A. M. Turing Award Oral History Interview with
Dana Stewart Scott
Part 1 of 4

by Gordon Plotkin

Berkeley, CA and Edinburgh, Scotland

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Gordon Plotkin: It's a very great honor indeed for me to interview Dana Scott. Dana of course is the joint winner with Michael Rabin of the 1976 Turing Award. However, he's accomplished far more than that, having made a wide variety of seminal contributions to mathematical logic, to philosophy, and to other parts of computer science, particularly the denotational semantics of programming languages. What is more, during the course of a long career starting in the 1950s, he has known and interacted with an uncountable number of people, logicians and computer scientists, including those central to the foundations and development of the field. We have clearly much to learn.

I am Gordon Plotkin and this interview is taking place on November the 12th, 2020. Due to the pandemic, it is taking place over Zoom. Dana is in Berkeley and I am in Edinburgh.

Dana, the story is a long one, but luckily there's a standard place to begin – with your birth. You have a very long association with Berkeley. I believe you were even born there.

Dana Scott: So 1932, October 11, 1932 is my birthdate. It's in Berkeley, California, where I am now in retirement. But my parents were living in San Francisco. In 1932, there were no bridges. The Bay Bridge, the Golden Gate Bridge were yet to be built. So my parents came to a dinner party by automobile using the enormous auto ferries that used to go back and forth across the bay. But during dinner, my mother realized that the baby was imminent and she refused to go back home on the auto ferry and I had to be born in the Berkeley hospital. Fortunately, this hospital is the same hospital that our two grandchildren were born in too, so we have lots of Berkeley connections.

My parents were married in Fort Bragg, California in about 1922. My father was born there. My mother was born in northern, northwestern California near Mount Lassen in Susanville, but then various of the relatives moved to Fort Bragg and she was living there when she met my father. I wasn't born until about 10 years later, and then my parents were sadly, for me sadly divorced when I was about five years old.

In our family, I think I was the second to go to college to get a college degree. My father had two brothers and a sister. The eldest brother died tragically in an accident in about 1915, but his other brother came to the Bay Area and after college became a very prominent dentist. But neither my father nor his sister went to college. And in my mother's family of four siblings that she had, none of them had gone to college. Of course high school degrees, but no college until my time.

My early days were moving around in Northern California in many places. My mother remarried and we lived on a farm near Susanville where I started the first grade in a one-room schoolhouse. All the classes were together and there were just three of us in the first grade. I can still remember it a bit. Then my mother divorced after her second husband turned out had misrepresented his financial position, and I continued school in Reno, Nevada for a year until my mother and I moved to Stockton, California because other relatives were living in Stockton. There I went to the elementary grades and the beginning of junior high school. I still remember hearing on December 7^{th}, "Extra, extra, read all about it. Pearl Harbor bombed." I can still hear that sound in my voice.

Not long after that, my grandmother passed away. She had a small house in Fort Bragg where she had moved to be near relatives, and my mother inherited it. So we moved to Fort Bragg where I continued elementary school and the beginning of junior high.

My mother remarried a salesman. She was working in sales at the Union Lumber Company department store and she met her next husband there. He was located in Chico, California, and so we moved to Chico, where I finished junior high school and started high school. That was very instrumental in my development, because it was a much larger town and a very fine high school there where I attended two years.

The key thing there was I had started already in Fort Bragg to learn the clarinet a bit. I'd had piano lessons. Music was very important in my life, all my life. I was very lazy with my piano lessons and my mother cancelled them on me for my laziness. But I took up the clarinet and then continued the clarinet in high school. The band teacher there was very interested in his students and their development. He saw one thing about me in giving me lessons and coaching me for the band, being in the marching band, was that I was very interested in why instruments made their sounds. He had a book that he had acquired many years before by a professor from Case Western called *Science of Musical Sounds* by Dayton Clarence Miller. He was quite an interesting experimentalist. He had contributed much to trying to determine the speed of light and things like that, and he wrote this book on musical acoustics, which I found fascinating. But the problem with it was it had lots of math, both in calculus and also understanding how it applied to physical objects. So I had to learn some calculus on my own. I had a book from my mother's older sister that her husband had had, so I taught myself a little bit of calculus and I started to understand those things about musical acoustics.

After the sophomore year, my stepfather got a new job in Sacramento, and so I was very sad to leave Chico where I had many friends. The school was really an excellent school there and the math courses there were quite good. I loved math. But moving to Sacramento was also it turned out a great luck for me because it made it much easier to then apply for the University of California. If we had stayed in Chico, I would have gone to Chico State College and become a high school math teacher, probably. But going to Sacramento and then having the opportunity to get a scholarship to UC Berkeley really changed my life.

But something else changed my intellectual life there in Sacramento. The State of California had a state library, a very dusty state library, and I found there a book *[0:10:00]* on musical acoustics and musical details and musical theory by Hermann von Helmholtz, the famous German scientist. I was fascinated by that. The things that I learned from that from the point of view of math was much more about Fourier analysis. Of course not advanced Fourier analysis but just how musical sounds have overtones and what it means mathematically in understanding those overtones. But the second thing I had to really study, which was hard for me to figure out at first, was logarithms. The reason is that the musical scale is logarithmic. By frequencies, when you advance by an octave, you double the frequency of a sound. But when you count octaves, you only count one, two, three, you don't count the powers of two. So I had to learn what logarithms really meant, and then I had to learn what logarithms meant to the twelfth root of two in order to understand equal temperament scale and how you… Helmholtz had very, very detailed studies of different scales and explained the tunings for the different scales in terms of this logarithmic scale. That I had to learn on myself and I did a small project for the Westinghouse Talent Search when I was a senior, and I got an honorable mention for that based on those studies from going back to Helmholtz.

But then with the very good teachers in Sacramento, and especially the math teachers were very, very inspiring. I was convinced when I then got the opportunity to go to college that I would immediately major in mathematics. And so that was the result of my high school education ending in summer of 1950.

Do you have some questions about what that early history means?

Plotkin: I have a trivial question which I wanted to know the answer to when I actually read your biography. I imagined you as a young person in the dusty state library. How did you find your way there? You just needed more books, you thought, "There's a library"?

Scott: I can't exactly remember. Maybe it's because either through the school library or the city library, it indicated that there were other kinds of books that had been there. I guess they collected them as giving information to the legislators. So probably no one had checked out Helmholtz for decades, but it happened to be there. So it was a lucky find.

And I really love the story of Helmholtz. There's a very fine biography of him. He was a surgeon. He studied all kinds of different things. He was a mathematician. Reading his biography really made me tired because he did so much in his life, and I really feel that I can contribute a lot of my intellectual awakening to some of the writings that he made.

Plotkin: Did that give you an ambition? I mean you're obviously going to study mathematics, but did you have some view of what your future might be?

Scott: No. No, no. Because in high school, you have no idea what college is like. So I had no thought. I mean, what had I studied? Algebra and geometry, and a little bit of analytic geometry. As I say, I had studied, somewhat on my own, calculus so that I could understand sines and cosines and that kind of thing, behavior of functions. But it was very, very, very elementary, so I had no idea what college was going to be like. But because I had enjoyed mathematics so much, I knew that I was going to major in mathematics at Berkeley.

Plotkin: I can't remember if you said, but was it a teacher who said, "Why don't you go to college?" or did you think, "I'm going to do that"?

Scott: Well, no. I think the high school just encouraged people who did well in high school. The McClatchy Senior High School in Sacramento, California, I think they just encouraged those students to apply to college. And so I did and was lucky to get a small scholarship to be able to go there.

Plotkin: Right. Moving on to Berkeley, to university, and to what you did there, how did you get to be interested in logic? That's a kind of starting point for you.

Scott: Okay. Well, partly it was because… Now I have to… I have to remember something… I have a problem these days at my advanced age that I block on words just as I'm about to say them. It was a Polish author, Alfred Korzybski, who wrote about… I cannot remember the name of his famous book, but I had read it in high school and it had some connection with logic. It was really much more concerned with linguistic understanding.[1] I had read about Korzybski, and so when I got to college, the first year there were some courses in

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[1] Korzybski's major work on general semantics is *Science and Sanity: An Introduction to Non-Aristotelian Systems and General Semantics,* (1st ed. 1933; 5th ed., 1994). The Institute of General Semantics (founded 1938)

the speech department, which I think no longer exists, but there were some nice teachers there and I discussed some things with them.

But then it turned out that in the second semester in philosophy there was an introductory logic course. Very elementary logic, but it was taken as a prerequisite for many other courses, so I signed up for that as a second semester freshman. Paul Marhenke, who was the teacher of that, was chairman of the department. He was very Germanic in his manner. In those days, people were smoking a lot. He was always covered with cigarette ashes on his vest while lecturing. It was a nice, easy course and so I liked logic very much, and then signed up for further logic courses in the sophomore year.

The lucky thing is that the teacher the next year was Benson Mates, professor of philosophy at Berkeley, whom I became very, very close to. He had done a thesis on Stoic logic, Greek Stoic logic, but he was very good at lecturing about formal logic. His course influenced me a lot, and of course I heard about Alfred Tarski that way. Tarski was a senior professor in mathematics and of course a leader in logic worldwide. And so I learned about the possibility of that kind of more advanced work.

But the other thing that happened in my sophomore year was that I had signed up for a course called Theory of Equations. Again, it's a kind of introductory algebra course which is no longer given because it's regarded as too elementary. The teacher of that course was a young Polish… he began as a temporary visitor and then got an appointment at Berkeley. He had studied logic in Warsaw. Tarski had left Warsaw for a big philosophy conference *[0:20:00]* in Cambridge, Massachusetts just at the beginning of the war, and then he was unable to return to Poland. His wife and two young children remained in Poland. She was not Jewish and she was able to live in the countryside with the children. But Tarski was caught here in the States. At first, he had some temporary lecturing in the east coast, and then he was appointed professor at Berkeley.

So this young person who had studied in the underground university in Warsaw, Jan Kalicki, had studied Tarski's work very, very closely, had published some small papers. He was able to escape Poland just at the end of the war before the communists took over and went to South America. So he then wrote to Tarski and told him about the work he had been doing, which Tarski liked, so Tarski arranged that he could come as a junior visitor to Berkeley. That was about 1951 I guess when he was able to come to Berkeley.

So he was teaching this course in Theory of Equations, but at the time in order to help support myself, I was working in the library in the periodicals room. At that time, periodicals had not bankrupted our libraries. There were quite a few periodicals, but not all that many, but they had to be carefully filed because every year they had to be bound up. So students were hired to do the filing and to keep the things tidy there. In the periodical stacks, I discovered the *Journal of*

Symbolic Logic, and I opened it and couldn't understand a thing in any of the articles. I was a sophomore at the time. But there was one paper on truth-tables by Jan Kalicki that I could read because, you know, finite truth-tables aren't too hard to understand. So I was able to read his paper. When I found in my second semester that he was teaching the course, I then went to him and told him, "I had read your paper." He was quite amazed and very pleased. So he suggested that we have more sessions working on those kind of things, and so we became very good friends.

Then the thing that was happening with Tarski's approach to logic, he was changing over, and had earlier changed somewhat too, from logical systems to model theory. In particular, he was emphasizing algebraic structures. Rather than truth-tables, you should consider an algebraic structure like a Boolean algebra, Boolean algebra defined by equations. There are many, many different kinds of Boolean algebras, so you think of all the models of the equational theory.

