is organized in the brain. My contribution went slightly (but only slightly) beyond statistical evaluation and towards the discussion and the theoretical modelling of results, which is reflected in the fact that I made first author in one of around ten papers with which I was involved (P1988).[16] This work absorbed me for quite some time, as it was so interesting, even though it distracted me from proof-theoretic semantics. It fitted my interest in cognitive science, nevertheless.

Philosophy of science. The 1970s and 1980s were the heydays of philosophy of science, in which general topics such as the status of theoretical terms and grand themes such as normal versus revolutionary science were discussed. A particularly important topic was the thesis of the incommensurability of theoretical terms in the case of revolutionary science, put forward by T. S. Kuhn. In Germany J. D. Sneed's model-theoretic unterstanding of scientific reasoning was prominent, especially through Stegmüller's (1979) propagation of it. Every logically inclined philosopher would have studied these approaches at the time. In my case this led to a paper in *Philosophy of Science* (A1989) which argued, by using model-theoretic means, that reducibility and incommensurability of theories are not necessarily incompatible. It was to become my only model-theoretic publication and used the expertise of my co-author Frank Schaefer, who was studying model theory with Prestel.

Frege. Something that has always intrigued me was the philosophy and logic of Gottlob Frege. I had come across it at a very early stage of my studies — my copy of the *Grundgesetze der Arithmetik* dates from 1971, my first semester of study. However, practically all interesting questions I have dealt with in various papers were triggered by remarks or exchanges with Christian Thiel, partly by remarks in discussions with him, partly by his many publications on Frege and Fregean themes.

The first paper of this kind was on Section 10 of the *Grundgesetze* (A1987a),

[16] See subsection P of the bibliography. Sadly, our co-author of many papers, Walter Ehrenstein, died in 2009, aged only 58 (Paramei, 2009).

which I also presentend at a Frege conference in Schwerin in 1984 (A1984b). At this conference I had the chance to meet almost the whole logic community of the German Democratic Republic (GDR), but also many people from elsewhere. Doing logic in the GDR was a way to avoid the ideological pressure exerted on other branches of philosophy. Consequently, in communist East Germany, logic was better represented within philosophy than in West Germany. In the paper I tried to reconstruct Frege's arguments of whether and which courses-of-values could be chosen to function as truth values, which is fundamental for the ontology of the *Grundgesetze*. At that time I was quite satisfied with the paper, and later even more when it occurred amongst the very few quotations Dummett made in his book on Frege's philosophy of mathematics (Dummett, 1991). Initiated by Kai Wehmeier, the topic of this paper later received a new rethinking in a joint paper (A2005), which we then dedicated to Thiel.

When looking at Frege's notation and terminology in the *Grundgesetze* I realized that in a certain way this resembles very much the structure of Gentzen sequents and the terminology applied there. I wrote a paper on this but realized just when it was finished that von Kutschera, to whom I had sent it, had independently had very similar ideas[17]. However, for another paper on Frege I claim full originality (A1997). In it I interpreted his propositional calculus from the *Grundgesetze*, which he was applying to solve a problem discussed by G. Boole, E. Schröder, W. Wundt and H. Lotze, as a system of propositional resolution. I found it very intriguing that systems with the cut rule (plus substitution) as the only inference rule, which have become prominent in automated theorem proving, could be traced back to Frege, at least for the propositional fragment. Needless to say Frege has been a frequent topic of my teaching.

Historical foundations of logic. Frege is the greatest figure as far as the foundations of modern mathematical and philosophical logic are concerned. Two other great figures in the (occidental) history of logic before were Aristotle and Leibniz. I was interested in both of them and have taught and written on both. On Leibniz I wrote a paper with Mittelstraß concerning his arithmetical calculus (A1986), towards which I had been directed by Thiel, and which can be seen as a model-theoretic semantics in terms of pairs of coprime natural numbers. This is closely related to Leibniz's programme of a *Characteristica Universalis* that would code concepts by arithmetical means and allow one to decide the validity of inferences by calculation, sometimes seen as the precursor of modern AI. The paper also contains remarks on the concept of probability in Leibniz as the degree of possibility. On Aristotle I have a paper (A2008), with Mittelstraß also, which discusses, in a certain context, his modal syllogistics and also the significance of the fourth figure of traditional syllogistics (not yet present in Aristotle). I emphasized in particular a result by Daniel Merrill (whom I knew already from my work on Popper's logic), which showed that in the presence of obversion, every syllogism (even an invalid one) can be reduced to the fourth figure, which is not possible for the other figures (to which, as Leibniz was already aware, every valid syllogism can be reduced). I also studied the work of Paul Hertz, a predecessor of

[17] Von Kutschera (1996). Though presenting them on various occasions (e.g. A1999), I published my results only much later (A2014a), of course with proper acknowledgement to von Kutschera.

Gentzen, who developed a system of purely structural reasoning, whose inference steps can be viewed as applications of the resolution rule, and whose proofs can be put into certain normal forms (A2002b). This is highly significant for proof-theoretic semantics, as this system is the starting point of Gentzen's research and the topic of his first publication (1933). His iteration of structural implications could remind one of my own higher-level rules. If I remember correctly, again it was Thiel who had drawn my attention to Hertz. Michael Arndt (D2008) continued and further advanced this work on Hertz.

Logic programming and computer science. Around the time of my doctorate computing took off for the general public. My doctoral thesis was still written using a typewriter with the formulas inserted by hand and larger corrections made with scissors and sticky tape, thus by cut and paste in the literal sense. I had already come across advanced computing through the linguists, who owned a Lisp machine. I do not remember which brand it was. In any case, it was a revelation to see the graphics display operated with a mouse, and the large 8-inch floppies to store programs and data. Through them I also acquired the reference to PROLOG and logic programming, and, following that, I hit on J. W. Lloyd's 1984 book on its foundations, which had just come out.

After reading Lloyd's book, I realized that logic programming, when understood proof-theoretically, had very much to do with what I myself was doing. In particular, my own framework with rules as assumptions offered not only a neat interpretation of logic programming, but even allowed for an extension of logic programming using embedded implications. These things were in the air, with people such as Dov Gabbay and Uwe Reyle (1984) as well as Dale Miller (1986) doing similar things.

Beginning with Schwichtenberg's Munich conference of 1981 (see Section 5) and through my contacts to Stockholm, I also became involved with the type-theory oriented programming community, which did not have much overlap with the logic programming community, apart from people like myself, Lars Hallnäs and Dale Miller. This community was much nearer to functional programming which has often tended to distance itself from other paradigms. People in Edinburgh such as Gordon Plotkin and the *Logical Framework* group realized that my higher-level rules and the generalized elimination rules for logical operators seemed to be of considerable computational use for the implementation of logic systems, to the effect that I was invited to conferences there with many prominent computer scientists and computational logicians present. My contribution to Gérard Huet's and Plotkin's volume on logical frameworks (A1991d), in which, by reference to substructural logics, I gave the general elimination inferences the status of a kind of structural rule, resulted from this. I also made contact with colleagues in Cambridge; I remember talks in Larry Paulson's and in Martin Hyland's seminar[18]. My strong interest in Martin-Löf type theory and the fact that I could apply my methodology to it (A1989a), kept me in lasting contact with the Programming Methodology Group in Gothenburg, quite independent of the very many and regular encounters and personal discussions with Per Martin-Löf himself over the decades.

