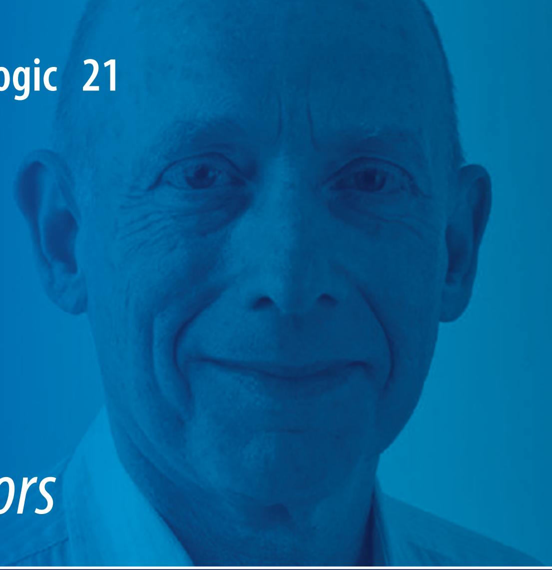
Outstanding Contributions to Logic 21



Ofer Arieli
Anna Zamansky *Editors*

Arnon Avron on Semantics and Proof Theory of Non-Classical Logics

Springer

Outstanding Contributions to Logic

Volume 21

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Preface

It is our honor and great pleasure to introduce this volume of *Outstanding Contributions to Logic*, honoring Arnon Avron's work on semantics and proof theory of non-classical logics.

Arnon Avron is a faculty member at the School of Computer Science, Tel Aviv University, since 1988. His research interests are very broad, spanning over proof theory, automated reasoning, non-classical logics, foundations of mathematics, and applications of logic in computer science and artificial intelligence. His foundational and pioneering contributions have been widely acknowledged and adopted by the scientific community. This was reflected in an international workshop celebrating his 60th birthday, held on November 2012 in Tel Aviv University, and followed by a special issue of *The Journal of Logic and Computation* (Volume 26, Number 1), published on February 2016.

This volume is another appreciation of Arnon Avron's seminal work over the years. It contains contributions of worldwide leading experts in semantic and proof-theoretical aspects of computer science logic. We are grateful to the authors for their positive response to our invitations as well as their cooperation in preparing inspiring papers in rather limited timeframes. Each submission has gone through a single-blinded peer-refereeing process by at least two reviewers. It is our pleasant duty to cordially thank all those who have acted as reviewers of the manuscripts submitted to this volume:

Leila Amgoud	Marcello D'Agostino	Edwin Mares
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We hope that this book will be useful for scholars who are interested in the foundations of non-classical logics. This is an outcome of an initiative by Heinrich Wansing, who kindly invited us to be this volume's editors. We would like to thank the series editor, Sven-Ove Hansson, for his valuable assistance during the preparation of this book. We also acknowledge with gratitude the financial support by the Israel Science Foundation (grant numbers 817/15 and 550/19).

Tel Aviv, Israel Ofer Arieli
Haifa, Israel Anna Zamansky
July 2020

An Uncertain Road to Certainty

Early Years: 1952–1970

The first error connected with my life was the date of my birth. I was planned to be born on the day of Lenin's Russian revolution that was supposed to be the beginning of an era of freedom and justice to all men and women all over the world. Unfortunately, I had disappointed my parents even before the revolution itself did so, and came to the world 3 days later, on November 10, 1952. I have been the youngest child in the family, having two sisters, 10 years and 5 years, respectively, older than me. In addition to being a boy, the first thing my parents noted about me after my birth was that unlike my sisters, I was a redhead—and being a redhead remained (and still is) one of my main characteristics, even though I lost most of my hair many years ago.

My parents have by far been the most dominant and influential persons in my life. My father worked at the small harbor of Tel Aviv (and has always been the leader of the workers there). This is why I grew up in the area of Tel Aviv that is still called "the harbor workers' neighborhood" (although there is no active harbor in Tel Aviv anymore, and all the harbor's workers that were living in that neighborhood are long dead). My mother was a housewife until I was about 9 years old. Then she learned librarianship, and not long after that she became the legendary librarian of one of the most prestigious (at least at that time) high schools in Tel Aviv. Objectively (and certainly from today's point of view), we were poor (at least until my elder sister left home, and my mother started to work and earn money too). However, we did not feel so, and my parents always found the money for what they considered as important. This did not include pocket money for us, but did include everything connected with culture, and especially books. They have never saved money on *that*, and so we had a huge and versatile collection of books. (Many of these books are still in my own private library.) Therefore, although neither of my parents had formal education beyond few years in high school in the countries in which they were born (my father at Belarus; my mother at Poland), their informal education was greater than most people I have met in my life. They were also very ideological, and my basic values and beliefs are still those that were installed in me by their education. (In the daily newsletter we were reading at home, immediately below its title, these values were summarized every day as follows: Zionism; socialism; comradeship among nations.) Another crucial feature that I think I got from my parents is the urge to fight for my values and for what I believe is right. This short scientific autobiography does not deal with those aspects and events in my life that are directly connected with those values and that urge. Still, it is worth noting that I think that they have indirectly influenced also my mathematical career and research, especially in seeking (and even willing to fight for) absoluteness: absolute certainty and absolute rigor.

My parents were very proud members of the working class. Nevertheless, they strongly did not want that their children will also be workers like them. Accordingly, when it came to us, learning and studying were by far their top priority. Not being good pupils was simply not an option for us. Luckily for my parents, we were all able to be *very* good pupils, and in my case—even an excellent one, the best in class in most theoretical topics. (In gymnastics, craft, etc. I was horrible...) Strangely, at the first half of my 8 years in primary school, I was not the best in class in mathematics. I liked the humanities much more, and my big dream as a child was to be a writer when I grow up. These tendencies still did not change at the last 2 years of the primary school, in which I unquestionably became the best in class in mathematics too.

The big change in my attitude to mathematics came in my first year (out of four) in high school. We had at that year a very good teacher, and with him we started to learn Euclidean geometry, with its theorems, proofs, and constructions. I simply fell in love with geometry then (and I still have a great interest in it). I was enchanted by the realization that interesting geometrical facts can actually be *proved* from some obviously true, simple axioms, and I found very great pleasure in solving difficult geometrical problems. Thus, I devoted the whole of the Passover vacation of that year (almost 3 weeks) to a problem that was given to us as a challenge by our teacher: to show that a triangle in which two angle bisectors are equal is necessarily isosceles. I still view my success in solving it at that time as one of the greatest mathematical achievements in my life. (The fact that my big competitor in class failed to solve that problem gave me extra satisfaction, of course. That competitor too is now a professor in Tel Aviv University, but in economics.) The love of geometry made me interested also in other branches of mathematics. So during high school I started to read books in mathematics whose content was well beyond what we were learning in class. I was not able to understand at that time everything I read, but I understood enough to become even more fascinated with mathematics. It became my favorite subject, and so I decided quite early that it would be what I would study at the university.

University and Army Years: 1970–1978

The title of this section might be confusing, because almost all my years since 1970 (when I finished high school and started to learn mathematics at Tel Aviv university) can be described as "university years". But what I mean by my "university years" is the 5 years in which I was only a student of mathematics, without any teaching tasks.

Here I should explain, first of all, that most Israelis do not go to university or college at the age of 18. They go to the army for 3 years instead (if they are male; female serve less.) However, I was accepted by the army to a special program called "academic reserve". In other words, I was a student with call-up deferred until finishing B.Sc., and in my case (since I got it with distinction)—even until finishing M.Sc. That meant that I spent in a boot camp a great part of the summer vacation between my first and second years as a student. Otherwise I was living the usual life of an Israeli male student at the years before and after the 1973 war. (This includes being called from time to time for a short period of army service.)