The thing that occurred to Kalicki and me was to think about those equational theories which cannot be extended to have more equations. Of course you can have a very weak theory, which has for example rings. Rings needn't be commutative. The commutative law is another equation you could add to ring theory to extend it. But there are also equational theories which are consistent in the sense of having non-trivial models beyond the one-element algebra. But the equational theory cannot be extended further without it collapsing to all equations being derivable. These we call "equational complete theories."

We discovered a number of things about equational theories. Tarski was very interested. He also thought about it in some other aspects. So several papers -- this is during my sophomore, end of the second half of my sophomore year -- several results came forward about… equationally complete theories that Kalicki and I developed. I mean it's fairly easy stuff. In books on abstract algebra now, it's only an exercise.

But in any case, that was my first experience in doing any kind of research in the sense of working out things and finding out some new facts. That of course was how I was introduced to Tarski and how he also became a great supporter for me eventually and of course for Jan Kalicki.

Very tragically, the next year Kalicki was killed in an automobile accident. He was a very, very lively person, but he was a terrible driver. He was in the car driving with Tarski, Mrs. Tarski, and one of Tarski's graduate students to go to an American Mathematical Society conference, a local conference. He lost control of the car, which turned over, and he fell out of the car and was killed in the accident. Fortunately, the other people in the car were not injured, but that was a very great blow to me because he was a very warm friend and a mentor for me.

But by this time, I had been introduced to the Tarski circle, and so I went on to then in my junior year to really take part in Tarski's teaching both in courses and in seminars.

So that's how I got to logic, through the elementary philosophy course, then through Benson Mates' advanced course, and becoming very good friends with him, and then through Kalicki and the introduction to model theory, to my introduction to Tarski. And so that's how the beginning started.

Plotkin: Of course, Tarski was one of the almost founders of logic, of mathematical logic one might say, so that was an excellent place to be. I was just going to ask basically what was Tarski like as a person, as a teacher? What was your relationship with him?

Scott: Well, he was an absolutely amazing lecturer. As many mathematicians can, he could lecture completely without any notes. So then for the junior year, in Benson Mates' course, I had met Richard Montague, who was a student of philosophy there and was very close to Benson Mates and came to Mates' lectures. And then a professor from UCLA, Donald Kalish, was coming on a sabbatical to Berkeley, and he was an old, old friend, a school friend of Benson Mates'. So there was this circle of connections. So we all then in my third year there, we all went to Tarski's set theory lectures. And of course, they were very, very inspiring because Tarski was a great expositor. And so learning Zermelo–Fraenkel set theory *[0:30:00]* and understanding things like ordinal numbers and all of that, that was quite exciting. So Tarski was a great motivator. He had many graduate students, and so there was quite a big circle of things that were happening.

Tarski also liked to give parties. One thing that he liked very much was making slivovitz. He was able to get people to smuggle pure alcohol in from Mexico, and then he would take prunes or other kinds of things and soak them in alcohol for many months. Then we all had to drink some schnapps at his parties. I remember going to one of the parties and Anne Davis, one of his early graduate students, came up to me and said, "What are you doing here? You're too young for this."

But anyway, there was quite a circle of people and interesting things. Of course, one of the close friends of Tarski were Raphael and Julia Robinson. Julia did her thesis under Tarski but was very much aided by her number theory teacher, whom, after she had been a student at Berkeley, married there, Raphael. And it was really he that conveyed… You can't imagine the difference between 1950 and today. In the Berkeley Faculty Club, only men were allowed. And so Raphael Robinson was having lunch with Tarski and Tarski mentioned some things. For example about, after all the work that had been done on first-order integer arithmetic, Tarski pointed out that there were many puzzles to be solved about rational arithmetic, the field of rational numbers. So he suggested certain things to Raphael Robinson, who then suggested that to Julia, and that's how Julia's famous thesis eventually evolved there where she showed the undecidability of the theory of rational numbers.

And so that was that connection there. And so they were very, very close friends of the Tarski circle, and luckily one semester they gave a seminar on recursive functions. And so that's really how I learned the basic of recursive function theory, from the Robinsons that time.

Plotkin: What was one's interactions with Tarski like? Was he a very severe Continental professor, or was it more intimate? For you, I mean.

Scott: Oh no, he was very personable. I mean he was very cultured. Well, there were two professors of Polish at Berkeley that he became very, very close to because Tarski had an amazing memory for poetry and could quote all kinds of poetry from memory, in Polish. That of course didn't do me any good, but he was very interested in cultural things and he was also very, very open to meeting people and, as I say, having parties and things like that with both students and faculty. And so it was a very lively atmosphere, and he was of course very interested in mentoring his students, and of course had many students. That's how I met Sol Feferman and… I'm blocking on the next name[2]. But anyway, the students around Tarski then were very influential in my further thinking about logic because of the atmosphere that was there, that Tarski certainly created the atmosphere that was very congenial.

Plotkin: Right. Coming back to Richard Montague, did you work with him? Were you just like students talking about everything together?

Scott: It was more like that. He then became Tarski's graduate student and did his thesis under Tarski. But it was just learning about logic. For example, for the set theory course where there was Montague and Mates and – again, I'm blocking on the name – other people, what we had to do afterwards was after Tarski's lectures, we had to do the homework. So there was a group of us who got together to discuss all the homework problems and to work out the proofs. That was my first interchanges with Richard. Later on, we became very good friends and roomed together just before he was finishing his thesis. Then luckily, Don Kalish, who had come to Berkeley and went back to UCLA, hired Richard Montague to come to UCLA just in the last year before he finished his thesis. He eventually then became a full faculty member at UCLA there. So I visited UCLA over the years many, many times, and the people there, there was another circle of people interested in logic too. That was part of the connection.

I suppose Richard and I were somewhat, in some ways rivals, especially rivals to get Tarski's attention, that sort of thing. So there are some stories of things that I

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[2] Robert Vaught

said to Richard that may be a little bit insulting. But it doesn't matter anymore. In any case, it was a very lively intellectual atmosphere and created lots of intellectual activity, of course very much based on Tarski's deep influence.

Plotkin: Sounds wonderful. I want to ask a question, since I have the opportunity to ask you questions. It's not so much about you, but Richard went on to invent the denotational semantics of natural language. Did you ever talk to him about that, or do you know how that came about?

Scott: Well, what we discussed together and I also wrote about over several years was modal logic. One of the things that happened in my undergraduate year was that I found the early book on modal logic. Of course I'm blocking on the name of the… Lewis and Langford. Lewis and Langford on modal logic. So already as an undergraduate I'd studied Lewis and Langford. Of course, modal logic interested Benson Mates and Richard Montague too. Tarski of course with some of his earlier students had done a lot about modal logic because he thought of it as an extension of Boolean algebra, Boolean algebra with operators, and so there were those papers. So I also studied Tarski's papers on algebra of operators very much. Of course, that led to other things much later in thinking about kinds of Boolean algebras with additional structure.

That was a connection that came up between me and Montague about modal logic and what you could do with modal logic. Of course, in thinking about natural language, modalities are very important, and so of course that figured very heavily in Montague's thinking as well.

Plotkin: What a fascinating connection. Circling around, you already mentioned Sol Feferman, another wonderful logician. *[0:40:00]* **Can you say anything about how Sol was then and how it was? Was he just part of the group there? I mean he would at least be part of the group.**

Scott: Yes. He and Bob Vaught were the two senior graduate students. Oh, there was also C. C. Chang, also at the same time. Vaught and Feferman worked together, and they were very influenced by Tarski's theory of models. But of course eventually Feferman moved over to proof theory. Tarski never lectured about or encouraged anyone else to lecture about proof theory. I chided him on that once, but it really didn't interest him so much. But then coming from the Tarski school, then Sol Feferman became the leader of thinkers about proof theory and had a big influence and many connections there.

But before, maybe it was during my senior year, Feferman was drafted and he and his wife had to leave for him to take up his military service, which he served in the east coast somewhere, not overseas. So he was pulled out of the circle by having to be drafted. This was I guess during the Korean War.

Plotkin: Really?

Scott: Vaught remained there and was very much an inspiration in his work. But it was only later, after Feferman had his professorship at Stanford that I had then closer connections with him and his family through Stanford. But that's a later period.

Plotkin: Yes. I was wondering, did you want to say anything else about any of the other graduate students then? You've mentioned several names. I don't know if you want to say any more.

Scott: Well, Anne Morel, who was at that time Anne Davis from her first marriage and in her later marriage was Anne Morel, she had already left Berkeley but came back to visit many times during the early '50s there, and so I got to know her very well. Then Tarski had a student, Tom Frayne.

What happened was that in the later 1950s, as the connection with the 1957 NSF summer school on logic, six-week summer school on logic, which was the first big meeting of logicians in the United States, Tarski raised various questions about how you could use products to show that certain theories were consistent. That suggestion that he made during that 1957 meeting then led in various ways for Anne Morel, Frayne, and myself somewhat independently but then jointly with them, to develop ultraproducts. But that was a much, much later development. We're still talking about my undergraduate years at Berkeley, but that happened then in my end of my graduate work there. But keeping connections with the people in Berkeley and, as I say, particularly Morel and Frayne.

Plotkin: That's very interesting to know that. You became a graduate student of Tarski, but that didn't last. There's a story. I don't know if you wish to tell it.

Scott: I'm not going to tell too much about it. But Tarski had had many of his early writings translated into English – they were in German mostly – translated into English by J. H. Woodger, the famous biologist who was very interested also in logic. A big volume was in preparation, of which Tarski was getting the page proofs. But the translations were not satisfactory to Tarski because of course Woodger, even though through best intentions, but as an amateur, didn't understand the logic completely there. So the page proofs had to be corrected. So various of us were helping Tarski collect the page proofs and Tarski actually hired me as a secretary to do those things.

Reading the page proofs was a horribly boring job and I got very lazy about it. Because of my laziness, Tarski became very angry and finally at one point had to fire me because I wasn't doing the work that he was paying me for doing. That was our break there.

Just at that point… This was in my first graduate year after graduation, in 1954. I graduated in 1954 and became a graduate student at Berkeley there next year under Tarski. But Norman Steenrod was visiting on a sabbatical from Princeton, and one of the other professors, finding out about the difficulties that I was having – of course it was entirely my fault, the difficulties – said, “Well, why don’t you think of going somewhere else? Steenrod is here. Go and interview him.” This was from Harley Flanders, the algebraist – I took his course in advanced algebra – suggested that I do. Because I’d already had some independent research on my own, Steenrod was very interested to meet me and said, “Of course you should apply to Princeton, and I’ll be glad to write a letter of recommendation for you.” So there was that connection with Norman Steenrod being there on sabbatical. That was then the connection to apply to Princeton for graduate school, and that’s where I went the subsequent year.

Plotkin: Before we go into Princeton, maybe one final summary question about Tarski. How would you speak of what he meant to you or what his position was in your life?

Scott: Well, I think it was his intellect. I mean the studies of logic and foundations and set theory and axiomatics and that sort of thing really grabbed me. But he was an absolute superb expositor to make things understandable and to formulate things in a clear and simple way. I mean he was a really outstanding, talented man. That was how the influence really settled on me. It was his intellectual powers of being able to explain things. I think that’s what I credit him for.