[18] And in Paulson's office a young French postdoc, Thierry Coquand.

All this coincided with me developing an interest in cognitive science and artificial intelligence, not only from the point of view of rule-based AI (today the "old" AI), but also from the viewpoint of neural networks, which were enthusiastically discussed at the time. From a conference in Berne, which I attended together with Gabi in the early 1980s or perhaps even before, I still remember a presentation by Allen Newell, where he demonstrated one of the big expert systems of the time in which he was involved, I think it was MYCIN. If I remember correctly, Herbert Simon was there, too (perhaps also Marvin Minsky, but my memory might be deceiving me). Later in the 1980s I attended a lecture by David Rumelhart in Paul Feyerabend's colloquium at the ETH Zurich, where he described his backpropagation algorithm for neural networks.

Habilitation. From October 1983 onwards, I had an assistantship with Mittelstraß[19] with much time for research. As my professional duty apart from teaching, I worked on his Encyclopedia. It was also expected that within the time period of this assistantship — six years — I would do my Habilitation, as normally required for the position of a professor in Germany. In the years after my doctorate, I wrote quite a number of articles on the topics mentioned above (Section 6), but no single coherent piece of work as demanded by the Philosophical Faculty as a Habilitationsschrift. In 1987 I decided to put all non-historic logic materials together, essentially around the theme of higher-level rules, which was already the topic of my doctoral thesis, but now extended in various directions. I included a chapter on relating my framework to the sequent calculus rather than natural deduction, to the bunch-based system of relevant logic (see below, Section 7), to logic programming and also to the framework of Martin-Löf type theory. Putting my family under enormous stress — Gabi was working as a postdoc in Zurich and our second child was born in March 1987 — I wrote it in a couple of months and submitted it in late summer, shortly before we left for a stay in Sweden from September 1987 until January 1988. The thesis (A1987c) was relatively short, but satisfied the requirements.[20] Prawitz was one of the reviewers, and I was later told that Prestel, who had attended the faculty meeting as an external expert, argued: "If Prawitz says yes, the faculty can't say no". In the obligatory colloquium with the Philosophical Faculty I spoke about "What is probability?" — the topic had to be different from that of the thesis and was selected from three themes submitted by the candidate. I talked about probability in a very general way to the humanities scholars in the audience. Because musicology was not represented as a subject at Konstanz, I even dared to say something about the Tristan chord and its relative frequency in music history, mentioning its occurrence in Beethoven's op. 31,3 piano sonata which I had learned about in Bonn with Martin Vogel: something that would have been a no-go for me if musicology professors had been there.

There was a formal inaugural lecture shortly afterwards. I still have the handwritten slides for it. I talked about logic and its future with quite some emphasis on issues of computer science and of artificial intelligence I had become interested in (including

[19] Assistant professor without tenure would be the closest American analogue.

[20] It circulated as a manuscript and was later made available on my website. A publication as a book in the Bibliopolis series "Studies in Proof Theory" was recommended by Dag Prawitz, but did not materialize.

nonmonotonic reasoning, polymorphic typing and hardware verification). So my general intellectual attitudes towards the end of the 1980s, when I moved to Tübingen, was a compound of proof-theoretic semantics, foundations of computer science, logic programming, general psychology and cognitive science.

7 Konstanz 1981–1989: Start of long-term collaborations and long-term friendships

At the time of my Ph.D. and shortly after I made scientific contacts and established personal friendships that lasted for the whole of my career and shaped my research topics.

Lars Hallnäs, definitional reflection and extensions of logic programming. Extending logic programming by means of implications in the bodies of clauses was a natural idea which fitted well with my approach of higher-level rules developed in my thesis. However, Lars Hallnäs had an idea which went much further, namely using the schema for general elimination rules as a schema for the inversion of systems of arbitrary, not necessarily logical rules, in particular rules of a logic program. He called it the principle of *definitional reflection* as one reflects on the given definition as a whole in order to invert it, the approach itself being called "definitional reasoning". Hallnäs's idea was to incorporate it into logic programming by means of a logic programming language which allowed one to evaluate goals according to this rule. In the 1990s, with his collaborators at the Swedish Institute of Computer Science (SICS), which was very much involved in implementing PROLOG, he developed such a system. We published the idea of proof-theoretic extensions of logic programming including definitional reflection in a two-part article at the end of the 1980s (A1990a, A1990b). From that the idea emerged to set up a series of conferences on *Extensions of Logic Programming (ELP)*. This worked out very well. We initiated and partly organized five conferences with corresponding proceedings: ELP1989 in Tübingen (C1991b), ELP1991 in Stockholm (C1992), ELP1992 in Bologna (C1993), ELP1993 in St. Andrews (C1994) and ELP1996 in Leipzig (C1996).

From the beginning of the 1990s Hallnäs and I have put more emphasis on definitional reasoning as a foundational approach that goes way beyond logic programming and is a general reasoning principle, as originally intended by Hallnäs (1991, 2006) (see my A1993, A1994b). Since it can be applied to any system of definitional rules, thus also to the rule defining p in terms of *not*-p, it has applications to paradoxical reasoning, and to any kind of non-wellfounded definition. This is why Hallnäs spoke of "partial" inductive definitions. This had implications for my later work on paradoxes. Hallnäs had done his Ph.D. with Prawitz in 1983 on normalization in set theory (Hallnäs, 1983), in which he had been strongly involved in the proof-theoretic treatment of paradoxes. Our work on definitional reflection quickly grew into a friendship between us and our families, with many short and long visits to each other's homes. Lars is

also a composer by training[21], and his present for my 60th birthday was a composition for organ.

Unfortunately, we were not very successful with these ideas about definitional reflection. This is partly due to the fact that there is an obsession in philosophical proof-theoretic semantics to deal with logical constants, which are particularly well-behaved. Many do not realize that an inductive definition is like a set of introduction rules, a fact well established in mathematical proof theory. Dale Miller and his group were essentially the only ones beyond groups in Sweden to appreciate this approach. Had there been more resonance, we would have pursued it further into more advanced directions including definitional reflection for definitions of functions and functionals. There is some work by Hallnäs in this direction (see his contribution to this volume, Hallnäs 2023), which presents a good starting point for such investigations.[22]