As a student, I discovered rather quickly that I am unable to follow the teachers in class. When I was still trying to understand what is written on one side of the blackboard, the teacher has already been writing on the other side. So I usually gave up going to lectures, and instead learned from books, and from lecture notes taken by students I knew. I was very successful at that, and so I got the highest possible grade in almost all the courses I took. However, already at that stage there began to be some gaps between the knowledge I showed at the examinations, and the feeling of "cheating" that I really felt about some of the proofs we were learning. A few years later I understood that all the proofs that I had found suspicious either include implicit applications of the axiom of choice, or introduce sets by using impredicative definitions. I should emphasize that nobody has told me at that time that there might be something problematic about such proofs. I also knew then nothing about the historical debates concerning them. I simply felt uneasy about them, but said (at that time) nothing about it to anybody. The result was that I began to be more and more interested in logic and foundations. However, at my first 2 years as a student of mathematics I knew nobody I could ask about these topics. Luckily for me, this state changed at my third (and last) year as a B.Sc. student. At that year a professional logician joined the department of mathematics of Tel Aviv University for the first time: Yoram Hirshfeld. So at that year I had the opportunity to take courses and seminars by Yoram on mathematical logic, set theory, computability, and model theory.

Yoram was a good teacher, and he was also open to talk about things. His courses and our private discussions made it clear to me that logic and foundations are going to be my mathematical subjects. The unavoidable conclusion was that I should do my M.Sc. thesis at the Hebrew University at Jerusalem, whose department of mathematics had then (as I learned from Yoram) one of the strongest group of logicians in the world. It included Michael Rabin, Azriel Levy, Saharon Shelah, and Haim Gaifman. (M. Megidor came a few years later.) In my first year as an M.Sc. student, I took courses and had conversations with all of them, and saw that of them Gaifman was the most philosophically inclined, and is the one to whom I was closer in spirit and interest. So in my second year at Jerusalem I did my M.Sc. thesis under him. The subject of that thesis (which has never been published) was progressions of arithmetical theories. It was strongly connected with Feferman's famous work on this subject, and this was the first time I heard Feferman's name and studied two of his classical papers.

Although I was an M.Sc. student in Jerusalem, I still spent most of my time at that period in Tel Aviv. (In Jerusalem I spent at most 2 days, including one night, each week.) I had good reasons for that: studying mathematics and working on my M.Sc. thesis occupied only a part of my time at that two crucial years in my life. Thus, I was working as a teaching assistant (who checks assignments of students) in both Tel Aviv and Jerusalem; I gave a lot of private lessons, and I was very involved in political activity (as a leftist, of course). However, what was most important of all at those 2 years were the time and energy which I devoted to what is in the center of life of most young men at their early 20s. In my case, this type of activity had ended already before I finished my M.Sc. thesis: At the beginning of June 1975 I married my wife, Tsipi, whom I met for the first time about 2 years before. I was 22.5 years old then, and she was (still is...) 2 years younger than me. In the 5 months that followed our marriage I finished (with great hurry) my M.Sc. thesis; we bought (with the help of our parents, of course) an apartment in Petah Tiqva (a town near Tel Aviv), and we moved there a month before I started my full 3 years of military service.

Despite having an M.Sc. in mathematics when I joined it, my abilities and knowledge in mathematics were not used by the army. This was in sharp contrast to what happened with most of those who were in the "academic reserve" with me. The reason was almost certainly my political activity at my university days, together with the political background of my parents. As a result, I was just waiting for my service to end, hating almost every moment of it (even though I was not a combat soldier either). However, I did find enough free time at that period to expand my knowledge in logic, and in particular to learn that branch of it that I had never learned at the university: Proof theory. In addition, I started also to study Philosophy on my own, since I reached at that time the conclusion (which is now even more valid than it was then) that adequate philosophy is the most important thing for humanity in the crazy times in which we live.

Two very important events in my life took place at my last year at the army. My father died at the beginning of that year. About 8 months later my first child, my son Haim (which is named after him, and is now a Professor of Mathematics at Tel Aviv University himself) was born. This happened exactly 1 month before my return to civil life. I was exactly 26 years old then.

Ph.D. Student 1978–1984

If you wonder why it took me 6 years to finish my Ph.D., the answer is that only before the end of those 6 years the head of the school of mathematics called me to tell me that if I do not submit my thesis within 4 months, I would not be able to work as an instructor in the school anymore. So I had no choice, but to sit and write down all the research I did over those years (which was sufficient for two Ph.D. theses)—and then to arrange for it to be typed. (There was still no LaTeX then, and I had to change four different typists before the work was done!) Anyway, I managed to do it in time.

But had nobody given me a strict deadline, I would probably have never finished my Ph.D... The trouble was that in addition to being a Ph.D. student, I was occupied with many other tasks. I was a father of two small children, for whom I was responsible in most of the afternoons (my daughter Noa was born at 1982); I was teaching in several places (in addition to the university), and I gave many private lessons too. I had no choice: we moved at that period to Tel Aviv, to the vicinity of the university, and our apartment there cost twice as much as our apartment in Petah Tiqva. So we had to take several loans, in addition to the mortgage on our new apartment. Another problem was that it was much easier and tempting for me to make progress in my research than to write down what I had already done in a form which is suitable for publication (a boring task).

The name of my thesis has been: "The semantics and proof theory of relevance logics and nontrivial theories containing contradictions". As this name suggests, its area was relevance logics, and more generally: paraconsistent logics. How did I arrive at this subject? Well, after the army I returned to Prof. Gaifman, and he agreed to serve again as my supervisor. (But since he was at Jerusalem University, while I myself was already strongly connected at that time with the School of Mathematics of Tel Aviv University, I needed to have a supervisor from Tel Aviv too, and Yoram Hirshfeld agreed to be the one.) I told Gaifman that I am interested in Philosophy, and I would be happy if my thesis in mathematics will be connected with philosophical problems. He, in turn, suggested two topics for my thesis. One of them was a concrete mathematical problem which was open then, and is connected with Gödel's incompleteness theorems: to extend to the first-order level Solovey's theorem about the completeness of the propositional provability modal logic **GL** for its arithmetical interpretation. The other subject was completely different in nature: to provide good explanations and model(s) for the fact that in both mathematics and physics, there have been useful inconsistent theories, even though such theories are, in principle, logically trivial (from a classical, and even intuitionistic, point of view). At the end, my thesis was devoted to the second topic.

The truth is that at the beginning, the first subject suggested by Gaifman was more appealing to me. Therefore, it was the one to which my main efforts then were devoted to. I even had some plan how to attack the problem. Its first step was to find a cut-free Gentzen-type system for **GL**. (Already then I was very fond of Gentzen-type systems, a topic I learned by myself at my years in the army, according to the advice given to me by Gaifman.) The next step was to show cut-elimination for QGL, the natural extension of that system to the first-order level. Then I hoped to show directly that the arithmetical interpretation obeys the same reduction steps as QGL. I succeeded (or so I thought) to implement a part of this plan. First, I completed the first two steps. This involved a complicated proof by triple induction of cut-elimination for QGL. I succeeded also to show the arithmetical validity of some of its reduction steps. However, I was completely stuck with others. Then a worse thing happened: I did the horrible mistake of checking again and again the correctness of my proof of cut-elimination, to make sure that I missed no possible case. And sure enough, I did discover a case in which something was going wrong. I tried to overcome the problem, but could not. Then came what I thought to be the ultimate disaster: by pursuing the small subcase in which my proof went wrong, I finally arrived at an example of a sequent which is derivable in QGL, but has there no cut-free proof. Not only the proof was wrong—so was the theorem itself!