Of course he also was very productive and proved so many things, and of course we all studied things about the undecidability. The Robinsons were very much concerned with undecidability questions, and Tarski, Mostowski, and Robinson wrote their monograph on *Undecidable Theories* together. So also that kind of exposition was very influential also on my learning things.

Let’s go back to my sophomore year at college.

Plotkin: Okay.

Scott: I was learning about formal theories and I found the little book by Paul C. Rosenbloom on mathematical logic, which covered many topics. One of them was Zermelo–Fraenkel set theory. So I learned quite a bit on my own reading Rosenbloom’s book on set theory. But another chapter *[0:50:00]* of the Rosenbloom book was combinators and lambda calculus, Curry’s combinators and Church’s lambda calculus. At the same time, I also got a copy of the very small volume on lambda calculus that Church wrote as well. So I studied those things in my sophomore year by those books.

Now in the Rosenbloom book, he explains in a fairly short order the complex interrelations between Curry's combinators. So I like to say that one afternoon after spending a lot of time trying to puzzle out how combinators combine and what they do to reproduce one another through their equations, I worked so much on it that at night when I went to bed, I had nightmares about combinators. There were all of a sudden these gigantic combinators that were coming to bite me or to do something terrible. I don't know whether that nightmare about combinators was what cemented my interest in thinking about lambda calculus, but that's exactly how it started.

But I didn't do much else about combinatorial logic or lambda calculus in the early years at Berkeley. When I got to Princeton, I was very interested to meet and be directed by Alonzo Church. But unfortunately because… Church had originally thought that he had solved the logical paradoxes and his logistic system, which involved lambda calculus, was going to solve the problems that Frege couldn't solve that led to the Russell paradox. But unfortunately Church's two students showed that his system of logistic was inconsistent. The only thing that was left so to speak of it was the equational part of lambda calculus. So Church wrote that up in his small monograph, but the logistic system that he had such hopes for failed.

So much later when I got -- I mean his monograph was published in 1940 -- much later when I got to Princeton in the late '50s, Church never discussed lambda calculus with his students. He did of course somewhat earlier write about *typed* lambda calculus and his formulation of the theory of types that many people pursued. In particular his student Leon Henkin very much worked on Church's theory of types. But the untyped lambda calculus Church never lectured about and never discussed with any of his students. So it's really too bad. I never had really a chance in order to discuss modelling untyped lambda calculus in later years.

Plotkin: Yes. Well, but did you think about models of lambda calculus even then or the thought that it could be …?

Scott: No, not really. I mean, I of course understood it as an axiomatic theory, but not from any model-theoretic point of view. Of course, the typed lambda calculus had all kinds of models to consider that many people worked on. But the untyped lambda calculus, I didn't at all think about it from a model-theoretic point of view at that time. We're talking about the late '50s.

Plotkin: Right, right. Jumping a tiny bit, Turing was a famous PhD student of Church, obviously a large number of years before you arrived there. Did Church ever say anything about Turing or how all that went, or…?

Scott: No, he never spoke about Turing. Even with the Turing centenary, Church wrote only very, very little about him. I don't think there was a close personal connection. Turing didn't like being in the States very much. He was only there for two years. He had really been recommended to go there because of the Turing machine approach to undecidability that he had developed as a student in Cambridge. He was mentored by von Neumann, who was professor at the Princeton Institute for Advanced Study. But Church insisted that he formulate all of his work in terms of lambda calculus, and in one of his letters, Turing said how much he hated to do that, but it was what he did in order to finish his PhD under Church for using lambda calculus for transfinite computation. But I don't think they had a personal, very much personal relationship.

Plotkin: How was your relationship with Church? Was he helpful for your thesis, or how was it?

Scott: Not really. Church invented the reviews for the *Journal of Symbolic Logic*, which was started in the middle '30s, and he spent a herculean amount of effort on promoting the reviews. Even at the time that I was at Princeton as a graduate student, he was spending most of his time editing the reviews. So it really didn't relate in much connection there. I was there only for three years. Of course, he had several graduate students, including Michael Rabin, that we'll want to speak about. But his kind of direction was to discuss with the students what kind of area of research that they wanted to do, and then he just let them do it. I very unkindly like to say that Church corrected the spelling in my thesis. But the problems really came from Tarski much earlier, and so he didn't have really much influence on my research topics at all.

Plotkin: Okay. A slight excursion then. I noted that with Hale Trotter you worked on the Princeton von Neumann machine, or you worked with it. So you did some programming. Can you tell us about that? Perhaps "escapaded." I don't know if that's the right word.

Scott: Yes, that was… I mean we're jumping ahead here, but that was very late in my time at Princeton.

Plotkin: Ah, was it?

Scott: I had become friends with a professor at Electrical Engineering called Forman Acton, who was an early advocate of computing machines. It turned out that he had money for a summer project – this was in my senior year – for working on using the von Neumann machine… You see, the von Neumann machine, it's very well written up by George Dyson about the history of von Neumann and computing at the Institute for Advanced Study and the development, which was a great engineering feat to develop the von Neumann machine there. The von Neumann machine eventually became the basic blueprint for the IBM 707 series, was taken over, and of course there was one version of it at Los Alamos. But after von Neumann died of cancer – I never got a chance to meet him, he was already ill in the hospital when I got to Princeton –

the Institute for Advanced Study never wanted to have anything to do with engineering, and so they *[1:00:00]* gave the von Neumann machine to Princeton University. There were several people still using it for astronomy and other kinds of calculations, but it was given to the university from the Institute for Advanced Study.

Unfortunately, the university discovered it's very expensive to keep a machine like that running. For example, the memory of the von Neumann machine was electrostatic tubes, Williams tubes. By exciting an area of the phosphorus on a tube, the excitement would persist for a small interval, and so that became a memory, because you could keep refreshing the excitement. That's how you preserved the memory. There were only 1024 words in the memory, and it was very tricky to keep it running well. In fact, the electrostatic tubes were much affected by humidity – Princeton can be very humid – so the best time to work on the von Neumann machine was three o'clock in the morning on account of its physical characteristics.

But Forman Acton hired Trotter and myself to do some projects on the von Neumann machine because the university just had just been able to get access to it. Trotter and I thought it would be a good idea to work on some kind of combinatorial problem. The influence on doing the work on it had really come from the popularity of a puzzle called Pentominoes, which was a kind of jigsaw puzzle with different shapes made out of squares, and you have to fit them together to make a rectangle. We thought that would be easy to code up in terms of binary numbers to search for solutions to this jigsaw puzzle thing. That's how we… because of the interest that many people had had in the puzzle. The Robinsons for example in Berkeley loved puzzles, and I think they had introduced me to Pentominoes in the first place. But all kinds of people, including Martin Gardner, liked it very, very much, and so the interest in this kind of combinatorial puzzle was in the air.

But then it was the close friends of the Robinsons – oh, again I'm blocking on a name, I'll think of it in a minute – they had done a lot of number theory computation on the SWAC computer at UCLA, and in doing searches, they had done… I'd heard them lecture on the backtracking method, which means you follow some conditions forward until you reach a brick wall, and then you backtrack and change one of the conditions to branch off another way, and you keep backtracking and backtracking until you find the path to a solution or eventually even to all the possible solutions. So the Lehmers… It was Dick Lehmer, the professor of number theory at Berkeley who was close -- he and Emma Lehmer, his wife, were close, close friends of the Robinsons -- that was the inspiration for understanding backtracking. So that's what was applied in doing the combinatorial puzzle on the von Neumann machine.

And it worked out. We were able to completely do one version of the puzzle. That was the summer of '58. Then I think by the fall, the Institute for Advanced Study had closed down the von Neumann computer. Eventually I saw the corpse of the machine, which was very sad, in the Smithsonian Institute. Because what was fun about the von Neumann computer, because it had these flashing lights on the cathode-ray tubes to check the memory, after you did your programming on punch cards and it was read in, you then could watch the progress of your computation by watching the flashing lights on the cathode-ray tubes. And it was so sad to see the guts of the von Neumann machine as a dead corpse displayed in the Smithsonian Institute.

But I would strongly recommend reading George Dyson's book called *Turing's Cathedral*, which he gives the complete history of the von Neumann machine and the many things that were done with it. But that was my small connection with it, through that summer job. Okay?

Plotkin: Yes, indeed. Yeah. And I agree, it's a very nice book by George Dyson.

There's so many questions one can ask. In connection with the Institute for Advanced Study, of course Gödel was a famous member. Did you already meet Gödel then? What was your interaction then if you did?

I didn't become close to Gödel really until I was a faculty member at Princeton in the early '70s. When I was a graduate student in Princeton, Church introduced his students to… or Gödel to Church's students, and there were interactions there. But during my time as a graduate student there, I became very closely associated with a visitor, Georg Kreisel, who came to the Institute for Advanced Study. He and Gödel were very, very close. Kreisel and Gödel loved to talk for hours on the telephone. Both of them were very hypochondriac and talking on the telephone is an excellent way of not spreading any germs. So they would have hours-long conversations on the telephone. Of course in German. German was a very important thing. I'm sure that the German language was one of the strongest things that made his friendship, Gödel's friendship with Einstein, because both of them felt somewhat culturally isolated, and so being able to talk to each other in German was very important for them, as it was with Kreisel and Gödel.

So I learned a lot about Gödel's thinking through Kreisel, and I feel I should go over some thoughts about that. And we'll have that, details of that as the topic of conversation in our next session. But for the time being… When I became closer to Gödel, and he of course could be very gracious and he wrote once a nice letter about work I did about set theory, but that really transpired in the later time, in the early '70s when I was a faculty member at Princeton.

Let me say something more about Princeton. Of course it was a very exciting atmosphere because so many mathematicians and people visited Princeton. And of course that was how I met Kleene. Kleene was one of Church's earliest students, second or third student along with Barkley Rosser. They came to Princeton often to visit. But Kleene came for a sabbatical year while Church was on leave, and so I became close to Kleene at that time, and that was very inspiring.

Many, many other visitors came for talks and seminars, and many famous mathematicians. Of course, Emil Artin. Of course I knew about some of Artin's work in algebra because Tarski discussed real closed fields and the Artin–Schreier paper on real closed fields was very important, and so it was quite exciting to meet and hear Artin lecture. But there were many other very interesting mathematicians at Princeton, and so it was a big… *[1:10:00]* together with the Institute for Advanced Study and all the visitors who came there, it was a big influence on me to see that wide culture of mathematical research.

So it was accidental in a certain way that I went to Princeton, just like it was accidental that I went to high school in Sacramento. Those accidents were enormously helpful in the long run.

Session 2: December 29, 2020

Gordon Plotkin: Okay. This is Part 2 of the Dana Scott Turing Award interview. Dana is in Berkeley and myself, Gordon Plotkin, I'm in Edinburgh. It's December 29th, 2020.

And we begin the interview at Princeton. Dana, how did you get to know Kreisel and Gödel?

Dana Scott: When I got to Princeton, of course it was a very big environment, lots of mathematicians, because the Institute for Advanced Study brings so many people there into Princeton. Steenrod of course was in his most active period and had many graduate students. There were other very interesting professors there too. Ralph Fox in knot theory, I took lectures from him. That was very interesting. I wish I'd taken the lectures from Emil Artin on algebra and I wish I'd learned more about algebraic topology as I should have then, but I was too concentrated on logic, I'm afraid.