Kosta Došen and logical constants. I met Kosta Došen at the logic colloquium in Florence in 1982. He had recently finished a D.Phil. thesis on logical constants with Michael Dummett and Dana Scott at Oxford, where he developed a theory of logicality based on an idea of iterated sequents (today one would speak of "hypersequents") and rules which could be read both downwards and upwards and which he called double-line rules. Later, through his publication on logical constants as "punctuation marks" (Došen, 1989), this approach became quite popular and widely discussed. I approached him personally in Florence because I was attracted by the abstract of his talk, where I saw immediate similarities to my idea of rules of higher levels. There was a certain difference, as his higher-level sequents essentially served for the interpretation of modal connectives whereas mine served for the interpretation of implication, for which he did not need any higher-order entitities. When we met, we got on well together and stayed friends for life. In Konstanz I worked with him on the notion of conservativeness and uniqueness of logical operators. In our joint papers A1985 and A1988 written in Konstanz in the mid-1980s we showed the duality of these notions and broke down the uniqueness problem to the interaction of two consequence relations. Later we initiated the topic of substructural logic (see below, Section 8). At the beginning of the 1990s he and his wife stayed with us in Tübingen for some time as he could not return to Belgrade during the Yugoslav war. In the middle of the 1990s, Kosta Došen turned towards category theory, which I did not find so interesting at the time. He became one of the leading figures in categorial proof theory, building very much on the work of Joachim Lambek. This was roughly at the time when he was giving up a full professorship in Toulouse in 1998, which he had held since 1994 after a two-year stay in Montpellier, that is, from the breakup of Yugoslavia onwards, to take up a professorship in Belgrade, after his arrival spending nights in bomb shelters during the NATO air raids of 1999. Later on, towards the 2010s, our contacts became more frequent once again, as we realized that his categorial approach to logic and my idea of the primacy of the hypothetical over the categorical converged. Sadly, in spite of an innovative cancer treatment in

[21] For his musical biography see https://quatuorbozzini.ca/en/artiste/hallnas_la.

[22] Cp. our notes presented to Dag Prawitz on his 80th birthday (M2016).

Germany the year before, he died in 2017, aged only 63. I delivered a eulogy at his funeral (M2022c).

Neil Tennant and the Scottish connection. I first got to know Neil Tennant through a paper in the *Journal of Philosophical Logic* (Tennant, 1980), in which, among other issues, he claimed to have given a proof of normalization for full classical logic, something which was correctly proved by Gunnar Stålmarck (1991)[23] some time later. I pointed Tennant to the deficiencies in his proof, and he invited me to a short stay at Stirling (Scotland) in 1982, where he had just started an appointment as a professor. It was there that I gave my first talk abroad in English. All presentations I had given before had been in German — times were different from today, where English has become the standard language of communication even in my Tübingen department. A follow-up stay — again with Gabi — took place in Winter/Spring 1983 when I was fellow of the Institute for Advanced Studies in the Humanities in Edinburgh, which was very productive.

Through Neil Tennant we met Stephen Read, and through him Roy Dyckhoff. This lead to an enjoyable and productive several-month stay in St. Andrews in spring 1985. Through Read I became acquainted with "standard" relevance logic — Tennant's deviating approach, from which his later *Core Logic* (Tennant, 2017) evolved, was already known to me. Particularly interesting was the proof-theoretic approach distinguishing different conjunctions at the structural level, something that Read and John Slaney had developed relying on earlier work by Michael Dunn. It was based on so-called "bunches" as a specific sort of structural entities and made it easy to formulate general rules for certain intensional connectives. I was quite enthusiastic about this topic and included it later in my Habilitation thesis (see above, Section 6).

Roy Dyckhoff, who had come from category theory and was now working in logic in computer science, I met originally as an attendee of the philosophical seminars in St. Andrews. As his work was so closely related to mine we collaborated intensively over the years. He was also somebody working on a contraction-free calculus for intuitionistic propositional logic independent and in parallel to the results in the Ph.D. thesis of Jörg Hudelmaier, who was a member of my group in Tübingen in the 1990s. Roy Dyckhoff was the first to propose general elimination rules without higher levels ('flat' general elimination rules, see below, Section 10). He died in 2018, aged 70. At his memorial service at St. Salvator's Chapel of the University of St. Andrews the bells were rung, for which he himself, as a passionate bell-ringer and theoretician of bell-ringing (see Dyckhoff, 2018), had raised funds.

8 Tübingen 1989–1997: Logic, philosophy, computer science

Application and appointment. After unsuccessful applications elsewhere, I became professor for logic and philosophy of language at the University of Tübingen in 1989. The support of Franz Guenthner, who was professor of general and computational

[23] I was an examiner of Stålmarck's Stockholm M.A. thesis which resulted in this article.

linguistics in Tübingen, was decisive both for the installation of this professorship and for my appointment. He was the editor with Dov Gabbay of the *Handbook of Philosophical Logic* and had first taken notice of my work through Göran Sundholm's *Handbook* conribution on proofs and meaning (Sundholm, 1986). As mentioned above in Section 5, Göran had been a reviewer for the *JSL* of my thesis publication. My application lecture was on "Logic and Cognition" — I tried to give an impression of my inclination towards cognitive science going beyond logic in the narrower sense.

Institutional struggles, offer from Berlin. In the fall of 1989 I started to teach in Tübingen. This was a wild time politically. I remember seeing the pictures of the collapse of the Berlin wall on TV during a stay in Tübingen. We were extremely lucky to buy a house in Tübingen at a very reasonable price in January 1990 that we rebuilt and refurbished, so that we could move into it at the end of the summer of 1990, just in time for school and the academic year. For me the first years in Tübingen were accompanied by certain struggles due to institutional issues between philosophy, computer science and linguistics. With the backing of the computer algebraist Rüdiger Loos, this was finally solved for me by moving into the newly founded department of computer science, with philosophy as a second affiliation, so that I had a joint appointment in computer science and philosophy, which for me and my range of interests was ideal. In 1991 I took my office in the computer science building, a former rehabilitation hospital with a beautiful view over the hills nearby and have stayed there until today.

Shortly after starting the Tübingen position I received an offer of a professorship in logic at the Free University Berlin. David Pearce, who at that time was working at Berlin, did all he could to get me there even though he was a strong candidate for the position himself. However, in the end I decided to stay in Tübingen where I had just started and our family had just settled.

Substructural logics. Apart from "proof-theoretic semantics" I am happy to have been involved in the coining of another term: "substructural logic" or "substructural logics". This term made it into the Mathematics Subject Classification, its current codes being 03B47 and 03F52 (American Mathematical Society, 2020). The situation unfolded as follows. I had just started my professorship in Tübingen in autumn 1989, while Kosta Došen had received a Humboldt grant for 1990–1991 and was staying in Konstanz. Franz Guenthner was in the possession of funds that he offered to me to spend on promising conferences. The first one in December 1989 was on *Extensions of Logic Programming* — see above (Section 7); the second one in October 1990 was organized together with Kosta Došen and originally entitled *Logics with Restricted Structural Rules*. We managed to invite top, or better the top, people in the respective fields. Our speakers were J. Lambek, S. V. Soloviev and J. van Benthem for the Lambek calculus, R. K. Meyer and J. M. Dunn for relevant logic, J.-Y. Girard, G. Sambin and A. Scedrov for linear logic, and V. Grishin and H. Ono for BCK logic. One or two days before the conference started Kosta rang me up: "*Logics with Restricted Structural Rules* is too clumsy, we must find a term that catches. I propose *Substructural Logics*. According to the Oxford English Dictionary it means 'relating to a substructure', that is, to the foundation of something." Initially, I was

opposed to this, as it sounded like "subculture" to me, but eventually I gave in. The volume that resulted from the conference then bore this title (C1993). While the title of the book worked well, publication came with some unexpected problems. We had proofread everything except the title page and cover, and when the book came out, the editors appeared in non-alphabetical order: Schroeder-Heister & Došen. Because it was me who corresponded with Oxford University Press, they had assumed I was designated first editor and changed our specified author order! Cover disaster aside, the project was a great success. A vast amount of literature on "substructural" logics has appeared in the meantime, including two textbooks.