Luckily, by that time I had also some ideas concerning the other subject suggested to me by Gaifman. So I decided to leave for a while the first subject, and to turn to the other one. And since I began to make a real progress in it, I had no motivation to go back to the difficult problem I had left unsolved. However, there was to be one more chapter in this story. Three years later, I looked at a new issue of the journal of symbolic logic, and found there ... a paper which presents "my" QGL, including exactly my faulty proof of cut-elimination for it! I was shocked. Until that moment in my life (and I was 30 years old then) I did not believe that it is possible that a respectable journal like JSL may publish a paper with wrong theorems and mistaken proofs! I told Yoram the story, and he advised me to submit to JSL a paper about this. So I took from my drawer the stuff that lied there 3 years, turned it into a paper, and submitted that paper to JSL. It was very quickly accepted, and then was one of my two first published papers [2].[1] (The other one was published at the same year, 1984, and at the same journal, but it already was in relevance logic.) That paper actually included some good results and proofs. Thus, it includes correct (but semantic) proofs of cut-elimination for Gentzen-type versions of the provability logics **GL** and **Grz**. (It turned out that the one for **GL** had been known before, but that for **Grz** was new.) It also contained some nice applications of the arithmetical fixed point theorem. However, what turned out to be its most significant contribution was the simplest (and least appreciated by me): the demonstration that QGL does not admit cut-elimination. Several years later Sergei Artemov told me that reading my paper was a turning point in his research on the subject, since this has been the first *negative* result concerning it. Therefore, it gave the first hint that Solovey's results in the propositional case fail in the first-order one. When I told him the above story he said that I would have saved him a lot of time and efforts had I published it before...

I devoted above a relatively big portion of my academic biography to the story of QGL. The reason is that I believe that one can learn a lot from it. I personally certainly did. Nevertheless, what unfortunately I could not internalize is what some might take as its main practical moral: do not check your proofs—publish them quickly instead. Checking is at best a waste of time, and you might even lose papers because of such a dangerous activity!

While still working mainly on provability logics, I started to think also on the other problem. Again my starting point was the use of Gentzen-type systems. I introduced three such systems, inducing three different logics, and proved (using Gentzen's syntactic method) cut-elimination for them. All my systems were based on the idea of weakening the structural rule of weakening. My real first achievement was proving (weak)[2] completeness of one of them relative to a certain three-valued logic which I thought had never been investigated before. I was wrong, of course. A

[1] From now on the numbers in square brackets refer to the articles in my list of publications.

[2] At that time, I was not aware yet of the importance of consequence relations, and the difference between strong and weak completeness.

few months later I discovered, to my dismay, that it was introduced and axiomatized by Sobociński in the year I was born. Unfortunately, this happened to me again and again during my academic career. For almost every new interesting idea of mine, it turned out that someone, somewhere, has thought about it before...

In parallel to my independent thinking about the problem of inconsistent theories, I started also to read the relevant literature. The only pointer that Gaifman was able to give me was the classical paper of Anderson and Belnap on Entailment. Soon I discovered that meanwhile they had published a big book, **Entailment**. (Volume 1; the second volume appears almost 20 years later.) Almost everything in this book was completely new to me. Therefore, I studied it extensively. and I was frustrated to find there all "my" three logics. For one of them, which was $\mathbf{R}_{\overset{\sim}{\to}}$ (the implication-negation fragment of the famous relevant logic $\mathbf{R}$), the book presented even $G\mathbf{R}_{\overset{\sim}{\to}}$—which was identical to one of "my" Gentzen-type systems. To the other two logics it presents only equivalent Hilbert-type systems. In particular, there was there a Hilbert-type counterpart of the logic I preferred most (and later called $\mathbf{RMI}_{\overset{\sim}{\to}}$). Like $G\mathbf{R}_{\overset{\sim}{\to}}$, my Gentzen-type system for it was obtained from the classical one by deleting the weakening rule, but unlike in $G\mathbf{R}_{\overset{\sim}{\to}}$ the two sides of a sequent consist in it of *sets* of formulas rather than multisets or sequences. (I found that, and I still do, much more natural.) In addition, I had a rather natural idea of possible semantics for $\mathbf{RMI}_{\overset{\sim}{\to}}$, based on using "relevance domains". I also had a nice conjecture that a particular instance of the general semantics, in the form of a simple infinite-valued matrix, is characteristic for $\mathbf{RMI}_{\overset{\sim}{\to}}$. I was, therefore, particularly disappointed at first to find this logic too in **Entailment**, even though very little information was given there about it. Luckily, Gaifman saw things differently. He told me that it is actually *good* for me that "my" logic had been introduced by others before. He also told me that if I succeeded to prove my conjecture, I would be able to publish that in the JSL. So I made an effort, and did manage to prove it. Then, exactly as Gaifman had predicted, this result, together with related ones, was accepted to the JSL [1]. It was my very first published paper.

By that time I had completely abandoned provability logics, and concentrated on relevance logics and paraconsistent logics. In the following years, I developed my own theory of relevance, and I collected more and more results. However, during my time as a Ph.D. student I published only a very small part of them. The main body of my thesis was finally published in three parts [12–14] only several years later. The main difference between my approach there and that of the main relevantists' school was that my systems were purely relevant, and any attempt to add, e.g., an extensional conjunction $\wedge$ to them (for which $\varphi \wedge \psi \to \varphi$, $\varphi \wedge \psi \to \psi$, and the adjunction rule are all valid) means losing the variable-sharing property. It seems to me that this was one of the main reasons that this big work of mine was almost totally ignored, despite my effort in [21] to get the community's attention to it. However, the time and efforts I devoted to my thesis had their merit. First, during those years I became a real expert about Logic and logics. Second, my thesis contains one (almost) new idea that did become rather well-known and popular: the use of *hypersequents*.[3] This was due to the fact that I could not find a cut-free Gentzen-type system for my main logic, **RMI**. My supervisor, Gaifman, asked me if this really matters to me. (Being more a model-theorist, this did not look so important to *him*.) My answer was positive, since already then my view was that a useful logic should have both an analytic proof system and an effective semantics. **RMI** did have the latter, but I needed to introduce hypersequents in order to be able to provide a decent proof system for it. I proved cut-elimination for my hypersequential formulation of **RMI** using an extremely complicated syntactic proof. This proof was later published in [14]. However, I had published before that a somewhat easier (but still very complex) versions of the calculus and of my cut-elimination proof for it in the case of the simpler, and better known, semi-relevant system **RM**. A few years later I found cut-free hypersequential calculus for Gödel–Dummett logic **G** (also known as **LC**), in which I introduced the "communication" rule. The latter, and the use of hypersequents in general, have become since then the basis of the proof theory of all fuzzy logics. As a result, my name started to be known among logicians. Well, I always took it as ironical that what was of a secondary importance in my thesis, done only because of my insistence, is the only part of my thesis that has given me some fame...

Post-Doc 1984–1988

Tel Aviv 1984–1986

I submitted my thesis at the end of 1984, but it was finally approved (with distinction) near the end of 1985. Meanwhile, I was forced to think at last about the problem: "What do I want to do when I grow up?". My wish was, of course, to get a position in mathematics in one of the few universities in Israel. (There were only five then.) Unfortunately, there was then very little hope for that to happen. There were at that time almost no jobs in mathematics at the Israeli universities, especially for logicians, and certainly not to someone like me, who had done his thesis on what was then a particularly esoteric subject. So how did I become a professor at Tel Aviv University nevertheless? The simple answer is that I have been lucky.

A few years before I finished my Ph.D., our school of mathematics had decided to establish a new department: computer science. (Already this decision was a part of my luck.) In its first years it was very small, and I even have not heard about it.