Toward the end of my first year, my living accommodation was not so good and Kreisel had begun his more-than-two-year visit at the Institute for Advanced Study. And I got to know him. I mean there were lots of meetings with logicians. I met Gödel. Church had various parties. I think Bernays visited and Church had a party for graduate students to meet these people. But I didn't really get to know Gödel very much in the beginning. But I did become good friends with Kreisel. It turned out that he wanted to rent a small house out in the country, and so we agreed to share that house out there.

There were very many visitors who came there. I remember distinctly that Gödel was invited to tea out there and I was assigned to drive Gödel out to the tea party at Kreisel's. During the drive, I turned to emphasize something and turned to Gödel to say something. He said, "Keep your eyes on the road!" He was a very nervous person in the car. One of my favorite stories about him was that one year the Reidemeisters were visiting from Vienna, old friends that Gödel knew when he was a student in Vienna, and they were staying with the Gödels. It was decided that a drive in the beautiful countryside would be very nice on the Sunday, but Professor Gödel decided he wouldn't take part in that. So Mrs. Gödel was driving the Reidemeisters and all of a sudden she said, "Oh, it's so wonderful to be out driving without a genius in the backseat."

My other favorite story about Gödel was from Church's secretary. Church had enormous work with the *Journal of Symbolic Logic* in doing the reviews section, and he had a full-time secretary to help him on that. She was the wife of a graduate student in topology, and I knew both of them very, very well. She reported that she was on the city bus one day, and that was a period when Gödel had been in the hospital. Mrs. Gödel got on the bus at a bus stop and a friend of hers greeted her and said, "Oh, oh, how is your husband doing?" Mrs. Gödel threw up her hands and said, "Oh, that man, he's nothing but a brain on two legs." I think what she meant was that nobody ever won an argument with Gödel because he could think of too many if, ands, and buts and alternatives. He must have driven his doctors absolutely insane because he would object to anything that was proposed on some grounds or the other. So it was always very difficult with his medical things.

Later when I was in Princeton, I got to know Gödel much more, especially through his friend Oskar Morgenstern, of von Neumann–Morgenstern fame. Morgenstern was also from Vienna. And Gödel was always an extremely charming person. I wouldn't say any problems about being a brain on two legs, but Mrs. Gödel had a different relationship with him.

In any case, Kreisel introduced me to quite a lot of things, especially concerning intuitionistic mathematics. As an undergraduate, I'd studied Tarski's papers on modelling intuitionistic mathematics, the so-called topological interpretation. But I didn't have an understanding for intuitionistic mathematics, which I gained much more through the association with Kreisel. Kreisel and Gödel during that period were also working very, very closely together. They might spend hours on the telephone talking to each other every day because talking on the telephone meant you weren't spreading any germs. So they both, being hypochondriac, liked that. But in any case, the relationship to Kreisel both as a friend but also as a teacher for things that I hadn't learned before was what happened there during my middle years there at Princeton.

Plotkin: Perhaps a central question of the interview. Michael Rabin was a fellow graduate student of yours at Princeton. Obviously, as we all know, it was your work with Rabin that led to your joint 1976 Turing Award with your famous invention of nondeterministic finite automata. Could you tell me all about that, please, about Rabin and how you worked with him, where it was, how it came about?

Scott: Yes. Well, there were quite a few graduate students there. Simon Kochen was a very important one. Raymond Smullyan came also as a graduate student. It was a very lively atmosphere. Michael Rabin was a year ahead of me, and we became very good friends. I don't remember if he got married while he was at Princeton or very shortly after that.

But it turned out in 1957 – that was my second year at Princeton – IBM recruited summer internships, and so both Rabin and I were chosen to go to IBM Yorktown Heights Research Center. The very fancy building that they have today wasn't quite completed then, and IBM had rented an estate, a vacant estate. The family had passed away. So the Lamb estate was where they had their research group there.

Now earlier, John Myhill, the logician John Myhill, who had done initial work in Chicago especially influenced by Anil Nerode, had given a talk in Princeton about his work on automata. When Rabin and I got to the Lamb estate, to the IBM center, we didn't know what to do. So we said, "Well, maybe we should review this work about automata and to think about automata." We thought we would try to look at it more from the point of view of model theory, think about structures, sort of more algebraic structures that way. That's what got us started, the influence from John Myhill and indirectly through Anil Nerode, who had just finished his thesis at Chicago. *[0:10:00]* That was really the genesis of the thing.

Now I went back and looked at our joint paper that was eventually published from the work that summer. When it comes to nondeterministic automata, which is one of the things that's always noted that we introduced then -- a nondeterministic automata is not a probabilistic automata. What it does is, when it's making a transition from one state to the next, it has many choices that it can make rather than a specific choice. So success in accepting a tape that the automaton is reading means that there is some path through the choices that eventually results in success. You don't have to worry about the paths that don't work out. You only have to find one successful one.

I don't remember how we thought of doing it, except maybe we kept coming into problems that it was difficult to create the states to do various kinds of decision questions. And so what we say in the paper is that nondeterministic automata are so much better because they require so many fewer states than deterministic automata. Of course, the proof that a nondeterministic automata accepts the same set of strings that a deterministic automata does is that you take the nondeterministic automata and to the take the power set of all the states and treat those as new states and define a transition function on sets of states. So of course the cardinality of the number of states goes up exponentially there. So the original nondeterministic automata has many fewer states than the resulting deterministic automata. Also, there are other reasons, that nondeterministic automata made it easier to prove closure conditions for families of sets accepted by the automaton.

But I don't remember why we thought of nondeterministic automata. All I can say at this late date is it somehow made life easier for us, and so we did that.

Then that summer, at the end of that summer was the National Science Foundation summer logic conference, six weeks in Cornell. That was the first big general meeting of logicians that had taken place in the United States. There'd been smaller meetings through the Association for Symbolic Logic, also sometimes in connection with the American Mathematical Society. But they got through Halmos and through Rosser, and they had made an application to the National Science Foundation and got the summer conference. Practically anybody who was anybody in logic came to that summer conference. It was a beautiful summer in Cornell. Rabin and I reported on our work on automata there, and then we together prepared the paper which was submitted the next year, next academic year to the IBM journal. That's roughly the story of that joint work at that time.

Plotkin: Thank you. Thank you very much. Do you remember at all how the work was received at the institute? I mean it's another "long ago" question.

No, no. I don't. I mean it was a sort of "theorem, proof, theorem, proof, theorem, proof" sort of thing. People agreed that the proofs were nice and clean. That was that. I don't recall any special enthusiasm about it at that time.

Of course the thing that later transpired was of course other people took up things, especially developing complexity theory, which made use of all kinds of work on automata by other people as well. So certainly the paper that we wrote had an influence on complexity theory that other people did. For example, Hartmanis was a leader there, to name one of them.

The other thing was that Rabin went on to think about probabilistic automata as a sort of natural generalization. Then much later he went on to think about infinitistic automata operating on trees and had very strong results there. But I didn't take part in any of those further developments myself.

Plotkin: But to make a start is incredibly important. Moving on, coming to your thesis work, Tarski, like Hilbert, was interested in axiomatizing geometry in terms of various geometrical primitives. Your thesis was on that subject. Why was it of interest to you? Why did you like that as a thesis topic?

Scott: Well, I think back from school, geometry was what got me into mathematics more than algebra. That interested me in… Well, I mean the thing that you learn in geometry is thinking up a proof. In algebra, and partly in calculus too, you have a lot of procedures which you apply somewhat mechanically in order to solve the problems. But in geometry, you really have to think about proof. That's why I like geometry very much.

Then when I was an undergraduate at Berkeley, Tarski's – through J. C. C. McKinsey, the associate of Tarski -- had written up Tarski's decision method for elementary algebra and geometry. I studied that very much.

Then also various questions came up about choosing primitives for geometry. Like Hilbert used points and lines and such things, and Tarski did in his original formulations there, but there are other ways to take geometric relationships that are sufficient to define all the first-order geometric operations. As an undergraduate I had thought about those things.

Then when I was at Princeton and was trying to think of a thesis topic, it occurred to me that there was a question that I hadn't seen addressed before. Namely, comparing geometry, elementary geometry where the basis for geometry are flat subspaces like points, lines, planes, etc., for higher dimensions, and the incidence properties of a point lying on a plane and so forth. That's the basic structure that determines the geometry. And I hadn't seen any comparison of the geometries at different dimensions. After I thought about it, I realized that many incidence theorems are true at all dimensions, and in fact I was able to establish – that's what became my thesis – that there is just one infinite-dimensional geometry. That is to say, as Tarski proved completeness of the geometric axioms, that in particular means that the theory is complete, *[0:20:00]* every theorem formulated with the primitives is either provable or disprovable, so it's a complete theory. So it turns out that the sequence of complete theories with different dimensions converges. There's only one infinite-dimensional theory. Those are the theorems that are eventually true from some point on.

That was what I established in my thesis. Church didn't have anything to do with the development of the mathematics there, but he very kindly corrected the spelling in my thesis. I can credit him for helping me in the presentation there. Of course, Church was very much concerned with presentation and editing and those kinds of things, so those were valuable lessons from him. But he didn't really take part in the understanding of the geometry there. But he was happy to accept it as a thesis topic.

Plotkin: Good. Thank you. So many interesting questions, but we need to move on, I guess.

Next one is this… well, it's a story that you might wish to tell and it's a technical paper. It's with Frayne and Morel, and that's about ultraproducts. That was later much use to you and much use to the field. Can you tell us about the story there? What happened?

Scott: Yes. At the summer logic conference, one afternoon where we were going somewhere with Tarski – and I'm sure Anne Morel was there too and some of the other graduate students of Tarski's – he brought up the observation that equational theories, if you… Now I have to think again how to say it. Equational theories have a nice feature from a model-theoretic point of view that if you take the direct product of algebraic structures, then equation is true in the direct product if and only if it's true in all the structures. So direct products sort of take the intersection of the equational theories. Tarski pointed out that results could be obtained when you took the direct product there, if you instead didn't say that it was true in all of the components but it was eventually true in… I mean say order components 1, 2, 3, 4, and say, "An equation is accepted if from some point on it's true in all of the components." That gives you other equational theories related to the ones you start with there. Rather than taking the intersection there, you take what you might sort of call “tail validity” eventually true there.

That seemed a very interesting observation of Tarski’s. So when I went back to Princeton in my last year then at Princeton, I thought very much about what happens with trying those kind of things with first-order structures rather than equational structures. Frayne and Morel back in Berkeley… Anne Morel was then on sabbatical in Berkeley, and she was working with Thomas Frayne. Independently, both of us came up with the fact that if you took products of first-order structures modulo a filter, then you could say how things were satisfied not individually in every possible component but only in those where the set of terms at which you’re thinking about are one of the sets in the filters. That means modulo what’s in the filter, you only think that something has to be true sufficiently often, not always true there.