Logic and cognition. Even though the formal denomination of the professorship in Tübingen was "Logic and Philosophy of Language", and even though I was a full-fledged logician, my interests had shifted in the time after my Ph.D. to many other subjects, and in particular to the cognitive realm. There had been various influences on me, and my involvement in cognitive psychology through Gabi certainly played a major role. Most interesting to me was that people started to see connections between logical or symbolic reasoning in the narrower sense and more general reasoning models developed in the cognitive sciences. Moreover, my position was connected to the "Institute for Natural Language Systems"[24] founded by Franz Guenthner, with its associated curriculum and degree in General Linguistics with Psychology and Computer Science. This was a four-year M.A. course in cognitive science with emphasis on (both general and computational) linguistics, with computer science and psychology as obligatory minors. It had absolutely brilliant (individually selected) students from different backgrounds who had all sorts of interests besides their main fields of study. I remember, for example, a very stimulating talk by Joseph Weizenbaum whom they invited to discuss the social impact of recent developments in information technology and artificial intelligence. They were a pleasure to teach — an experience that I enjoyed already two years prior, as a substitute professor.

This suited me extremely well, and the titles of the courses I initially taught in Tübingen were all, in a sense, related to the cognitive field. Almost immediately after my start in Tübingen, in the autumn of 1990, I led seminars on connectionist modelling. My philosophy colleague Rüdiger Bubner complained when I made an announcement using the title "PDP", which sounded to him like the name of a political party in the GDR (it ceased to exist shortly afterwards in October 1990). He was right, of course, so I named it "Connectionism" and discussed the topic using its full name: "Parallel Distributed Processing", the title of the collection by David Rumelhart and James McClelland (1988) that was being widely debated at the time. I also taught philosophical foundations of cognitive science, philosophical critiques of AI, causality, connectionist reasoning systems and the like; many issues that would fit very well into the current landscape, where AI is the big thing, including in philosophical discussions.

A great success was a four-year research grant on the cognitive side of deduction as part of a programme called "Cognition and the Brain" by the German national science foundation (DFG) at the beginning of the 1990s together with a simultaneous grant

[24] "Seminar für natürlichsprachliche Systeme"

on efficient Gentzen systems within the DFG programme "Deduction". I was able to hire as postdocs Venkat Ajjanagadde, who had just finished a much quoted paper with L. Shastri on the connectionist representation of rules (later published as Shastri & Ajjanagadde 1993) and Seppo Keronen, who had done his Ph.D. with R. Stanton on "Computational Natural Deduction" (D1991); and, as a doctoral student, Uwe Oestermeier, who finished with a thesis on pictorial and logical thinking, which won a dissertation prize at the university (D1993).

What I was interested in, with respect to cognitive science, was the establishment of a proper combination of symbolic and connectionist modelling of logical reasoning, something that would be at the cutting edge of science nowadays. Had I pursued that further, my research could play a role in today's discussions, but this would have been at the expense of proof-theoretic semantics.

My interests in connectionism and AI were essentially from the philosophical side. At the same time I did a lot of teaching in theoretical computer science, as in the beginning of the 1990s the chair devoted to that field was not yet filled. I taught both advanced topics such as denotational semantics of programming languages, but also elementary courses on formal languages and computatability with large audiences. This was very much in accordance with my interests in logic programming as well as in type-theoretic approaches and thus advanced functional programming.

At the beginning of the 1990s I also had Jörg Hudelmaier working with me, who had done excellent work on the complexity of intuitionistic theorem proving, and with whom I published a paper on the non-commutative Lambek calculus (A1995) demonstrating certain limits for cut elimination. Ernst Zimmermann, whose Ph.D. thesis of 1995 on the philosophy of modal logic (D1995) I co-examined, has been present in my group for the past 20 years.

In Tübingen I had two collegues, on whom I relied heavily. On the philosophy side this was Walter Hoering, who was the logic professor in philosophy until he retired in 1998 (M1998a, M2019a) and on the informatics side this was Rüdiger Loos, who was instrumental for my affiliation to computer science (see above) and without whom computer science would not exist in Tübingen in its current, independent, and strong form.

Large public lecture series on "Music in the Sciences" and "Music and Informatics" (organized together with Loos), on "Thinking and Calculating", and a full public lecture series on the work of Karl Popper immediately after his death in 1995 (delivered with the philosopher of science Herbert Keuth), which were all very well received, serve to show that at the time, proof-theoretic semantics was not yet my key occupation.

9 London 1997–2000: Back to my roots

Sabbaticals and research stays. Longer stays at other institutes and with other colleagues, especially abroad, are normally highlights of a researcher's career. I only mention the longest one, in London, from 1997 to 2000 (others took place in

Edinburgh, Stockholm, Berne, Paris and Oxford). Initially, I had only a sabbatical in the winter term 1997–1998, for which I had an invitation from Dov Gabbay to Imperial College London. We went to England as a family and rented a house in Kingston-upon-Thames, not far from the German School. We extended our stay, and in the end remained there until summer 2000, that is, for a total of three years. From the summer semester 1998 on, I commuted between Tübingen and London, flying to Tübingen for three days a week during the semester and spending the university vacations in London. This was possible at the time without impairing my duties in Tübingen. I still conducted the full teaching programme of nine hours per week of classroom teaching, split between philosophy and computer science. Besides Imperial College, I was, through Edmund Robinson, also affiliated with Queen Mary and Westfield College (QMW, now Queen Mary, University of London). While working there in computer science, I met David Pym with whom I still collaborate today. On a personal level, staying in London was one of the best decisions we made in our lives, in particular as far as our children were concerned. It was a great experience that shaped their future. They both went on to study and live in England.

Logic in philosophy. In a sense the time in London marks my returning from a dominant interest in cognitive science (still with an emphasis on logic) which I had retained for more than a decade, to logic and in particular philosophical logic and philosophy of logic. These were my roots when I started studying in Bonn and also my occupation in my doctorate. Even the computer science aspects of my work I now started seeing from the perspective of philosophical (rather than mathematical) logic. For example, the inversion rules in extended logic programming languages could be seen as philosophically motivated inversion principles in a proof-theoretic semantics of logical and other signs. This did not mean that I gave up cognitive science or computer science, but rather considered it as a background and application for more philosophical theories. What contributed to it was the joint Tübingen-Konstanz research group *Logic in Philosophy* which I initiated, and which I applied for together with Wolfgang Spohn (who had just taken up a professorship in Konstanz) in 1996 with many brilliant philosophical projects both in Tübingen and Konstanz (C2005). The project started exactly when we went to London, so the Tübingen part was partly directed from London. Through this I collaborated with Patrizio Contu and Reinhard Kahle. With Patrizio I published my first paper on hypothetical versus categorical reasoning (A2005), with its philosophical claim that the hypothetical concept of consequence should be given conceptual priority over the categorical concepts of truth or provability.