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[3] As usual, it turned out that similar structures had been used before. Thus, the referee of my first paper on hypersequents, [6], pointed out that Pottinger had used what I called "hypersequents" in a small abstract concerning modal logics. Years later I was shown that Mints too had used a similar structure in his proof systems for modal logics. Strangely, although I discussed with Mints my use of hypersequents for **G** when I was on sabbatical at Stanford, he has never mentioned to me this fact! Anyway, it was certainly me who first developed the general theory of hypersequents, applied it for various families of logics, and made it known. (Even the name "hypersequent" is due to me.)

However, the wish to make it bigger and important led to my first big luck. In 1981, B. Trakhtenbrot immigrated from the USSR to Israel, and joined the new department of computer science in our school. He was then a very famous logician and computer scientist, but I myself had not heard about him before he came, and knew very little about him also after that. Therefore, it did not even occur to me to make contact with him. Instead, he made contact with me, since he was looking for people to work with, and someone had told him that I am one of the few people in the whole school who have some knowledge in logic. My thesis was not ready yet then, and I was used to work in isolation from others, and on what was of interest for me. So at first I viewed my connections with Trakhtenbrot (that he practically forced on me) as a headache—especially that he was very demanding, and assigned me tasks that were of no interest for me at that time. The first such task was to read and then explain to him Martin-Löf's type theory and its purported relations to computer science, as described in his famous 1982 paper: "Constructive mathematics and computer programming". (Funny, but the fact is that my acquaintance with computer science started with this paper...) I did not want to do that, but I had no choice. So I did it the best way I could, and Trakhtenbrot was very impressed. He decided that I have a great potential—and in few years I found out how crucial for my future was this good impression I made on whom I viewed then as an old, imperious person! There is no question that I owe him my career. More than that: after finishing my thesis, he practically became my mentor for many years, and once I began to really appreciate the knowledge, insights, and vision of this great scientist, I also learned from him a lot.

Stupid as I was, after submitting my thesis it did not take me too long to realize that turning to computer science was my only chance and hope. So I started to try to learn it. As a part of this attempt to become a computer scientist, I gave an advance course on automated theorem proving (which I practically learned and taught in parallel). At the end of that course I was looking for problems to include at the final home examination of the course. By chance, a friend gave me at about the same time the following problem as a challenge: Given two points in the plane, construct by means of a compass alone the corresponding midpoint. I solved it easily using a bottom-up search. Then it occurred to me that asking how to attack it using a computer would be a good problem for the home examination. However, before giving it to my students, I wanted to check whether I can do it myself. This was also an opportunity to do some programming for the first time after 15 years. (I did have to write some programs in FORTRAN at my first years at the university.) So I learned PROLOG, which was very fashionable at that time, because of the Japanese fifth-generation computer systems project. Then I wrote a program in PROLOG that could indeed solve the above problem, but was able also to find many other points that can be constructed by means of a compass alone, given two points in the plane. An inspection of the output of my program led me to a conjecture about what points it can produce. So I tried to prove that conjecture, but instead I proved that it is wrong. (However, the computer itself gave in to my result only after outputting several thousands points that confirmed my wrong conjecture...) Next I noticed that my proof of what my program can do can be turned without too many difficulties into a simple proof of a *strengthening* of the classical Mohr–Mascheroni theorem about constructions by means of a compass alone (which I had heard about by that time). So I wrote my proof down, and submitted it to the Journal of Geometry, where it was published [5]. However, the referee has some reservation about my proof, since in addition to proper intersection points, it allowed also the use of intersection points of two *tangent* circles. (This was something that both Mohr and Mascheroni avoided.) Nevertheless, I was able to remedy this problematic feature of my proof with the help of a construction given to me by the program, and this was again published at the Journal of Geometry [11]. I was, and still am, very proud of these two short papers. I fulfilled in them a big dream that I had had when I was in high school: to prove an interesting new theorem in Euclidean Geometry that anybody can understand. In addition, this experience showed me for the first time (but not the last) how fruitful for my research can teaching a course be.[4]

While I was devoting most of my working time at that period to standard academic activity (new research; turning parts of my thesis into papers; teaching; learning new subjects), my main goal then was to find a post-doc position in a good place abroad—something that was a necessary condition for getting a permanent position at a university in Israel. This was a rather frustrating task, involving getting one negative answer after another, even from places that had shown at first some interest. However, at a certain point my luck (together with the great help of Trakhtenbrot), found a place for me that proved to be really great: the Department of Computer Science of Edinburgh University. That department had already been one of the best in the world, with giants like Robin Milner, Gordon Plotkin, and Rod Burstall. Luckily for me, at that time they decided to find a new internal research institute called LFCS (Laboratory for Foundations of Computer Science). Moreover, one of the first two big projects that were planned for the new LFCS was the construction of the first *Logical Framework* (LF): a general computerized system for implementing a variety of logical systems of all sorts. Therefore, the department was looking for post-docs who might be able to contribute to this big project, and Trakhtenbrot convinced them that I am a good candidate. At that academic year (1985–86), I also won the Rothschild Fellowship for the following year (which I failed to get the year before, because my thesis was not approved yet). This meant that I would not have financial problems during my 2 years as a post-doc at Edinburgh. At last, things fell in place for me!

Edinburgh 1986–1988

I truly fell in love in Edinburgh during the 2 years I spent there. I believe that it is the most beautiful town in the world. I also like its atmosphere. (Especially in the summer; the winter is somewhat problematic, because you almost never see the sun.) Therefore, I am always happy to return to Edinburgh for a visit. However, my

[4] The full story, together with an analysis, can be found in [24].

first months there were not easy at all. I was older than all the other postdoctoral researchers there, and while almost all of them were not even married, I had two children: a boy at the age of 8 and a daughter at the age of 4.5. Neither of my children knew a word in English when we came to Edinburgh, so taking care of them was a major (and uneasy) task for me at those first months. In addition, it took me time to find myself in this new environment. I was unable at first to follow what the people there were talking about; I have difficulties in communicating with them, and I was still desperate to understand what is computer science.[5] Luckily for me, the atmosphere in the LFCS was rather relaxed and tolerant, unlike the pressure I heard about from friends that went to the USA. This fact was of great help for me. There was also a *person* who helped me a lot at those difficult days. His name was Furio Honsell. He was an Italian who started his post-doc in Edinburgh a few months before me.[6] Like me, Furio belonged to the LF group, and at the beginning he was the only one in the department with whom I was able to communicate. Thanks to him, I started to understand the ideas that underlie the planed design of the LF. On the other hand, he has learned from me too. In fact, at a certain point I realized that although logic has a central place in the research made at the LFCS, most (if not all) of the people there had a rather limited knowledge and narrow view about it. So I wrote for Furio by hand some pages of remarks about logics and logical systems that I thought every logician know (or should know). Furio became very enthusiastic about those remarks. He started to spread their content, and the ideas presented there had great effect on the theoretical development of the LF, as well as its practical use for implementing logical systems. Furio then encouraged me to turn my notes into a paper. I did so several months later. The resulting paper, "Simple Consequence Relations", was first published in the form of a technical report of the LFCS about a year after I came to Edinburgh. Already in this form it became rather popular, and one of my most successful papers.[7]

My notes to Furio were one reason for the change in my status in the LFCS from the sort of an outsider that I was in my first months there, to a respected member, whose knowledge in logic was much appreciated. The other reason was due again to a great piece of luck. A few months after I had joined LFCS, another member of the LF group, Bob Harper,[8] returned from a visit in Paris, and brought with him a preprint of a new big paper of J.Y. Girard. Bob told us that it was what everyone was talking about at that time at the University of Paris, although he himself could not

[5] I frequently say, as a half joke, that I finally understood what is computer science only when I became an editor of "Theoretical Computer Science", because from that point something belongs to computer science if I decide so...