It turned out that in Warsaw, Jerzy Łoś, a logician there in Warsaw, had formulated this idea in a different kind of a formulation that nobody in Berkeley understood at the time there. So it turned out that using the filtered products and eventually the ultrafiltered products was equivalent to things that Łoś had proved about it, but that only became clear somewhat later. But then I additionally proved that using this method there, one could prove compactness theorems about various kinds of satisfaction of theories using the ultraproduct construction, which was something that Frayne and Morel hadn’t done. But we were working independently there and then realized that we had had several similar results, starting with Tarski’s original observation about equational theories.

In the meantime, Simon Kochen was working on things that went back to Erdős and other people in analysis where they had thought of products of real numbers and things being true, not forever but eventually true there. Again, ultraproducts in a special case had been discovered by Erdős and people thinking about functional analysis, and Simon Kochen also worked on that. So those kind of strains all then came together in that year. That was in ’57-58. That was independently of my thesis work there, which I was writing up. But Kochen was thinking of the real closed fields from the functional analysis point of view, and Frayne and Morel were working under Tarski in Berkeley. So those things eventually all came together that way.

Plotkin: Okay. Dana, looking back over Princeton, over all these various things that happened, all the people you met, all the work you did, how do you think your time at Princeton influenced your approach to research?

Scott: Well, I think the key thing about Princeton is that it was really a center of the mathematical universe. So many famous people came through Princeton, and one heard them talk and met them. I mean the Princeton faculty was very, very strong too. I wish I’d taken more advantage of what I could have learned at Princeton. But it gave me another view of all the mathematical lights. Hirzebruch was very, very impressive. I was very sorry that I missed Hermann Weyl. He had passed away. Of course, Einstein had passed away before I got to Princeton. Von Neumann was in the hospital already when I got there and then died of cancer. So I missed seeing some of the very big historical figures there, but it was quite inspiring to see the kind of mathematicians that came through Princeton.

And of course the two key things that the Institute for Advanced Study did for me was introduce me to Kreisel, who we spoke about earlier, but also in the last year, there were many visitors… I can't remember which semester it was, but Church was away on sabbatical. *[0:30:00]* I think that was a little bit earlier. And Kleene came in place of Church and gave seminars and had many visitors too. So I got to know Kleene that way. I think that was in '56-57.

But then in '57-58, my last year there, Halmos was visiting the Institute for Advanced Study. Much, much earlier, he had been von Neumann's assistant, but he was at the University of Chicago then in '57-58. So we got to know each other. He was very interested in algebraic logic. Tarski had a certain kind of cylindric algebras version of algebraic logic, and Halmos had a competing version, polyadic algebras. Two of Church's students got very much concerned with polyadic algebras and worked with Halmos. But I got to know him that way. So it was his influence that got me an invitation to come as a beginning instructor, non-tenured instructor to the University of Chicago after my PhD in June of 1958. I went back to California. My mother had come out for the graduation and we drove back to California. And then I came to Chicago at the end of the summer to start up my job there. So it was Halmos's influence that was the reason that I came to Chicago. Of course Halmos was working on a book on Boolean algebras then, and I consulted very much with him on that book at the time in Chicago. So Halmos was a really key question for why I went to Chicago.

Plotkin: Also, I mean as well as Halmos, there were other people at Chicago that you knew and came to you. Can you tell us a little bit about them and their influence on you?

Scott: Well, it was a great pleasure to meet Marshall Stone. He had retired the previous year, but he was still very much around. And a very grand old man, a very special character. He is the most travelled person I've ever met. He is a person who knew a year in advance what hotel he was going to be staying at. One time I met him and I said, "Oh, I just read an interesting article about the Silk Road in Asia." He said, "Oh yes, we were there last year. We went across it by jeep." You couldn't mention any place in the world I think that he hadn't visited or planned to visit.

Other people there of course were Saunders Mac Lane, who became very important also in my life on many occasions. He was a major professor there at Chicago. Irving Kaplansky, the algebraist was very, very impressive, and I got to know him that way. Antoni Zygmund, the analyst, who wrote a marvelous book on complex function theory – I wish I had studied it more closely – he was teaching there too and had many students. It was a very, very lively atmosphere. A. Adrian Albert, the algebraist, was the chair of the department. So I went from a quite rich mathematical environment in Princeton to another rich mathematical environment at Chicago at that period.

Plotkin: You're making me jealous. Another environment was the infinitistic methods conference at Warsaw in 1959, which was I believe your first trip to Europe. That was presumably an experience in more than one way for you.

Scott: Yes, that was a very important experience of all the people that I met there. And of course it was influenced… By that time, I had had a rapprochement with Tarski, and Tarski was instrumental in getting me invited to come to that. Montague was at that conference. Bob Vaught, Tarski's student, was also at that conference. Then of course we met the Polish logicians and many other European logicians also at that conference. Kleene was there also as a key member.

But leading up to that was a very important thing for me in Chicago. Quite early when I arrived in Chicago in early '50-… fall of '58… Wait, I'm confusing on the year. When did I get my degree at…?

Plotkin: You came to Chicago in '58. I don't know when.

Scott: Oh yeah. No, the infinitistic methods conference was '59.

Plotkin: Yes.

Scott: We said '57.

Plotkin: Fall of '59.

Scott: We said '57, but it is '59. Or I may have been confused.

Anyway, early in coming to Chicago, I met Stanley Tennenbaum. Stanley Tennenbaum was a close friend of John Myhill and other people. He had come as an early entrant to Chicago. Chicago had a terrible idea that bright high school students should come in maybe their junior year to college to get an advance in education. It was a terrible idea because young people are not really ready to come into an adult environment like college, and the effect on Stanley was he never finished his undergraduate degree. He came to Chicago and took part in many things and met many people, but he never finished his undergraduate degree. And I've heard stories that other people who came as early entrants also were really emotionally badly affected by being pushed too early, too soon.

But Stanley was an amazing personality. He was married with three children. I got to be a part of the family very soon, very close to the family. And Stanley had known Raymond Smullyan very well, who had a long time in Chicago there.

Now Raymond Smullyan as a very young person in '39-40 or so was very much involved in music and studied the piano. And that's where he met my wife and her family, in San Francisco when he was very, very young. He had kept in touch with Irene Schreier and her mother and her mother's husband that she married in Chicago, and they had many, many friends in common there. So in my very last year at Princeton, Irene had been a contestant in a contest, a piano contest in New York, and somehow had done very, very poorly in that, and was very discouraged. Raymond was there in New York and they met up, and he said, "Let me take you to Princeton where I'm a graduate student, and I'll introduce you to Emil Artin, who was one of your father's closest friends." They had been *Privatdozents* in Hamburg in 1928-29 and had done work together. There's an Artin–Schreier paper on real closed fields that's very important, a connection there which then connected to the work of Tarski on the decision method for real numbers. So Raymond said, "I can introduce you to your father's best friend there from that time." Irene was conceived in Hamburg, but her father died from poor health just a month before she was born, *[0:40:00]* and then her mother went to the grandparents, his parents in Vienna, where Irene was born in Vienna, not in Hamburg.

So Irene was very keen to meet Artin. He was a very dramatic character. He was extremely thin, piercing eyes, and when she was introduced to him, he looked at her and said, "Tell me, do you share your father's unfortunate liking for Brahms?" And she said, "Yes. Yes, of course. Brahms is very important to me." Artin was only interested in Bach and contemporary music. He was quite musical himself and both of his sons I think were musical. But he didn't like Brahms, and so Irene was very impressed by that.

I met then Irene at the math department at Princeton on the stairway. You entered down on the ground floor and then the social room upstairs, you went up the stairs. That's where Artin held out during the tea time there, talking to people, and that's where Raymond was taking Irene to meet Artin, upstairs. So Irene and I were introduced very, very briefly on the stairway in Princeton there.

However, when I got to Chicago, it turned out that through the connection with Raymond Smullyan, Tarski… I mean Stanley Tennenbaum, knew very well Raymond, who was in Chicago quite often there. And so the connection between Raymond and the Schreiers introduced me again to Irene, and that's where we really came together and developed a relationship. We then met through relatives of… still relatives from the San Francisco years in the summer of ’59. That’s where we really became engaged. So then we planned to get married in the fall of ’59. We met in California, and then when we came back to Chicago, we planned to get married. But this conference in Warsaw came up in October of ’59, and so I left Irene with the problems of dissolving both our apartments and moving everything into the new apartment, and then was a week away in Warsaw, and then came back and we were immediately married just before my birthday in the second week of October there. But it was the connection through Stanley Tennenbaum that made it possible that we met up, and so that’s a key thing that totally influenced my life.

Of course, Tennenbaum and I thought a lot about logic. He was very keen on many things and there were many things we thought about together. In particular, it turned out that he came up with… he thought very, very much about Post’s problem, which was something very important in recursive function theory. But then in our discussions, he was thinking about using non-standard models somehow in trying to get a simpler proof of Post’s problem about intermediate degrees. But in thinking about non-standard models, it occurred to him that he could make an argument there to show that there are no computable non-standard models satisfying the laws of first-order arithmetic. Mostowski, whom I met in Warsaw, who had been Tarski’s student, was very chagrined that Tennenbaum came up with such a tidy proof of that because he would have loved to have proved that theorem. That’s still known today as “Tennenbaum’s theorem.”

We did a lot of other work together which unfortunately was lost because when I left Chicago in the end of the next academic year, Stanley was also leaving to take up a job and he had all of our papers with things that we had written down in a big box which was in the back of his car. But when he was visiting New York in connection with jobs, the car was broken into and that box of papers was stolen. I’m sure it was dumped within 10 minutes when people saw what it was. So a lot of the work we’d done together never saw the light of day, which is too bad.

But in any case, Stanley was a big influence on me because he was interested in so many things. One very good thing the friendship with him did was that he wanted to understand how Gödel had proved the independence… no, had proved the consistency – I’m sorry, getting mixed up – how Gödel had proved the consistency of the axiom of choice by introducing Gödel’s so-called constructible sets. Now I knew and understood what the result was, but I never paid really much attention to the proof. So it was explaining from Gödel’s monograph, explaining the proof of the… Gödel’s consistency proof, that was the first time that I really understood it and it was very important that I got to understand it that way. But it was I had to explain it to someone. So often you don’t understand something until you explain it to someone else. That was a major thing that happened to me because of Stanley Tennenbaum.

Plotkin: That's a great story. It's a pity you never published papers together, but still many good things happened.

After that, I guess then you moved back home so to speak to the origin, to Berkeley. How did that happen? How did you get from Chicago to Berkeley? Because you're already extracted Irene, so you didn't need to go back.

Scott: Well, no. We were married in Chicago and then we had… '59-60 we were together in an apartment there after our marriage near the University of Chicago. She was teaching at a music school on the North Shore of Chicago. So this was my second year as an instructor at Chicago, a non-tenured position. However, Chicago was one of the major mathematical centers of the country, and so when it came time to finish my term, two years as an instructor there, they decided not to hire me further there. So I had only the two-year appointment in Chicago.

Fortunately, earlier and of course both through AMS conferences and through the Warsaw conference, I had a full rapprochement with Tarski in the meantime. And so it turned out at the University of California in Berkeley, they had just started a new institute for both younger and older mathematicians. And so it was arranged that I could have a fellowship at Berkeley as a Miller Fellow, which would also then evolve into an appointment at the University of California. That opportunity to have a nice fellowship was then what brought me to Berkeley, and so we moved to Berkeley in the summer of 1960.