The Salzburg offer. In the year 2000 the University of Salzburg offered me a chair in philosophy — the successorship to Paul Weingartner, who had just retired. This was quite unexpected. I almost decided to accept it; we even registered our children at local schools. However, it finally failed for administrative reasons, which had essentially to do with the German pension system and the losses I would have incured when moving to Austria, even though it was within the European Union. Turning it down was a hard decision as there was only little that could be gained in Tübingen; for example, the fact that my position in Tübingen was a second-class professorship, not

a chair, was non-negotiable. Even though I declined it, the Salzburg offer contributed to my intellectual shift back towards philosophy.

10 Tübingen 2000–2019: Topics in proof-theoretic semantics

During my time in Tübingen, and in particular after the stay in London, which fixed, so to speak, the topic of proof-theoretic semantics as the key topic of my group, I have worked on various aspects of proof-theoretic semantics. There was no linear order. I tried to identify critical topics and find solutions to certain problems, though many of them are still open (A2016a). I shall just mention a few of them.

Definitional reflection, paradoxes and intensional proof-theoretic semantics. I have already mentioned Lars Hallnäs's idea of definitional reflection and the idea of definitional reasoning, which my mind always kept returning to. It is clear that definitional reasoning constitutes a kind of intensional proof-theoretic semantics, as definitions are intensional entities. It depends on the sort of definitions whether the system obtained is well-behaved. Hallnäs called this well-behaviour, where we have cut elimination and the like "total". Changing the definition, that is, changing the meaning of terms, changed the global behaviour of the system, so there was a strong sense of non-locality. The standard example was always the definition of p by *not-p*, which should be allowed (as in logic programming), but which globally leads to systems without normalization or cut elimination and which can be seen as the skeleton structure of the various logical, semantical and mathematical paradoxes. One follow-up question is then under which conditions definitions are "well-behaved" and are in this sense extensional entities. In several papers I showed that this strongly depends on the structural rules available, whereby these structural rules interact (A1992a, A2016b), including the seemingly trivial structural identity axiom ("A entails A"). I could also show that, in order to avoid paradoxes, a very limited restriction of the rule of contraction is sufficient, in contradistinction to abandoning this rule altogether (A2012b), based on an intensional distinction of the ways a formula is 'given' to us — either through a definition or without any specification (A2022b). I even speculated on the idea of a 'free' type theory, in which the application of rules depended on the evaluation of certain definitional terms (A2012d).

A purely intensional paradox was discovered by Jan Ekman in a thesis he wrote supervised by Hallnäs (Ekman, 1994). By translating set-theoretic paradoxes into propositional logic he could show that under certain assumptions derivations in propositional logic cannot be normalized. This has fascinated me ever since its appearance, and I often mentioned it in talks. I knew Ekman from the time he was still a doctoral student, and would have liked to employ him on a post in the research group *Logic in Philosophy*. However, he had already accepted a permanent position in industry at that point. Only in recent years did I manage to write two papers on the subject, together with Luca Tranchini (A2017, A2021). By that time Luca had embarked on a different notion of intensional proof-theoretic semantics, which is

concerned with the identity of proofs, and an objection we made was that from this point of view Ekman's translation has certain deficiencies. This is closely related to the issue of harmony, and Luca is pursuing this strand of research.

In fact, I came across the issue of identity of proofs when I was external examiner ("opponent") at the doctoral defence of Prawitz's student Filip Widebäck in 2001, who had written on exactly that topic (D2001). In preparing my talk I had noticed that Widebäck presented results that had been obtained independently by Kosta Došen at the same time. Only much later I realized the significance of these issues for the sort of proof-theoretic semantics I was advocating. Incidentally, a thesis defence in Sweden, with an external opponent who first presents the thesis (to a potentially large audience), and then publicly questions the candidate about it, is a great experience in its own right, even if it costs the opponent a week of preparation as he needs to read the thesis really carefully. Before I had acted as opponent in Sweden once already, for Lars-Henrik Eriksson in computer science, who had worked on extensions of definitional reflection (D1993).

Direct negation, Béziau's square, and bilateralism. In the still unpublished part of my doctoral dissertation, I had already discussed an extended natural deduction system with a 'structural' kind of negation — structural, because it is built into the deduction machinery and not a logical constant. Such negations, also called "denial", sometimes "strong negation", have been considered for a long time, not only in philosophy but also in computer science. I discovered that the idea of definitional reflection gives rise to a slightly different sort of denial, as it allows a distinction between a direct denial based on definitional clauses, and an indirect denial induced by definitional reflection. The latter resembles, in a rough way, what in logic programming is called negation by failure. Correspondingly, we obtain two sorts of assertion, namely direct assertion (by definition) and indirect assertion (by failure). I presented some initial ideas at the first Uni-Log (Universal Logic) conference organized by Jean-Yves Béziau in Montreux in 2005. This was an excellent meeting with, for example, Saul Kripke present (as a so-called "secret speaker", who was not announced beforehand).

It turned out that this idea with two sorts of assertions and two sorts of denials could be put into the structure of a square, so I presented it in 2007 at the first conference on the square of opposition by Béziau, again in Montreux. Initially it seemed to me to be a crazy idea to devote a conference to this topic, and later a whole series of conferences, but it turned out that there were sufficient serious ideas in it to compensate for the stranger aspects. The conference proceedings only appeared in 2012. I still consider my contribution there (A2012a) significant, although it had no impact whatsoever. When I was invited for the third congress on the square in 2012 in Beirut, I presented an even further developed variant of my approach, calling it a "calculus of squares". I arranged the two assertions (or positions) and the two denials (or negations) at the corners of a square, proposed a sequent calculus for such entities and discussed major theorems (cut elimination etc.) for such a system. The conference was an impressive event at the beautifully located American University. People in Beirut tried to live their lives normally despite the Syrian war having started the year before. To us as visitors at least, it was not much felt there yet. I have not continued this

line of research since, partly because of lack of resonance, partly because I have been engaged in other issues of proof-theoretic semantics. I think it has a great potential. It would fit very well into today's discussion of what is called "bilateralism".

Béziau also invited me to give a course on proof-theoretic semantics in 2007 at the second Uni-Log conference in Xi'an (China), which took place after the LMPS congress in Beijing. Even though, or perhaps because I essentially talked to colleagues rather than to students, it was extremely instructive to me. I later gave a related course at the ESSLLI conference in Bordeaux in 2009, this time to students (A2009a). I had already given a course with Lars Hallnäs at ESSLLI 1993 in Lisbon and have always considered the ESSLLI summer schools a great success story. Thomas Piecha and myself also organized a workshop at the sixth Uni-Log in Vichy in 2018 (C2018b). Jean-Yves Béziau, through marketing projects in a way one is not normally used to in logic, contributes to keeping the machinery of research interaction running. I myself have strongly profited from this, as I would probably not have developed certain ideas otherwise. The fact that we now have a *World Logic Day* (14 January) approved by UNESCO and celebrated even during the Covid pandemic as a massive collection of online events, is also the result of Béziau's initiative.