[6] Later Furio became the Rector of the University of Udine, and then he served many years as the Mayor of Udine. He still divides his time between research and politics.

[7] The paper was finally published 4 years later in "Information and Computation" [15]. The truth is that this was not the most appropriate place for it. I chose it only because it was crucial for me at that time to have papers in respectable journals of *computer science*.

[8] Bob came to Edinburgh a year before me, and by the time I came he has already been a well-established member of the LFCS. At the time of writing, he is a well-known professor of computer science at Carnegie Mellon University.

really understand what is written there. The name of the paper was “Linear Logic”. Naturally, everyone in the LFCS wanted to understand what it is about—and I was the first one to succeed. The reason was simple: I soon realized that except for the notations, linear logic was a very close relative of relevance logics. Accordingly, there was nothing mysterious for *me* at Girard’s paper, and so I volunteered to read it and to lecture about its content. The three lectures I gave on it made great impression on everyone, including Gordon, Robin, and Rod. Therefore, they were the turning point of my time in Edinburgh—and this was only the beginning of the great positive effect that Girard’s linear logic had on my career. Thus, I could rather easily adapt to linear logic some of the easier things that I had done in my thesis. The result was submitted to TCS, not long after Girard’s original paper had been published there. It was quickly accepted and published there too ([7]). This was my very first paper in a computer science journal, and also the second published paper on linear logic. (The first was Girard’s paper itself, of course.) Since there was a huge interest in linear logic at that time, this paper of mine really helped to make my name known. Ironically, its most important contribution for many was a table I included in it of translations from Girard’s notations to those which had been used in the relevance logic literature. That table was called by some relevantists “the Rosetta stone” that let them understand the very fast growing literature on linear logic, a subject that was very hot at the end of the 80s and at the 90s. Consequently, my name became known also in the community of philosophical logic.

I have always found as ironical that the two papers that gave me some fame at the first stages of my academic career were papers that I myself did not appreciate much, since they did not have real mathematical depth, and their popularity was mainly due to ignorance of people about issues that I believed should have been well known. But I was not complaining, of course. Thanks to these two papers, most of my time in Edinburgh was rather nice and fruitful. I had other works there, some of them more important in the long run than the two mentioned above (like [16] and [17]). I even had there my very first joint paper with other people: the second big paper on the LF, written by Furio, Ian Mason, and me [22].[9] This was significant, since until then I had worked in complete isolation. (Even with Gaifman, my supervisor, I met very rarely, and there were years in which we did not meet at all, since he was abroad.) Even more important was the fact that I met and talked with many scholars. First, the LFCS has a very international atmosphere. There were people there from Sweden, Denmark, Poland, Italy, Germany, China, Japan, India, England, Canada, USA, Australia—and even Scotland! Some of them became good friends of mine. Second, during my 2 years at Edinburgh I took part in several scientific meetings, and visited several countries. This was almost a new experience for me. Before coming to Edinburgh I had participated just in three international meetings, and the first of them (which was also my very first trip abroad after high school) took part only when I was 31 years old. (It was the big 7th congress on logic, methodology, and philosophy of science in Salzburg, 1983.) Things completely changed during my

[9] [9] was an earlier, shorter version of it, while R. Polack’s contribution was added later.

2 years at Edinburgh, and only there I started to be (and to feel as) a part of an international scientific community.

In my second year at beautiful Scotland, there was only one thing that prevented me from fully enjoying my stay there: the worry about my future, and the great uncertainty whether I would find a position in Israel. This uncertainty remained in force most of the year. But at the end fortune again smiled at me. The computer science department of Tel Aviv University has only six faculty members when I left to Edinburgh, but it had 14 (including me) when I returned. My great luck was that the university decided exactly at that time to really expand it, and the year in which I applied was the main one in which this decision was implemented. I was one of five new faculty members that joined it at the end of 1988—the biggest expansion in the history of our debarment until present, and practically the last time in which someone whose Ph.D. had not been in computer science was given a tenure-track position in it. Even so, I would not have been one of the fortunate five had not Trakhtenbrot strongly fought for me. Without him, even the good letters that were sent on my behalf by Gordon, Girard, and others would not have helped.

Climbing the Academic Ladder: 1988–1999

As my story so far showed, it might be necessary to be lucky in order to get a position in a place like the School of Mathematics at Tel Aviv University. (I am not saying, of course, that it can be *only* a matter of luck.) However, from the point in which one gets the *chance* to have a place there, by being given a tenure-track position, luck has nothing to do with the rest, i.e., actually getting tenure and then being promoted. It completely depends on what one does. I knew that, and I worked hard during the period of 10–11 years that followed my return in order to successfully pass through all the stages of an academic career in Israel, until I became a full professor at 1999. This time I was not lucky at all, since in my case the process consists of no less than four different stages, with all the agony and the complicated process that each such stage involves. How come? Well, like all those who joined the computer science department at those years, I started as a lecturer. (This position practically does not exist any more in mathematics or computer science; now every new comer immediately starts as a senior lecturer.) After 1 year, I was promoted to a senior lecturer.[10] In 1992 I got the tenure, in 1995 I became an associate professor, and since 1999 I am a full professor.[11]

[10] This was an initiative of Trakhtenbrot, and it was a mistake: it was better to wait and get this promotion together with the tenure.

[11] Nowadays, there are just two stages: getting tenure together with a promotion to associate professor, and becoming a full professor.

Getting Tenure: 1988–1992

My main worry after my return was still to secure my future by getting tenure. It was not at all guaranteed, and I even had to work for that harder than others, because my being a real computer scientist was somewhat suspicious. (On the other hand, when it came to promotions, it helped my cause that we were then still a part of the school of mathematics, and so were judged by its standards.) At the first 4 years, my main research activity was devoted mainly to continuing, finishing, and writing down works that I started before returning. However, I was trying to look at new subjects as well. One of them was concurrency, because this was a subject in which Trakhtenbrot was very interested at that time. Just one paper directly came out of this [28], but learning it gave me ideas how one might use calculi of hypersequents for modeling parallel computations or processes. I have never managed to seriously pursue those ideas myself, but recently this was successfully done by others.[12] A particularly fruitful source of ideas and research was my teaching courses in computer science, since this involved a lot of thinking on the topics of the courses. Thus, [23] was a direct product of teaching automated reasoning, while [19] and its full version [26] were inspired by my course on databases.[13] The reason that it was me who taught databases at those years was that I had heard in Edinburgh that its theory has connections with logic, and there was no real expert then on it in our department. (Now there are two.) At the first year after my return, I both learned the theory of databases and taught it. After that I started also to look at interesting research topics that I had found in the material on databases that I was teaching. The abovementioned work with Yoram was the first result. However, the really great outcome of my teaching databases for 4 years came several years later. It will be described in the sequel.[14]

Another academic activity that I continued, of course, at that time was presenting my work in meetings abroad, and getting to know more people there. The trips that I made in those 4 years were very important for my career. However, there was among them one that proved to be *particularly* important: On April 1991, I returned to Edinburgh for the first time (out of many) in order to participate in a conference there, and my wife Tsipi joined me on that trip. Exactly 9 months later my third and youngest child, Uri, was born (about 10 years after his sister and 13 years after his brother). This has by far been the most productive academic trip I have ever made!

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[12] F. Aschieri, A. Ciabattoni, and F. A. Genco; A. Beckman and N. Preining.

[13] It should be noted that this was the first, and so far the only, time I had a paper with one of my three mentors; I have never had a joint paper with either Gaifman or Trakhtenbrot.

[14] I should admit that since I left to my course assistant the responsibility for the practical project that the students of my course had to do, I have never used a database system myself. Well, I have also taught my children how to ride a bicycle, even though I cannot do it myself...