Plotkin: Turning to the research that you did in Berkeley, you got a famous paper, "Measurable cardinals and constructible sets." It sounds quite technical, but it's very important. Can you tell us why it's significant and how you came to think of the result? *[0:50:00]*

Scott: Well, it wasn't immediately then in 1960. It was a little bit later. I was three years at Berkeley before I went to Stanford.

Plotkin: Oh. Okay. Sorry, got it wrong.

Scott: So it was sort of the middle of that. But one of the things that happened was that there was enormous activity on using ultraproducts. So for example, a student came up from Southern California to work with Tarski, Jerry Keisler, Jerome Keisler. And he was very, very taken with things about ultraproducts. He proved dozens and dozens of theorems about ultraproducts and properties of them. He proved so many theorems about ultraproducts that Tom Frayne, who was still trying to get his PhD thesis, had nothing else to do. And Tom Frayne never got his PhD because Jerry Keisler and other people proved so many things about ultraproducts.

Another big area was large cardinals. Tarski and Erdős had much earlier proven a number of things about large cardinals. And that was also taken up very much. I mean Tarski's influence from foundations of set theory very much put in the fore the questions about behavior of large cardinals. And so any number of people worked on large cardinals -- some of Tarski's students. And then also there were a lot of connections with the ultraproducts, because ultrafilters are connected with large cardinals because you ask whether the ultrafilter is closed under infinitary intersections, not just finite intersections. And so there are a lot of problems about ultrafilters that connect up with properties of large cardinals.

Bill Hanf and I together introduce some things about undescribable[3] cardinals, very much larger cardinals than just measurable cardinals. But then in thinking about measurable cardinals, it occurred to me, "What happens if you really seriously look at ultraproducts over measures, coming from measurable cardinals with extra infinitary properties?" Earlier, Tarski had influenced any number of people in thinking about infinitistic logic, and so these infinitistic ultraproduct constructions were connected with things about infinitistic logic just as ordinary ultraproducts were connected with things about ordinary first-order logic. And so the generalization involving larger cardinals came up.

But then what I realized in thinking about… I mean it's also mentioned… the earlier questions about non-standard models there were also connected with ultraproducts, non-standard analysis that Abraham Robinson had taken up very, very strongly. In any case, in thinking about ultraproducts with infinitistic properties there, it occurred to me that if you took an ultraproduct ultrapower with a measurable cardinal of the model of set theory, then because their properties are non-standard numbers that we understood about submodels and supermodels… I mean non-standard analysis you could think of as an expansion of standard numbers, so it also would work the same way with sets and things like that, that ultraproducts would give you expansions. But then there would be inner models and outer models that you would get using these infinitistic ultraproducts there.

But Gödel had shown that the constructible sets were a minimal model, really. And so it would turn out then that if you use the measurable… problems of the measurable cardinal and the infinitistic ultrapower, then you would have a model for set theory where the constructible sets were contained in a submodel and the measurable cardinal was then outside of the submodel. When you put two and two together there, it turns out then that because of the minimal properties of the constructible sets, there could be no measurable cardinal inside the constructible sets because you showed that the constructible sets were much, much, much smaller than the measurable cardinal when you allowed for the measurable cardinal. So the measurable cardinal couldn't be inside the constructible sets because the argument, analogous to non-standard analysis, was that the measurable cardinal was *outside* of the constructible sets. So that's how I proved

[3] Current terminology is *indescribable cardinals*

that measurable cardinals contradicted *V* = *L*, Gödel's statement that all sets are constructible.

It turned out that a very brilliant young logician in Prague at the very same time found the same proof there, Vopěnka, and then he established a school in Prague with many, many very productive logicians coming out of Czechoslovakia at the time. We stumbled on the same result at the same time. My motivation came from Tarski's infinitistic logic and thinking of what kind of analogous to non-standard analysis would work out when you thought in terms of these infinitistic ultraproducts.

Plotkin: Thank you. That's fascinating. Do you feel like commenting on the further development of large cardinals, or…? You didn't take part in it yourself, but you no doubt observed it.

Scott: I don't have any special comments on it. No, no.

Session 3: January 12, 2021

Gordon Plotkin: This is Session 3 of the video interview with Dana Scott. Dana is in Berkeley, California, and I'm in Edinburgh, Scotland, and it's January the 12th, 2021.

Dana, when you were at Berkeley during this period, Rabin was also there, and you took the chance to work with him again, this time I believe in arithmetic. You also invented the famous notion of Scott sets in connection with nonstandard models of Peano arithmetic. There's a very interesting story there. Could you tell us please about your interaction with Rabin and your work on arithmetic?

Dana Scott: Rabin and his wife came to Berkeley for a one-year sabbatical. It turned out to be the only other time that I ever had a chance to work with him because he then after that went to Jerusalem and then to Harvard and back and forth, and we were never close together again, unfortunately. We had a very, very good time working together there. He discovered a new proof of Trakhtenbrot's theorem that the set of first-order sentences true in finite structures is not axiomatizable, not enumerable. And we had some other results together. But alas, that was my last working with him.

I do want to tell a story about him and his wife Ruthie, however. In those months, she was pregnant with their first daughter, and they went to a doctor in Berkeley and the doctor said, "I would like to take some X-rays." And they said, "Oh no, we don't want any X-rays. X-rays can cause mutations." The doctor said, "Well, you know, some mutations are good."

Plotkin: [laughs]

Scott: It didn't convince them. That baby turned out to be Tal, who's had a fantastic career of her own, fortunately without any additional mutations. So that's one of my memories about the Rabins.

There was quite a lot of activity there, many visitors coming and going in Berkeley. That was also the time that I worked with John Myhill. He was still at Stanford at that time, and he had come to give a talk about using definability with ordinal numbers in set theory. After I thought about it, I realized that ordinal definability is a very widespread notion. So he and I got together to write the paper about "Ordinal definability," which in particular shows that with using those sets that are hereditarily ordinal-definable, that is not only the set but all the elements and elements of elements of elements are ordinal-definable, those sets form a model for set theory that satisfies the axiom of choice. We were very pleased with that. It's not surprising that Gödel said, "Oh, I thought of that years ago. But my proof with the constructible sets was so much more important to get to the continuum hypothesis, I never told anyone about it." Oh well, that's what happens.

I can't remember all the details right at this moment, but I also thought about nonstandard models. Abraham Robinson was making great strides in nonstandard analysis, and so I had some small results along that line. That was another interest.

Plotkin: In a different direction, you mentioned once to me that you were party to early conversations about starting a CS department at Berkeley. Can you tell us about those conversations or those interactions?

Scott: Yes. Well, of course Harry Huskey had been at Berkeley for some time and had many students here, but he was more concerned with engineering aspects of computers. Everything was done in the electrical engineering department. But in 1957, a Belgian computer man had come to Berkeley, René De Vogelaere. He was a very, very enthusiastic proselytizer, and was particularly very strong about ALGOL and the use of ALGOL and that how ALGOL should really change programming. He introduced me to ALGOL and there was quite a lot of activity, seminars, and things like that trying to understand what might be the opening up of design of computer languages.

So the question came up, because in other places too there was an interest in having more concentration on study of use of computers, to have a separate computer science department in Berkeley. Also, Zadeh was here and he was very instrumental in trying to develop a new departmental structure. So I was involved in early discussions on that, which didn't quite gel at the time. However, because I left Berkeley only after 3-4 years here, they then established the EECS department quite soon after that. That was in the middle '60s when that happened, and of course it's become a very major department in computer science. But I'm afraid I didn't have much to contribute to the development of that department, even though I was involved in discussions about it at the time.

Plotkin: So the EE department was there initially and then that became the EECS department? Is that what happened?

Scott: Well, they're really two departments. Somehow… I mean Computer Science is a sister department to the pure electrical engineering department. I think they're still two separate departments.

Plotkin: Ah, okay. Well, as you said, you didn't stay. In fact, you moved… before much longer you moved from Berkeley to Stanford. How did that move occur? In 1963, I think.

Scott: Well, John Myhill, who had been at Stanford, decided to leave Stanford – this was after we had done our joint work together – so there was an opening at Stanford. Kreisel was much involved in Stanford. He came several months every year. He went back and forth between America and Europe. I was still very, very close to Kreisel at that time, and so he suggested that I come, move over to Stanford. There had been some unhappiness at Berkeley because, with the number of new appointments, the mathematics department was branching out and had many major appointments in various branches of mathematics, and there was a certain amount of animosity toward Tarski and logic because Tarski had pushed the development of logic so much. So there were some rather hard feelings against Tarski at that time. So, whether it was a foolish move or not, I decided to get away from that. So when Kreisel and Suppes at Stanford… Suppes of course had been a major professor there for some time. When they suggested that I move from Berkeley to Stanford. I decided to do that.

I had some students working under me at Berkeley. I'm not sure that all of them were ready to finish though, but I finished them up remotely after I moved to Stanford there. So there were some of the students from Berkeley *[0:10:00]* and then of course many new students at Stanford because the logic group there had expanded very, very much. Also, when Myhill was there, he was quite influential in getting people to come there. Then of course there were very interesting students who worked with me then at Stanford after I moved there.

Plotkin: Yes, you have some very famous students there. It would take a long time to go through all the students and everything they did.

Actually, you mention Pat Suppes, if I could move to Pat Suppes, because you had a real long association with him dating back to your first period at Berkeley a long time before that. You have a famous joint 1958 paper, "Foundational aspects of theories of measurement." That puzzled me a little bit in a sense because it contributes to mathematical psychology. It's not exactly logic. So I'm just curious about your association with…

Scott: Well, it was really concerned with a certain kind of model theory. I mean that was really influence of Suppes. I'd retained very close friendship with him over the years. I mean he was just a beginner at Stanford when Montague and I took his course at Berkeley in the mid '50s. But I kept in close contact with him and he very often was able to give me a summer job at Stanford working in his research group there, so we had been very close over the years. Now he was interested in mathematical psychology and the question about logical deduction, how could logic be applied to questions that might have some psychological interest. So measurement or understanding relations between different theories of measurement or different scales of measurement or how to connect those things was something that had been much of a concern to him.

That was how our joint paper arose in talking about in particular questions about relating measurement also to probability theory. It was his influence and I made some contribution so to speak really on the side of model theory, not on the side of applications to psychology.

Plotkin: Does that connect with your later interest in logic of probability, the work you did with Krauss?

Scott: Well, that really… No, that really came up from… Well, of course probability is a very important area and the connection with logic I mean is there from the very beginning of the subject. But what inspired that work with Peter Krauss, who was a student at Berkeley and then he finished up with me after I was at Stanford, really came from the paper… Now I'm going to block on names. Haim Gaifman, who's been for many years… he was from Israel, but for many years he's been a professor at Columbia. Haim Gaifman had a paper about assigning probabilities to logical formulae, in other words the logical formulae form a Boolean algebra, and you can think of having a probability measure on that Boolean algebra, and so he had some interesting results. So that was really the inspiration for the work with Peter Krauss, which resulted in his thesis, and our joint paper was expanding on from the original work of Gaifman.