Assumption-conclusion bilateralism, hypothetical reasoning and the dogma of standard semantics. As far as bilateralism is concerned, I have always understood it in the sense that reasoning should not only be one-sided, from unspecific assumptions towards a specific conclusion, but two-sided in the sense that in the course of reasoning, both the assumptions and the conclusion can be modified according to semantic rules. This is in certain ways related to negation-bilateralism, but rests on different intuitions, as no negation is involved in the first place. I have argued in favour of this sort of bilateralism[25] in the context of criticising standard semantics. By standard semantics I mean both model-theoretic semantics and most intuitionistic semantics such as BHK, realizability, validity-based semantics in the sense of Prawitz and Dummett etc. All these semantics start using a categorical concept such as truth or validity. Then a hypothetical concept of consequence is defined as the transmission of this categorical concept under all circumstances. Assumptions are nothing but placeholders for categorical entities (truths or valid proofs) — the specific semantics only applies to conclusions. This motivates the emphasis on introduction rules in standard proof-theoretic semantics. I called this a *dogma*, as there is no real reason for choosing this sort of approach. Why not choose a hypothetical concept of consequence as a basis and then derive the categorical from the hypothetical rather than vice versa?

There is a formal model of such an approach in the form of the sequent calculus, where one can modify both the left and right side by independent rules. There is also categorial proof theory which is essentially based on such an approch, as one deals

[25] I spoke of "bidirectionality" instead of "bilateralism". Perhaps this is a preferable term to avoid terminological confusions.

with hypothetical entities (arrows) from the beginning.[26] However, this has never been developed into a philosophical foundational theory.

I developed this idea together with Patrizio Contu in our research group "Logic in Philosophy" in the late 1990, who very strongly insisted on it (A2005), and have 'preached' it ever since at many conferences and many occasions, with limited success. Certainly, my arguing was more programme than execution, but the target was clear: developing a semantics of hypothetical judgements as primordial entities, that is, whithout presupposing categorical judgements. I remember presenting it at the conference of the Society for Analytical Philosophy in Bielefeld in 2004 (A2004), the Logica conferences in Hejnice in 2007 and 2008 (A2008b, A2009b), at a conference at the Swedish Collegium for Advanced Study (SCAS) in Uppsala in 2010 (A2012e), where a great deal of the proof-theoretic semantics community was present, on several occasions in Kosta Došen's colloquium in Belgrade, as well as in a manuscript for a special issue of *Erkenntnis* that never saw publication (M2008c). Perhaps there will at some point be a student prepared to work out this concept in detail, both on the classical and intuitionistic side.

Harmony. Harmony — a term introduced in this context by Dummett (1973, pp. 396f.) — is considered one of the fundamental notions of proof-theoretic semantics. The conditions for asserting a sentence should match the conclusions that can be drawn from it. What it should mean in detail is still a matter of discussion. In my thesis I tried to achieve harmony by generalized elimination inferences for logical constants which guarantee it due to their syntactic form. To enable this I had to introduce higher-level rules, as otherwise implicational connectives could not be interpreted. They are also a crucial tool to interpret certain connectives, for example, of relevance logic such as fusion (A1987c). Now there is also another variant of generalized elimination rules put forward by Dyckhoff, Edgar López-Escobar, Tennant and Jan von Plato, which are of an elementary level (called "parallelized" by Tennant) and are a natural deduction translation of Gentzen's sequent calculus rules (see my A2014b, and above, Section 7). Their adherents have always preferred these 'flat' rules and even claimed that they provide a framework as powerful as mine to generate harmonious rules. It bothered me for many years that I did not have an easy argument at hand that certain constants could not be expressed using flat generalized elimination rules. This changed when I met Grigory Olkhovikov. He was a student of Grigori Mints, and Mints recommended him to me at a conference in Bochum in 2012. When I posed the problem to him — translated into a problem in intuitionistic propositional logic — he came up with a solution, which resulted in a joint paper in the *Review of Symbolic Logic* (A2014a, with an extension to arbitrary levels in A2014b). This settled the issue, although incorrect claims continue to be propagated.

However, this was still a limited notion of harmony, as it only laid down appropriate elimination rules given certain introduction rules (something which, by the way, also could be inverted; A2015c). A more universal notion would tell when a given pair

[26] Note that here the term "categorial" refers to mathematical category theory, whereas "categorical" is used in the traditional philosophical sense of denoting a categorical in contradistinction to a hypothetical judgement.

of introduction and elimination rules is in harmony. I made such a proposal in my contribution to the "Outstanding Contributions to Logic" volume in honour of Dag Prawitz (A2015a) and a subsequent paper in *Studia Logica* (A2014d), where I gave second-order translations of the introduction and elimination meanings of connectives and declared them to be in harmony when these translations were equivalent. This was a conceptual achievement, I think, even though it is 'only' an extensional notion of harmony based on deductive equivalence. Luca Tranchini is developing this approach further in the direction of an 'intensional' notion taking proof identity into account. (I made some remarks on this issue in A2016a.) He elaborated on this idea partly through interaction with Kosta Došen and his work, with whom we were quite intensively collaborating in the 2010s, and also worked with Paolo Pistone, who had done his Ph.D. with Girard and was in Tübingen as a postdoc in 2018–19.[27]

Incompleteness and atomic systems: Beyond (intuitionistic) logic. As mentioned above (Section 6), the idea that proof-theoretic semantics could in a sense justify intuitionistic logic, in that the latter is complete with respect to the former, has fascinated me ever since my Ph.D. thesis. In the past fifteen years, Thomas Piecha and myself have essentially come to a negative conclusion. This was considerably stimulated by exchanges (including mutual visits) with Wagner de Campos Sanz and also with Tor Sandqvist, both of whom had the idea that certain only classically valid formulas are verified by proof-theoretic semantics. Several papers with de Campos Sanz and Piecha (A2014, A2015) resulted from that, and in the end Piecha and I found a counterexample free from deficiencies, depending only on a few plausible assumptions about the proof-theoretic semantics used (A2019c). This is, of course, a somewhat negative result, and not what I had expected 35 years ago, but perhaps it is a prejudice to think that intuitionistic logic is *the* proper logic of constructive or operational reasoning. As a desideratum there remains, of course, to describe this proper logic, if there is (a finitely axiomatizable) one.