At Stanford: 1992–1993

As I wrote at the beginning, I chose mathematical logic as my area because of my deep interest in foundations of mathematics and in philosophy of mathematics. Unfortunately, for almost 17 years after finishing my M.Sc. thesis, I did not have time to do research on these related subjects. However, I did find time during those years to think and to read about them. Thus, already when I was a Ph.D. student I learned about platonism, formalism, logicism, and intuitionism. Surprisingly for me, my own views about the foundations and nature of mathematics did not fit to any of these major schools. Then, at a certain point in my conversations on foundations with Prof. Jonathan Stavi, he told me that views like mine are known as *predicativism*, and suggested that I read Feferman's papers about it. That is how I have come to realize that I am a predicativist.[15] Needless to say, after that I returned to study Feferman's papers. Since my M.Sc. thesis had been based on his first two main papers, it was as if I am simply continuing to the next ones. Anyway, Sol Feferman became my academic hero, so I strongly wished to have the opportunity to work with him. Our first meeting took place in the congress at Salzburg in 1983. I simply came to him and introduced myself as a student of Gaifman, and told him that I am very interested in his work. Our conversation was rather short. Therefore, I was surprised that he remembered it when in 1990 I wrote him, and asked to spend some weeks of that summer in Stanford. He agreed, and arranged the financial side of my 6 weeks visit. That visit was the beginning of my connections with Sol. It turned out to be also an opportunity to renew my connections with the LF group: There was a week in which Gordon, Furio, Ian, and me were all there. (Feferman too was at that time interested in logical frameworks, so some of our conversations were on that subject.)

At 1992, I knew that I am about to get tenure at last, and so I decided that it is a good time to take sabbatical abroad. Stanford was, of course, the place I wanted, but at first it seemed impossible to go there, because of the strong recession that USA was experiencing at that time. Luckily, Feferman was then the chair of Stanford's Department of Mathematics, and at the last moment, when I was about to go elsewhere, he found a possibility to support my visit. So again I went abroad with my family (which now included also a baby). This time it was for just 1 year (1992–3), and to California instead of Scotland. Another difference was that not a long time after we came to Stanford, I officially got my tenure. Therefore, I was free from worrying about my future for the first time in my life (at the age of 40). This fact allowed me to start devoting a part of my research to foundations of mathematics. My meetings with Feferman during that year helped me to put my views on that matter in context, and to understand the related research that had been made up to that point. As usual with me, no joint paper came out of our discussions. Still, I was able to surprise Feferman by showing that a certain significant improvement of his own suggested logical framework, which he had believed to be impossible, is possible

[15] Laura Crosilla once asked me how I became a predicativist. My answer was that it seems that I was born one, and that I only can tell when and how I *discovered* that I am a predicativist.

after all. This improvement was strongly connected with my first foundational idea and subject of research: the use of **AL** (ancestral logic, also known as transitive closure logic) as the basic logic that underlies absolutely certain mathematics. (For me the latter is identical with predicative mathematics.) My study of **AL** actually started at that year in Stanford, even though my first paper on this logic [56] was published only 10 years later.

There were two other important (from my scientific point of view) developments that took place at that year in Stanford. One was my first meeting with Mike Dunn, with whom I only had had some correspondence before that. Mike invited me to visit Indiana University at Bloomington, and to give a lecture there. This has been a very nice visit, and it included several very useful discussions with Mike and with other people there.[16] The other development was again a meeting. This time it was with a young fellow I had not heard about before. His name was Richard Zach, and now he is a famous Professor of Philosophy at the University of Calgary. At that time, he was still only a research student in TU Wien. He came to a short visit at Stanford near the end of my year there, and during that visit he initiated a meeting with me. I could not guess it then, but that meeting was the beginning of my close relations and friendship with the great group of logic in TU Wien and with many people that have spent time there over the years. (Including, of course, some which are still there.)

Becoming a Full Professor: 1993–1999

The next stage in my academic career came relatively quickly. Two years after returning from Stanford I became an associate professor. There was a price to pay for that, though. Not long later, between 1996 and 1998, I had to serve as the chair of the CS department. The reason was that there were very few professors (either full or associate) in our department at that time, and each of the others had already done this job. Therefore, I had no choice but to agree. Being the chair demanded a great part of my time, and was a very hard test for my nerves. Nevertheless, at the end, both I and my department somehow survived (not without great difficulties) those 2 years. Happily, this was the last time I had to take on myself such a big administrative task.

As for my research activity, a part of it continued to be devoted to subjects I had worked on before, like substructural logics, including relevance logics and linear logic ([35, 40]). However, the main subject I was working on at that period was bilattices, and their use for uncertainty reasoning. Originally, bilattices had been introduced by Ginsberg, but I learned about them from Mel Fitting at a conference in Varna in 1990. After that I read Fitting's papers on this subject, and became interested in it. Therefore, I soon began to be engaged with research on it myself. Most of this

[16] One side effect of that visit was that I started to use the name "Dunn–Belnap logic" for the famous four-valued logic that everyone, including myself, had called "Belnap's four-valued logic" until then. Slowly but surely, this more correct name has been adopted by others too. I am really glad that I have helped in giving my friend Mike the credit he deserves here!

research was done after returning from Stanford. One of its main achievements, with which I was particularly pleased, was to be the first to prove a conjecture of Fitting about the structure of interlaced bilattices ([33]).[17] However, this result was a purely mathematical one. More important from the practical point of view was to investigate the applications of bilattices for logic and reasoning. I carried this investigation together with my first Ph.D. student, Ofer Arieli. Our fruitful cooperation led to some papers on logics for uncertain reasoning that are based on using what we call logical bilattices [29, 32, 38, 39]. Those papers became rather popular.

The logic induced by bilattices is paraconsistent. Moreover, inconsistencies in our knowledge is one of the major concerns of the vast area of uncertain reasoning, which may be classified as my area at that time. Accordingly, I naturally returned to make research on paraconsistent logics. A good opportunity to become updated about the state of the research on paraconsistency was the first congress on it that took part at Ghent in 1997. It was a big event, with many invited speakers, but I was not one of them then. Still, I was happy to present there my work with Ofer in a contributed talk. That congress was rather fruitful for me: I got to know most people who were working on the subject, and with many of them I have been keeping close connections ever since. (And not long after that congress, the community started to recognize me as one of the main experts in the field of paraconsistency.)

Another meeting that turned out to be very fruitful for my research was the Tableaux conference that took place in Pont a Mousson at the same year. It was the first Tableaux meeting (out of many) in which I took part, and I gave there a tutorial on the proof theory of propositional modal logics. But again the most important outcome for me were the new personal connections I made there. By far, the most important among them was the acquaintance I made with Beata Konikowska from the Polish academy. It was the beginning of my first (and so far the only) long-term cooperation, leading to several joint papers, with a researcher other than my past or present students.

In addition to the developments in my research, there was also a crucial development at those years in my other academic activity: teaching. After returning from Stanford I became one of the two main teachers of the very first course which is given by us since then to our first-year students: discrete mathematics. This course has in our department two parts: an introduction to set theory and general mathematical concepts, and standard subjects in combinatorics and graph theory. When I started, I knew almost nothing about the second part. However, I was the department's expert on the first, and I was very enthusiastic about this chance to shape the course according to my views about what every student of mathematics or computer science should know and understand. Accordingly, I turned the first part of the course into an advance introduction to the language of mathematics and its logic. I put a

[17] Unfortunately, there was also a very unpleasant affair connected with this nice result. One of those who independently (and using a different method) proved Fitting's conjecture after me, a guy named Pinko, refused to recognize my priority. More than that, he blamed *me* of stealing his result—even though my paper on the subject had already been published by the time he submitted his. What Pinko wrote me and tried to do next was simply unbelievable. But something I would better skip the details here.

particular emphasis on the correct use of formal expressions. Thus, I introduced the use of λ-notation and its associated rules into the material of the course, and used it myself consistently. (The other main teacher did not like it at first, but then learned to appreciate the advantages of using λ.) This was a small revolution. Indeed, the course we developed was different from any other one taught in Israel, or presented in textbooks on discrete mathematics. Therefore, I realized at a certain point that I should write my own book on discrete mathematics. It took me several years to complete it. It is a book in Hebrew that only our students can get. However, I do hope to find one day the time and energy needed in order to translate it into English and publish it. Anyway, writing this book and teaching this course involved a lot of thinking. As will be described in the next section that thinking had very significant consequences for my research as well.