Plotkin: Well, I need to read it, I think. Jumping around a bit, going back to an earlier theme of the origin of computer science departments, you were also present at the early days of computer science at Stanford. George Forsythe, Donald Knuth, and John McCarthy were all there, well-known people. A less known fact was that more briefly Barbara Liskov, another Turing Award winner, was there. Do you have any remembrances from those days?

Scott: Very much, yes. Well, you see, Forsythe had always been there. He was in math and there were other people in math, in numerical analysis, and they were beginning to feel that it was time that computer science had some independence. Really, the mathematics department at Stanford, which was very heavily oriented toward classical applied mathematics, was very happy to give away numerical analysis and other things to form the new department. I wasn't involved in the formation of the new department, but I knew everybody who was there. Very soon, Don Knuth moved over from Caltech to Stanford, and John McCarthy came and started up his AI institute, and I knew many people involved in that. Of course, I had known McCarthy before from the time that he and Minsky so to speak started their approach to AI in the '50s.

So I was very much a colleague of very, very many people there, though I wasn't directly involved in the computer science department. However, from the point of view of logic and automata theory, I gave some courses. Barbara Liskov was in one of my courses. It turned out to be a very lively group. There was lots of interest from many different students there. I remember her very fondly from there because she was such a bright light and of course has continued to be a major figure now in computer science. So that was where I first had any connections with her, that seminar that I gave at Stanford in the '60s.

Plotkin: Wow. So did you know McCarthy and Minsky already? Was that when you were in Princeton earlier? Is that how you got to know them?

Scott: I didn't know McCarthy all that very well, but I got to know Minsky very well. Minsky had a very interesting idea about register machines, and I proved some small results about that. Over the years, I had a very, very close connection with Marvin Minsky. I visited him many times in Cambridge, Massachusetts there. That friendship, yes, went back to the late 1950s.

Plotkin: Wow. Did that lead to your work, or I guess it would connect to your work with Bill Rounds in automata theory, which was also done in the Stanford period?

Scott: Well, I think it was really just the general development of automata theory, not particularly Minsky, though of course he's a major figure. Of course, Rabin had gone on to do many very fundamental things, not only probabilistic automata but also automata infinite trees. So we were quite inspired by those developments. And so that's how. I had two students in automata theory there that finished up while I was at Stanford.

Plotkin: Right. Jumping back to logic and set theory, so a major thing that happened in this period was Cohen, who was also at Stanford, introduced forcing to show the negation of Cantor's continuum hypothesis was consistent with set theory and thereby with Gödel's result showing that it was independent of set theory, which was a major result. Later, you and others showed how this could also be done using Boolean-valued models, and you were awarded a Leroy P. Steele Prize for that in 1972. That's a huge development and a huge story. Can you tell us something about that period and those times?

Scott: Cohen came from Chicago, and there were many people in Chicago, including Myhill and Nerode. And of course Saunders Mac Lane was a very major professor there. His original thesis was in logic from Göttingen. At the time that he got his PhD, very often people went to Germany to get a PhD rather than get a PhD in the United States, and so he was one of the late people who had his graduate work in that way. However, Saunders Mac Lane very soon transferred over to topology, *[0:20:00]* algebraic topology, and of course was very intimately involved with many major figures there.

Paul Cohen was a very, very brilliant student at Chicago and did major things in classical analysis. But he heard a lot about logic and was always rather irritated by the logicians because he felt that they claimed too much importance. He really looked down very much upon it. And he kept hearing about how there was the proof of the consistency of arithmetic done by proof theory in logic. He heard that from many people. Stanley Tennenbaum knew Cohen very well while he was working in Chicago. So when Cohen came to Stanford, there was a lot of logic going on there – Suppes of course in philosophy, Feferman in mathematics, and Kreisel loomed very large and was very much in view there.

Cohen especially liked to be able to do things on his own. For example, he proved Tarski's decision method for a real closed field his own way. I don't think he ever read any of the Tarski papers. He just did his own version himself. And he said, "I'm going to show the consistency of arithmetic," and so he really reinvented for himself, completely on his own, the method of showing consistency of first-order arithmetic. "And now," he said, "I'm going to show the consistency of second-order arithmetic." In thinking of second-order arithmetic, he had to consider quantifying, in effect quantifying over infinite sets or over functions, and so he had to think in terms of approximating functions a little bit at a time and giving some kind of relationship to force a function to have some properties just on the basis of a small amount of information. After he started getting that idea of forcing properties based on information, he then saw that he could extend that to not just to functions over the integers but to the transfinite realm as well.

And so that's how he invented forcing. Of course he was quite influenced by Gödel's work. He knew of course about constructible sets. In the beginning, you can understand it coming from the thinking about arithmetic through… He made very strong use of taking countable standard models of set theory. I mean by the Löwenheim–Skolem theorem, even a transfinite model has a first-order equivalent countable submodel. So he used countable standard models in connection with his forcing.

Of course, everyone was completely amazed by what he was doing. He was rather bitter at times that people criticized his work. Several people were afraid that he was making a mistake with his claims of showing the consistency of … the independence of the continuum hypothesis, that he might have made a mistake, and they raised various questions there. But of course in the end it turned out he hadn't made any mistake. But he was very annoyed by anyone ever questioning that he might be wrong about things.

All kinds of people were very interested in this. Bob Solovay had come after his PhD in Chicago, where of course he knew about Cohen and many other people, had come as a junior professor to Berkeley, and he was very, very interested in the Cohen developments. So he came to Stanford many times and we had many sessions with Cohen, with Feferman, Kreisel, Solovay, other people too, trying to understand Cohen's work.

One day, Bob Solovay said, "I've been thinking about the notion of forcing, and you can reinterpret what Cohen has been doing by saying he associates a Boolean value… By looking at the forcing conditions that force a sentence to be true, you can make those forcing conditions into an element in a Boolean algebra. And so," he said, "he's giving Boolean values… these forcing notions are giving Boolean values to statements about set theory."

Now this was just about… when he brought that up and explained how he connected that point of view with the techniques of forcing… was just about the New Year's break. My wife and I were living in San Francisco at that time, and when I went home over the holiday, I thought, "Wait. If Cohen via Solovay can give Boolean values to formulas, why don't I start with the Boolean algebra in the first place, which could be easily described, and think in terms of giving Boolean values to … using a Boolean algebra, a given Boolean algebra? Instead of doing Cohen forcing, you pick the right Boolean algebra and then you extend the Boolean algebra from being just a model of propositional calculus to being a model of not only first-order logic but higher-order logic by thinking of… the power set operation which generates the sets in set theory is just iterating two-valued combinations using two-valued logic. Why not use Boolean-valued logic to iterate things into the transfinite?"

That was how I saw an insight into starting with Boolean algebras first and then using that to interpret set theory, and of course referring to of course the kinds of things that Cohen did. In any case, it redoes his theorems in a different way for the independence of the continuum hypothesis. The way it comes about of course is that Boolean values, multi-valued logic can give you many more sets of integers than you had before. Instead of taking just two-valued sets of integers, if you take multi-valued sets of integers, you can have an awful lot of them, and that's how you get connected up with cardinal numbers. Of course, there are a lot of details to work out that way.

Of course, Cohen… I don't mean Cohen, I mean Solovay… immediately said, "Wait. I thought of that myself." So when it came time for the UCLA Set Theory Institute a couple of years later, we decided to make it a joint paper on Boolean-valued models there, because really the motivation came from Solovay and Solovay had really many insights there. So I was really auxiliary to the development with Solovay. That's how it came about.

Plotkin: Wow. That's a really interesting story. Thank you. I know that I was supposed to ask you about UCLA Set Theory Institute because that was an important meeting. But is there anything else you'd like to say about that? Or is it just simply the importance of being able to present Boolean-valued models there that was important to you? *[0:40:00]*

Scott: Well, it was the first time that all kinds of people came together. There was amazing interest in set theory and models of set theory. So it came at a very fortunate time that all kinds of people could get together to discuss these things.

One anecdote I'll tell about it is that Saul Kripke was going to be one of the featured speakers at UCLA at the big set theory meeting, and he was very late in arriving. I was one of the organizers on the organizing committee, and we got this telephone call. It was Saul Kripke. He said, "I'm at the airport but I don't have any money." I said, "Saul, Saul. It doesn't matter. The meeting is about to start. You've got to get here. I'll meet you at the curb and I'll pay the taxi. Don't worry about it." So Saul Kripke arrived and I picked up his bags and said, "Saul, we have to hurry to get you registered for this. It's the last minute." So walking through the hallways to get to the registration room, we passed Jerry Keisler, and Jerry Keisler said, "Hi, Saul. I see you got someone to carry your suitcases." Saul Kripke was, always has been amazing to get people to help him to do many things in life. So that's one of my favorite stories about him, that I was carrying his suitcases.

Plotkin: Wow.

Scott: But I just want to say something as the aftermath of the set theory conference. It was a very, very big conference and the publication of the proceedings of it was quite complicated. I'm afraid I slowed up the publication very much. It was finally published in two volumes. But the Scott–Solovay paper never appeared in the proceedings even though we gave several lectures during the time of the actual conference. The reason was when it came time to make the final paper – this was in '68-69 when I was on sabbatical in Amsterdam – Solovay came to visit me, but by this time dozens and dozens of people had proved so many important things about set theory, independence results in set theory of all kinds. Solovay wanted to make sure that everything was incorporated into the paper, and I'm sorry to say it was too much for me. I couldn't really synthesize all the things that Solovay wanted to put into the paper, and so I could never make the final version of it.

Fortunately, from the notes that we had prepared for UCLA, fairly soon after that John Bell wrote a very good book, starting with our lecture notes, wrote a very good book on Boolean-valued models for set theory that in effect is the Scott–Solovay paper but done by another party that way.[4] It's very helpful because it carried out things in the spirit that we had wanted to have it done and is available now for students if you want to look at that point of view.

Plotkin: Thank you. You did many different things, so the interview jumps around a little bit. But you mentioned Saul Kripke, so that made me think of tense logic and modal logic. And there were lots of people around at Stanford. It was a big interest of yours and people, famous people like

[4] John L. Bell. *Set theory: Boolean-valued models and independence proofs* (3rd. ed.). Oxford University Press, 2005

Arthur Prior on tense logic, Jaakko Hintikka, John Lemmon. Richard Montague we already came across. Can you tell me something of that area and developments in that area, your interactions there?

Scott: Well, I'd already learned about modal logic as an undergraduate, and Tarski and collaborators of his had also papers on modelling modal logic that I knew. But then with my friendship with Richard Montague at Berkeley as an undergraduate, he was very interested in what you might call today "philosophical logic." That is varied ways of thinking of logical methods to analysis of philosophical problems or questions about language. So my introduction to modal logic came that way.

By the time of the mid '60s, there were lots and lots of people in philosophy… You mentioned… Hintikka was a very strong one. Kripke had started as a teenager being interested in modal logic and invented Kripke models for modal logic, which turned out to be also in another form already invented by Tarski in the much earlier papers there. But people in philosophy didn't… Tarski's formulations were very much done in terms of algebras of operators on Boolean algebras, and so Kripke's approach with Kripke structures was much easier to understand for many people. As I say, he invented that when he was a teenager and started his career from that point of view. Of course, many people… Hintikka pursued it very, very strongly in many different directions. So questions about tense logic could be reformulated in terms of Kripke models, as many people saw. There was a lot of that in the air and there were many visitors who came to Stanford to do that.