Closely interconnected with the completeness problem is the question of the atomic base of proof-theoretic semantics. In model-theoretic semantics it is structures that tell us which atomic sentences are true and which are false. In the proof-theoretic case one considers atomic deduction systems instead. However, what kind of such systems are admitted strongly influences the logic one obtains, in particular if in atomic systems not only production rules but more general rule concepts including definitional reflection are allowed, as joint work with Piecha shows (A2016b, A2017). Quite independent of any logic built on top of atomic systems, these systems in themselves represent a highly interesting topic that goes way beyond logic and impacts, for example, the theory of inductive definitions as well as rule based argumentation theory. Given the current interest in the latter, this is perhaps a future research field to invest in.

The format of deduction. One topic that interacts with almost all others is which form a logical deduction should take. Proof-theoretic semantics has a bias towards natural deduction, but the sequent calculus is another possible format which fits

[27] Cp. their joint contribution to this volume (Pistone and Tranchini, 2023).

very well in particular with approaches that want to make assumption-conclusion bilateralism explicit and allows one, for example, to establish a perfect duality between conjunction and disjunction, something on which Došen's categorial approach and Giovanni Sambin et al.'s Basic Logic (Sambin, Battilotti, and Faggian, 2000) rests. Definitional reflection works under both formats, and I have argued it has certain advantages over Sambin et al.'s approach (A2013). However, as soon as it comes to implication, not even the standard rules of the sequent calculus are exempt from criticism. I have claimed that for the introduction of implication on the left side (the assumption side) different rules might be considered, if one wants to distinguish between implications as rules and implications as links (A2011b, A2014b)[28] and thus to disentangle logical from structural features of implication. This also plays a role if one wants to give the approach of higher-level inferences rules in my doctoral thesis a sequent-style formulation, as Arnon Avron has pointed out (Avron, 1990 — incidentally the only paper referring to me in its title). In addition, there is the even more fundamental question of the role of proof search, that is, the idea that in reasoning we often start with a goal that we want to prove, and then *reduce* it to subgoals. Perhaps "reductive" reasoning in this sense should be given a more fundamental stance, something which is the case, for example, in dialogue logics. We have been involved in the latter through research grants (see Section 11), and Thomas Piecha and myself have worked on it (A2012, A2015a) — in fact, Piecha wrote his doctoral thesis on it (D2012), which represented a major advance for the field. However, I think the general problem of deduction versus reduction still needs to be settled, possibly in a much wider framework.

11 Group, grants, cooperations

In the sciences it is absolutely standard to work with a group of people with whom one can discuss problems and results, and to publish with them. In the humanities, there is still a considerable number of single researchers who reach (often outstanding) results without much interaction. I definitely wanted to have at least a small group of, say, three people to talk to. I had some money from the university to pay for one position, which I secured when I turned down the Salzburg offer. For all the rest grants were needed, both doctoral and postdoc grants.

I mentioned already the grants in cognitive science and deduction as well as the research group "Logic in Philosophy" during my first decade at Tübingen and London (Sections 8 and 9). Successful grants after 2000 were a grant from the European Science Foundation (ESF) on "Dialogical Foundations of Semantics" (2008–2012) for a joint project with groups in Lisbon (Reinhard Kahle) and Amsterdam (Benedikt Löwe) as well as a 10-year long French-German collaboration on "Hypothetical Reasoning" (2009–2019) within a joint programme of the French and German national science foundations (ANR and DFG), with the Institut d'histoire et de philosophie

[28] Cp. Michael Arndt's (2023) contribution to this volume.

des sciences et des techniques (IHPST) in Paris. All this made it possible to hire as postdocs Kai Wehmeier, who was brilliant but left after one year to become a professor at the University of California, Irvine, and Bartosz Więckowski who in his thesis had created the topic of subatomic natural deduction (D2006) — now a growing branch of proof-theoretic semantics. Later, it allowed the recruitment of Rainer Lüdecke, who wrote a thesis on the game semantics of logic programming (D2012), Harald Maurer on connectionist modelling (D2014), Tiago Rezende de Castro Alves (D2019) on identity of proofs, Hermogenes Oliveira (D2019) on proof-theoretic validity and René Gazzari on the notion of occurrence (D2020).

In addition to providing money to employ researchers, the French-German project was a very successful research cooperation through which we made many new friends. It opened up entirely new perspectives, with many exchanges and research stays in Paris, and also a number of joint conferences. I knew Michel Bourdeau and Jean Fichot, the French project leaders, from the 2007 meeting on one hundred years of intuitionism at Cerisy castle (van Atten, Boldini, Bourdeau, and Heinzmann, 2008), where I gave a talk on Lorenzen's *Operative Logic* as an approach to proof-theoretic semantics (A2008a) and met Michael Dummett again after the 1999 Tübingen conference on proof-theoretic semantics (Section 12). As the attendence of the conference exceeded the number of available single rooms, Jean Fichot and I had to share a room, which fostered our friendship. Within our own project, Jean organized a meeting at the same place in 2017.

Another collaborator over the decades was Reinhard Kahle. He worked as a postdoc in our research group "Logic in Philosophy" and has collaborated with us ever since during his long time as a professor in Lisbon. In 2018 he was appointed to the newly created Carl Friedrich von Weizsäcker chair for Philosophy and History of Science in Tübingen. It is not a dedicated logic position, but, as Reinhard is a logician, logic will be represented there (my own professorship has been discontinued as a logic position).

Most important for shaping the field of proof-theoretic semantics in Tübingen were the three members of my group who were constantly present during the last decade of my work there and whom I would name first when speaking of "my group": Michael Arndt (thesis D2008), Thomas Piecha (thesis D2012) and Luca Tranchini (thesis D2010). I benefited enormously from my interactions with them. None of the papers we wrote together could have been written by me alone, and I am sure that many of my single-authored papers would be of poorer quality.

12 The Tübingen conferences on proof-theoretic semantics

We organized three major conferences in Tübingen on proof-theoretic semantics which initially served to establish the subject under this name and then to keep its status. The first of these I organized in January 1999 at Tübingen castle together with Reinhard Kahle. We had quite prominent guests there including Dummett, Prawitz, Martin-Löf, Mints and William Tait. We had big problems obtaining a visa for Grigori

Mints, who was living in Stanford but had Russian citizenship. In the end, I made this public in the local daily newspaper. First thing in the morning on the day the paper appeared, I was rung up by Herta Däubler-Gmelin, our local Tübingen member of parliament who at the time was Minister for Justice in the federal government. She solved the problem by intervening through channels available to her, and Grigori was able to come.[29]

It took seven years for the proceedings to appear (C2006), partly due to me, as I needed quite some time for my own contribution on validity concepts in proof-theoretic semantics (A2006d). Originally I had wanted to write about definitional reflection as an alternative approach to standard proof-theoretic semantics. However, then I thought I should first elaborate the original Prawitz approach to validity, which then took the space allotted to my contribution[30]. The effect was that many considered the paper to be describing my deeply rooted proper opinion, and Prawitz was full of praise for it as presenting some of his ideas in a congenial way. I am no longer certain in what direction the 'most appropriate' approach to proof-theoretic semantics should go, and Prawitz is not absolutely certain about it either (see his contribution to this volume, Prawitz 2023), but in any case this is a paper making accessible a certain conception of proof-theoretic semantics in a thorough way. The special issue of *Synthese* probably paved the way for the term "proof-theoretic semantics". At least, it was roughly from this time on that the term took off and entered a great number of publications.