In addition to my book on discrete mathematics, I wrote in those years another book in Hebrew. It was a small, popular book on Gödel incompleteness theorem and the problem of the foundations of mathematics. The book was based on 13 short lectures I gave on this subject in the Israeli radio, in the framework of what is called "broadcast university". In contrast to my other book, this one *was* published in the broadcast university's series of books, and it has been rather successful. My book and a similar one by David Harel on the foundations of computer science are still the only two books (out of hundreds) in this series that are about mathematical subjects. I have been urged many times by colleagues to translate it into English, and I hope that one day I will.

Full Professor 1999–

My becoming a full professor was a very important event for my department. By this it has reached the minimal number of full professors which is needed in order to be able to become an independent school. So in the following year we left the school of mathematics, and became the school of computer science. (Ironically, I myself was not happy with this move...) As for myself, since I have never wanted to be the head of anything, my being a full professor was for me the height of my professional career. I was at last free from any worry about promotions, etc., and could do research on whatever I like. Accordingly, since then I have been having peaceful academic life, with no important turn points. I have even been avoiding any trip abroad that is more than 18 days long. Therefore, during my sabbaticals I remained in Tel Aviv. On the other hand, there was a very important change in my status outside the academy: at the end of 2010 I became a grandfather. Since then, my two elder children gave me more grandchildren, and at the time of writing the set of my grandchildren includes three grandsons and two granddaughters. I hope and believe that one day my youngest son will add new members to this exclusive set.

One remarkable event in my academic life, that did take place at the period described in this last section, was the workshop "Logic: Between Semantics and Proof Theory", which was held at Tel Aviv University on November 2012, on the occasion of my 60th birthday. I was really moved and very grateful to see so many friends coming from all over the world to take part in this celebration. Later most of them (as well as others) also contributed papers to the proceedings of that workshop, which was published in February 2016 in the form of a huge special issue in my tribute of the Journal of Logic and Computation. The workshop itself was organized by my previous Ph.D. student Anna Zamansky, with the help of Ofer Arieli, and the Ph.D. students I had at that time.

Talking about students, I have not had too many. Still, I was very lucky with those that I did have. All of them were exceptionally good (from any point of view). I should add here that I am proud about the fact that all the students who have finished their Ph.D. under my supervision have found positions in the Israeli academy: Ofer Arieli is a Professor at the Academic College of Tel Aviv, Anna Zamansky is an Associate Professor at Haifa University, Ori Lahav is at Tel Aviv University, Liron Cohen at Beer-Sheva University, and Yoni Zohar is joining Bar-Ilan University next year. (I am sure that they will all become full professors in the future.)

So far about honor. In the rest of this section, I describe my major ideas and directions of research during the years that passed since I became a full professor.

Non-deterministic Matrices

At the abovementioned congress on paraconsistency at Ghent, I met Diderik Batens for the first time, and heard from him about his adaptive logics. I wished to know more about this approach to paraconsistency, and so suggested adaptive logics to a new M.Sc. student of mine, Iddo Lev, as the topic of his M.Sc. thesis. While studying together Batens' papers I discovered a very interesting idea hidden in one of them. It was connected with the semantics that Batens gave to the basic ordinary (i.e., not adaptive) logic **CLuN**, on which adaptive logics are based. **CLuN** is obtained from positive classical logic by adding to it the axiom of excluded middle, and it is easy to see that a corresponding cut-free Gentzen-type system is obtained from the classical one by deleting the left introduction rule for negation. Batens' semantics for that system looks at first strange to me, and in trying to understand better what is going on there, I realized that its presentation can be simplified and better understood if it is put in the form of what I immediately called "two-valued non-deterministic matrix (Nmatrix)". (Here being in a computer science department was rather helpful!)

The notion of an Nmatrix is a generalization of the usual (algebraic/truth-functional) notion of a logical many-valued matrix, in which the "truth-tables" that correspond to the connectives may be non-deterministic. Once this idea occurred to me, I immediately saw its great potential.[18] So after Iddo submitted his M.Sc. thesis and became my Ph.D. student, we devoted our joint research to it. We started with

[18] As in the case of hypersequents, it turned out that I had not been the first to have this idea. Ori Lahav found at a certain point that both Schütte and Girard applied certain three-valued Nmatrices in their books on proof theory. Much before Ori's discovery, J. Marcos sent me a paper of Crawford and Etherington, which uses another three-valued Nmatrix. Years later I discovered by accident that

generalizing the case of **CLuN**, by showing that every logic which is obtained from classical logic by deleting some of its Gentzen-type logical rules has a characteristic two-valued Nmatrix. Then we generalized this too, by introducing the notion of a canonical Gentzen-type system, and showing that such a system admits cut-elimination iff it is not trivial; iff it has a characteristic two-valued Nmatrix; and iff it satisfies a certain simple, easy to check, coherence criterion. ([52,61]. The implications of these results to the "tonk" problem are described in [92].)

After completing the study of two-valued Nmatrices, the turn came of multiple-valued Nmatrices. The first related main result was due to Iddo: He proved that the compactness theorem applied for every logic that has a characteristic finite Nmatrix.[19] Unfortunately, after that Iddo decided to switch into the area that really interested him: AI. So although he had already made a nice progress, he left it all, and went to Stanford in order to do there his Ph.D. in AI and NLP. So I continued without him (sometimes with the help of Beata Konikowska). Thus, following meetings and discussions with W. Carnielli and J. Marcos, I found that the use of Nmatrices is particularly efficient for the study of their big family of paraconsistent logics called LFIs (logics of formal inconsistency). This study revealed again the big advantage of the semantic framework of Nmatrices: the modularity it allows in developing effective semantics as well as cut-free Gentzen-type systems, for families that contain thousands of logics.

The framework of Nmatrices has been one of my main research topics as a full professor. Many of my papers at this period, either alone, or with others, are devoted to various directions of its applications. Some examples of such directions are first-order languages and beyond (together with Anna Zamansky); constructive logics and non-deterministic Kripke frames (together with Ori Lahav); fuzzy logics (together with Yoni Zohar); proof theory (together with Beata and Anna); and knowledge bases (together with Beata, J. Ben-Naim, and Y. Dvir). [97] is a survey of most of the results in this area at its first 10 years.

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the Russian logician Y. V. Ivlev practically introduced Nmatrices not long before me. He called them "quasi-matrices" and applied certain special such "quasi-matrices" in modal logics. (This discovery was rather embarrassing for me, because I saw that Ivlev had a talk about it in a conference at Torun at 1998, in which I took part too. The name of the talk was "Quasi-matrix logic as a para-consistent logic for dubitable information", and I do not even remember whether I attended it or not. Even if I did, I certainly did not understand then what the speaker was saying—which is usually what happens to me in talks... Strangely, nobody has ever told me about Ivlev's work. Not even Ivlev himself!) Nevertheless, I did reach the idea independently, and as in the case of hypersequents, I was the one who turned it into a new subject of its own, with many diverse applications.