The second Tübingen conference on proof-theoretic semantics (C2016a), in March 2013, coincided with my 60th birthday. In addition to the conference, where again the big figures of the proof-theoretic semantics community were gathered, Reinhard Kahle and Thomas Piecha organized a birthday colloquium where David Pearce, Heinrich Herre, Walter Hoering, Gereon Wolters, Bartosz Więckowski, Ernst Zimmermann, Marie Duži, Pavel Materna and Gerhard Jäger spoke, all of them significant people in my intellectual development. This was a wonderful present. It also meant that we enjoyed two conference dinners, the second one being a birthday dinner hosted by me. The third Tübingen conference took place in spring 2019, one semester ahead of my retirement. It was the largest one with a lot of contributed papers and listening guests, reflecting that the subject has reached a mature state (C2019d).

In 2015, initiated by Luiz Carlos Pereira, Thomas Piecha and I organized a conference on *General Proof Theory* to celebrate the 50th anniversary of Dag Prawitz's book on natural deduction. Došen, Martin-Löf, Pereira, Schwichtenberg and Wansing, among others and in addition to Prawitz himself, all spoke. This was later published as a special issue of *Studia Logica* (C2019a). In 2001, Pereira, who had done his doctorate with Dag Prawitz around the time that Dag was acting as external referee for my doctorate, organized a conference in Rio de Janeiro celebrating Dag Prawitz's work, so the Tübingen conference was a kind of continuation of it. At the conference in Rio I presented my historical research on Hertz and on the first paper by Gentzen which was written in the spirit of Hertz's work (A2002b).

[29] As Grigori said at the dinner, it was the first time in his life that he toasted a government minister.

[30] The more inclusive and much longer first version of the paper is available as M2003b.

This conference made a lasting impression on me, not only because of the excellent presentations, but also because there was sufficient time both for socialising and for seeing the beautiful city. Besides the "standard" sightseeing places I remember well the wonderful reception by Oswaldo Chateaubriand in his house, and also the visit of Rocinha and the beautiful view we (Grigori Mints, Jan von Plato, Ernst Zimmermann and myself) had from the flat roof of a house up there.

13 Service and honours

My main service to the scientific community was my involvement in the Division of Logic, Methodology and Philosophy of Science (DLMPS, from 2015 Division of Logic, Methodology and Philosophy of Science and Technology, DLMPST). This association organizes its namesake congress every four years, dating back to 1960. I have been to several of these congresses. At the Salzburg 1983 congress I presented my first international conference paper (A1983a). For the 2011 congress, which was to take place in Nancy, the DLMPS executive committee (in particular Wilfrid Hodges, DLMPS president), supported by Gerhard Heinzmann (local organising chair) chose me as the general programme chair, which was of course a great honour. Preparing the congress was a lot of work — we could invite around 70 speakers, and accepted some 650 contributed papers (C2014, C2014–2015). The congress went quite well, in particular as the local organizers were so efficient. At the general assembly I was elected secretary general of DLMPS for the subsequent four years, and shortly afterwards I became in addition treasurer of the association, a job I kept for eight years. At the 2019 congress in Prague I retired from all DLMPST positions. All in all, this sort of activity was a great experience, but its more than ten years were enough to exhaust my administrative energy.

A great honour in my intellectual career was the doctorate *honoris causa* received from the University of Belgrade. The award ceremony took place in May 2016. It was very nice, in a beautiful Auditorium Maximum, with *Gaudeamus Igitur* being sung. The following weekend we celebrated Easter at Kosta Došen's home with some of his friends. For me it was the first year with Easter celebrated twice, this one being the orthodox feast, which was very late that year.

14 Retirement and outlook

As a professor in Germany, one can postpone one's retirement by three years beyond the retirement age, but only if the university agrees. This was not the case for me, as they needed my salary money otherwise (my pension comes from a different source). I just managed to obtain a half-year extension and retired on 1 October 2019. In November, I gave my retirement lecture on "Logic yesterday – today – tomorrow", to which plenty of former colleagues and friends came. Coincidentally, that very day

the UNESCO General Assembly installed World Logic Day, something I was happy to announce. Shortly afterwards I left for a half-year research stay at the Swedish Collegium for Advanced Study (SCAS) in Uppsala, where I had intended to work with Dag Prawitz on a joint monograph. Initially it was wonderful. I had perfect working conditions, very interesting and interested colleagues from a variety of fields; Gabi and I were provided with an excellent flat where we could even accomodate our children as visitors. Unfortunately, in March the Covid-19 pandemic struck and at the urgent recommendation of the German foreign office, we broke off our stay at the beginning of April, driving 20 hours non-stop with a rental car back to Stuttgart. Now, in spring 2023, we have had three years of pandemic, a much wider scientific interaction due to videoconferencing, and much time to think about what to do next (M2022d). There are very many loose ends in the topics mentioned above (Section 10), so plenty of work and also ideas to continue. There are also other topics to consider, such as the mathematical applications of Martin-Löf's ideas and results in the context of homotopy type theory (The Univalent Foundations Program, 2013). On the other hand, given the advances proof-theoretic semantics has already made, I doubt I can make much more than incremental progress.

Nevertheless, there are two book projects that I would like to finish. The first one is the monograph with Prawitz on general proof theory. We have been planning it since Prawitz brought up this issue when we met at a conference in Dubrovnik in May 2010 (the conference was on the philosophical nature of logical consequence). Acting as his co-author is a great honour for me, and I regret very much that progress has not been as fast as envisaged. The pandemic and other matters interrupted our work in Sweden. Completing it has first priority for me. The second is a monograph on proof-theoretic semantics. This I give more time, even though I wanted to write it already four decades ago, as mentioned in the first paragraph of this autobiography. In any case, it is to rely on the book with Prawitz. While Nissim Francez's monograph (2015) covers many topics including some applications in linguistic semantics, and certainly helps to establish the discipline, I still see so many open problems in proof-theoretic semantics, in particular on the conceptual side, such that for me there is no doubt that an advanced textbook covering both philosophical and technical aspects is a desideratum[31]. I very much hope that I will continue to have the intellectual strength and good health to pursue this goal. As far as the field of proof-theoretic semantics is concerned, it is on the right track. Even though my group is not being continued in Tübingen, there are plenty of groups continuing its topics, the most outstanding one in Germany being Heinrich Wansing's in Bochum. I very much hope that in the long run proof-theoretic semantics, which originated from logic, will prove itself in a great number of applications outside logic.

Acknowledgements I would like to thank Thomas Piecha and Kai Wehmeier for having taken on the great burden of editing this volume, and, of course, the authors for their contributions. I am also

[31] Originally, my SEP entry on proof-theoretic semantics (E2012c) was considered a first step in this direction, which was actually based on a much longer draft (available online: M2011c). Given the fast development of the field and changes in my own attitudes towards it, my original conception requires further thought.

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Most publications can be downloaded from Peter Schroeder-Heister's homepage.

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