[19] Later, I found out that this generalizes a similar theorem of Shoesmith and Smiley for ordinary matrices.

The Book on Paraconsistent Logics

In 2010, Ofer had a sabbatical, and he had the idea to use it in order to write a first extensive book on paraconsistent logics. (There was no such a book then.[20]) He suggested to Anna and me to join, and we both liked the idea. We thought then that it would take us about a year. However, the project turned out to involve much more work than we had anticipated at its beginning. At a certain point, we realized that our initial plan for the book (which includes topics like non-monotonic inference mechanisms, first-order systems, and several more) was too ambitious. In order to be sure that we finish the project one day, we decided to restrict it to the propositional level, within it to ordinary (monotonic) logics, and among them only to what we call *effective* logics. For the latter, we made a list of criteria that a logic should satisfy in order to count as such. However, even with this restricted scope, we soon saw that a lot of research is needed in order to write a book of the type we want.

- First, we had to provide exact definitions for many fuzzy notions that had been used in the literature on paraconsistent logics. This even includes the very notion of a "paraconsistent" logic itself. Our precise definitions naturally led, in turn, to the need for precise propositions about the various defined notions and about the relations among them (together with precise proofs of those propositions).
- Second, we discovered that many of the logics that we had thought should unquestionable be dealt with in our book were not meeting yet all our criteria for effectivity. So we had to fill in many serious gaps that existed in the knowledge about those logics, while we were writing our book.

As a result of all these circumstance, our goal of writing a book on paraconsistent logics developed into a massive research program, which led to many new ideas and results, as well as to papers that described them. The work took us, therefore, more than 8 years, and the book was published only in 2018. On the other hand, I (at least) am very pleased with the final outcome—I see it as the climax and ultimate conclusion of my 40 years of research on paraconsistent logics of all sorts.

Safety Relations and Predicative Mathematics

In my course on discrete mathematics, I was putting a lot of emphasis on teaching students how to correctly manipulate formal expressions, like abstract set terms and λ-terms. With the former I had the problem that not every such term can be taken as denoting a set. ($\{x \mid x \notin x\}$ is a case in point.) So I developed (and taught) a system of syntactic rules, closely connected to the axioms of the formal set theory **ZF**, for writing legal abstract set terms. At a certain point, I noticed some similarity between those rules and Ullman's rules for writing safe (i.e., domain independent)

[20] In contrast, by the time we finished writing our book there were at least two. Neither of them has the broad scope of our book, though.

queries in database theory. To exploit this similarity, I had to generalize the domain independence *property* of formulas to a *relation* between a formula and its set of free variables. Following Ullman, I called such a relation "*safety relation*". Using one particular such relation, I was able to provide a rather convenient formalization of **ZF** in a language which allows the use of abstract set terms. My formulation was based on purely syntactical principles (most of them taken from database theory), and has very natural axioms. I believed that my system may be useful for MKM (Mathematical Knowledge Management). So I published it [58], and then generalized it to a general syntactic framework for formalizing set theories [77].

The next step in this line of research came when I observed that there is actually a property of formulas (due to Gödel) called *absoluteness*, which is very important in the meta-theory of set theories on one hand, and on the other it is really close (semantically and syntactically) to the property of domain independence (d.i.) which is used in database theory. Moreover, my notion of a safety relation unifies these two properties in a rather nice way: a formula is d.i. if it is safe with respect to its whole set of free variables; a formula is absolute if it is safe with respect to the empty set of free variables. This observation, together with the recognition (already due to Poincaré and Weyl) that absoluteness is the key idea and notion in the predicativist program, made it possible for me to contribute at last to the research on predicative mathematics, and even to develop my own version of predicativity. At first, there was one obstacle, though, to do so in a fully satisfactory way: As long as I confined myself to the use of a first-order language (augmented with variable binding term operators), there was essentially just one way to introduce the natural numbers as a set into my framework: by using brute force. However, I saw that this problem could be solved easily and naturally if ancestral logic and its language are used as the underlying logic and language. I was, of course, happy to introduce both into my framework. Combining that with the basic principles of safety, I developed a syntactically defined predicative set theory which I am calling **PZF**. (See [91].)

Predicative mathematics has been (and still is) one of my major research topics in recent years. One direction of this research is developing classical analysis within **PZF** and related systems. This was one of the main subjects investigated by Liron Cohen at her Ph.D. thesis. (See [127, 128].) Understanding other approaches to predicativity and comparing them to mine is another important current direction of research. Thus, my latest (so far) rather big paper [139] is doing that to Weyl's original system in his classical book "**Das Kontinuum**" from 1918, while my Ph.D. student Nissan Levy and I are at present investigating the relations of my systems with those which have been studied in Friedman-Simpson's program of Reverse Mathematics.

The part of my research that is connected with the use of safety relation has one more branch, whose ultimate goal is to develop a general, unified theory of constructions and computations. It started with yet another observation about safety relations, which this time I made when I was teaching an advance course devoted to Gödel's incompleteness theorems: that the same principles that underlie d.i. in databases, and absoluteness in set theory, can be used to characterize decidability of formulas in computability theory and formal number theory. Some steps toward the aims of this branch have been made in [78] and [130].[21]

A Proof from THE BOOK

I would like to end my story with a very short paper of mine (together with my old friend and colleague Nachum Dershowitz) that I admit to be particularly proud of. Like my papers on geometric constructions with a compass, it is not a paper on logical matters, and like those papers (and many other works of mine), it has grown out from a course I was teaching: the course on discrete mathematics (again).

As I said above, our course in discrete mathematics includes a chapter on graph theory. The choice what to include in it was made by Prof. Michael (Miki) Tarsi, one of our experts in combinatorics, who was the other principal teacher of the course. Despite the very short time that we were able to allocate to this topic, Miki wanted to teach in our course at least one nontrivial nice result in graph theory, and he chose for that Cayley's formula for the number of trees. The only proof that he knew (and so also I, who have learned the subject from him) was the one which is based on Prüfer's code. Accordingly, Cayley's formula has been taught since then by all of us using basically that proof. However, every year when I was reaching the subject, I gave some thought to it, trying to get deeper understanding of the theorem and its proof, and better ways to present it to the students. At a certain point, I decided that it would be easier to derive Cayley's formula from another formula (which I learned later that Cayley had known too), for which using a Prúfer's code is somewhat easier and clearer. After few years it occurred to me that the easier formula can further be generalized. So I devoted some thinking to the whole subject—and at a certain point I was surprised to realize that I have found a proof of Cayley's formula which is totally different from the one I had known. I also discovered a sequence related to the topic whose limit was the famous number e .

With all my past experience, I could not believe that the proof and limit that I had found were new. Therefore, I sent them first to the experts in combinatorics in Israel that I knew. None of them had been acquainted with either, but I was told by Noga Alon that there are many known proofs of Cayley's formula. So I started to look at the literature, and indeed saw many proofs. In fact, in the famous **Proofs from THE BOOK** (of M. Aigner, G. M. Ziegler) alone I found four.[22] However, none of the proofs I read could be viewed as identical to mine (even though one of the proofs in **Proofs from THE BOOK** was based on a similar approach). Meanwhile, Nachum found a significant simplification of one important step in my proof. With

[21] Like Iddo Lev, Shahar Lev (no family connections) is a former brilliant M.Sc. student and then Ph.D. student of mine, who did not finish his thesis. In his case, the reason was that he got tired of the academy, and left it for challenges at the industry, which he finds as more exciting.

[22] This is a book that presents selected particularly nice proofs from all branches of mathematics—proofs of the type that its authors believe should belong to "THE BOOK", in which, according to Erdös, God keeps the most beautiful mathematical proofs.