I don't remember how I first met John Lemmon. He was working in Southern California. But he was a very congenial person and gave interesting lectures, and so I struck up a relationship with him, and that we wanted to do more of a textbook on modal logic using his work but also using things from the Boolean algebra side as well. So we had started to make that development. Krister Segerberg was a PhD student of mine in Stanford at that time, and so we had some notes that we put together.

But alas, John Lemmon had a weak heart, and one time he was on an outing to mountains near where he worked in Southern California for a picnic with people, and he just had a heart attack and died suddenly on this outing. That was terribly sad. It was terribly sad for… because he influenced so many people and students and colleagues. So I never finished trying to write the Scott–Lemmon textbook, but an abbreviated version of it was put together by Krister Segerberg, which was published. That's my connection with John Lemmon.[5]

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[5] Scott, D. and Lemmon, E. J. *An Introduction to Modal Logic*, (edited by K. Segerberg). American Philosophical Quarterly Monograph Series, No. 11, Oxford University Press, 1977, x + 96 pp.

Plotkin: Yeah. I enjoyed reading that book many years ago. I still remember it.

Actually, there's a connection with computer science, which is that Amir Pnueli, another Turing Award winner, won the 1996 Turing prize, and his citation begins, I read, "For seminal work introducing temporal logic into computing science," and it's part of a very long and practical development. Did you have any hint of those things in those days?

Scott: No. No, I didn't really. No, no. That was a different point of view. Of course, with all of those things, people want to take over things from logic, and so tense logic can be useful there. But of course those kind of developments are much, much more concerned with particular applications like proving properties of programs in terms of when time may be an essential thing. I mean there are many questions about time in the execution of programs, and so that's much, much more concerned with the implementation of computations on computers.

Plotkin: Yeah. There's so many connections, which makes things interesting.

Jumping back to logic, there's infinitary logic. That's another variety of logic. That was important to you and you worked on that with the Scott sentence and so on. Can you say a little about that interest of yours?

Scott: Well, that really came earlier. I mean Tarski had and various people, certainly Russians and others, had worked very much on infinitary logic, Tarski and his students. I learned about that at the time at Berkeley both as an undergraduate and later when I was there as a faculty member. Already at the Cornell conference, Tarski and I had a joint paper on infinitary logic. That was where my interest came from there, the developments from that. I think the things that you referred to are things that I did in the early '60s in Berkeley, later on, but the original influence came from Tarski's earlier work on infinitary logic.

Plotkin: Okay. Another Tarski point. He features often.

Well, this is going to be Tarski again perhaps. Intuitionistic constructive logic was another topic of importance to you in this period. There are links to Tarski and areas of great interest for questions on foundations of mathematics. What was the abiding fascination for you in this area, which continued… maybe it still continues?

Scott: Well, in connection with modelling of modal logic, Gödel had already pointed out that there was a connection between modal logic and intuitionistic logic, and Tarski had introduced the topological models for modal logic, which include topological models for intuitionistic logic. I mean roughly speaking there, if you think of a topological space, then the subsets of the space form a Boolean algebra, but if you take the interior operation, then the open subsets of the topological space form not a Boolean algebra but form a certain kind of distributive lattice. Tarski realized very early on the open sets form a model for Heyting algebras or intuitionistic logic. Of course, that's also connected very closely with what happens with Kripke models and things like that too. There are many connections there that people developed over the years.

So I knew about that work from the early days with Tarski and connections between Boolean algebras and algebras with operators, modal logic. But I really didn't get into understanding constructive reasoning until later. Of course, it was very strongly emphasized in the important textbook by Steve Kleene, *Introduction to Metamathematics*, both from the point of view of proof theory and from the point of view of realizability interpretations of intuitionistic logic. So when I came to Princeton and was very much then influenced by Kreisel, I really looked much, much more into the questions of intuitionistic logic from that point of view. Though I never became adept at doing things in proof theory, I certainly understood recursive function theory and realizability interpretations.

That was my connection, and of course I met various people connected with intuitionistic logic that way. That was really how I had my strongest interest in it, was from that point. But Tarski never taught about proof theory at Berkeley. Feferman, who was later strongly, very strongly influenced by Kreisel in his interests, really was the first of Tarski's students I think who really went back and read Hilbert, Bernays, and those kind of things, and then made major contributions to proof theory. But I didn't develop along those lines at all, even though I did understand quite a bit about constructive logic in the way that I just indicated.

Plotkin: Yeah. Thank you. There's all these connections between Boolean algebra and topological models. And continuing to jump around a little bit, Boolean-valued models came up again for you another time, but in yet another context, the context of nonstandard analysis. I'm very curious about that, whatever you have to say.

Scott: Well, if you look at ultraproducts, you can really think of ultraproduct as starting with a Boolean-valued model and then taking a homomorphism so to speak by using a filter on it. So you could really start with Boolean-valued logic and then look at trying to cut down the Boolean values by means of a filter. So those connections. Of course, as Abraham Robinson emphasized so strongly, he thought of getting his models for nonstandard analysis by using ultraproducts to get to the infinitesimals. So there was lots of connections there that brought the Boolean-valued models and nonstandard analysis together.

But on the other side, going back to Tarski's topological interpretation of intuitionistic logic, after the period of development of Boolean-valued models in set theory, it occurred to me that using the lattice of open subsets, instead of using a lattice as a complete Boolean algebra to make Boolean-valued models in classical logic, you could use the lattice of open subsets to models of intuitionistic analysis. So I did that. I thought of that after the period of the development of Boolean-valued models. That's how I thought of modelling intuitionistic analysis using the topological interpretation.

Plotkin: Thank you. We have to cover Amsterdam and de Bakker and so forth. Is that okay? We got a few minutes?

Scott: Yes, yes, yes.

Plotkin: Okay.

Scott: I was at Stanford for about six years, and we've covered some of the developments there. Of course, it was very lucky there were many excellent students there both for Feferman and myself, and many visitors came to Stanford. It was a very, very active time.

I was then eligible for a sabbatical in '68-69. I'd already met, I mean at various conferences and other places, many people from the Netherlands. So I decided it would be very nice to have a sabbatical in Amsterdam. I knew Anne Troelstra, Dirk van Dalen. I'd met Heyting. Heyting was just retired from his chair, and so the University of Amsterdam had money that they could have for a temporary visitor there. *[0:50:00]* So I agreed to come to Amsterdam where I could also do some teaching there.

That was also… I must have met Edsger Dijkstra during those things. But very strongly was -- in the computer science there in Amsterdam -- was Aad van Wijngaarden, who was a very big mover, especially in development of ALGOL and eventually in ALGOL 68, which was apparently an enormous effort on his part and a lot of battles with other people who had many different ideas about how ALGOL should develop. But ALGOL 68 finally was van Wijngaarden's baby. I got to know him and made friends with him then.

One of his students was Jaco de Bakker. There were others there too that I had a lot of connections with. De Bakker and I had very many interesting times together. So then in the summer of '69 was when I wrote up the notes for the work that de Bakker and I had done. That all developed during that sabbatical year, '68-69.

The summer of course was very, very important for me, because that was when the idea… Pat Suppes said I could come to the IFIP Working Group 2.2 where he was a member but he couldn't attend it that year. That was where I met Strachey and heard many, many things, questions about language design and all the arguments that were going on, especially with the development of ALGOL at that time. After hearing all of those things and seeing the approach that Strachey wanted to take, I felt very, very much attracted to that point of view of thinking of computer languages.

To backtrack a little bit, one thing that happened in Amsterdam is Donald Davidson, the philosopher, had been a long-term professor at Stanford, but in ’68 he left Stanford to go to Princeton. So he was at Princeton, and at the end of that academic year, he was on a trip to Europe and he came by in Amsterdam. So he recruited me to come to Princeton. He was directly responsible for me after my sabbatical leaving Stanford and going to Princeton. One of the things that was difficult in Stanford was that the mathematics department was very, very oriented toward classical analysis and was not really interested in helping with the development of logic. I was kind of split between mathematics and philosophy, but I felt in mathematics that logic was not especially welcome there. That’s how Donald Davidson influenced me to come to Princeton. Of course, I knew Princeton as having been a graduate student there, but that’s how I got to the philosophy department in Princeton as a faculty member.

However, having met Strachey and became so involved in thinking about the development of computer languages from those connections, I made a special plea that I could have my first semester on leave from Princeton so I could visit Oxford in order to do work with Strachey, and they reluctantly agreed to that. That was how I went to Oxford then, to work, to continue work with Strachey.

So the next time we meet, Gordon, we’ll pick up the story there with the time in summer of ’69 and starting, then coming to Oxford and working with Strachey. So we’ll pick up the story there.

Plotkin: **We will. Thank you.**

Session 4: February 18, 2021

Gordon Plotkin: So it's February the 18th, 2021. This is Part 4 of the interview with Dana Scott, Turing Award winner. Dana is in Berkeley. I'm Gordon Plotkin. I'm in Edinburgh. And we'll begin.

Dana, how did you become involved in computer science and start to work with de Bakker?

Dana Scott: Well, I think we covered some of it before. I had of course long experience with recursive function theory, and then it was 1957 that I worked with Rabin, and then 1958 I programmed on the Institute for Advanced [Study] machine… Institute machine that von Neumann built while the university still had it for a short period there. So I had some background in early, very, very early computers.

Then I'm a little bit confused about the exact date that we're talking about here, when I… During the postdoc period at Chicago, I really didn't have any intersection with development of computer departments or computer science. But when I got to Berkeley in 1960, things were just starting to develop there, and the key thing was that I was introduced to ALGOL 50, ALGOL 58 I guess at that time. Maybe it was ALGOL 60. I'm sorry. René De Vogelaere was a numerical analyst. He was very, very enthusiastic about ALGOL, and so he was doing a lot of proselytizing. Then there was some movement to get a computer science department started at Berkeley, different from the electrical engineering department, but that took some years to evolve and I wasn't directly concerned with that because I had moved to Stanford.

Then at Stanford, I lectured on automata theory and had connections with many people involved with the newly evolving computer science department that George Forsythe started at Stanford. Then of course that was the time that McCarthy came to Stanford and started his AI Lab as well. So I was connected with those people, though I was not directly working with anyone.

It was while I was at Stanford that I took a sabbatical in '68-69 that I went to Amsterdam and renewed friendships with many of the Dutch logicians. I lectured on set theory and on model theory. But in the spring, Pat Suppes had recommended me to join the IFIP Working Group 2.2. And as I wrote to you, Gordon, earlier, I looked up the history there. Apparently, it was in the spring that was the meeting that I went to where I first met Strachey.

But also in Amsterdam, I had got to know various… through van Wijngaarden, I got to know students there, and in particular Jaco de Bakker and I spent a lot of time together. Then in the summer of 1969 while we were on holiday, I wrote up the notes of the Scott–de Bakker approach, which I then lectured on when I went to Vienna at the end of the summer. And I think there must have been then very soon after that another meeting of W … 2.2. But that's over 70 years ago, or 60 years ago. I can't remember all the details of when I met people.

But it was because of the WG 2.2 meeting that I got to know Strachey, and so I decided that, after I had accepted the offer from Princeton, that I would ask for the first semester on leave so that I could work with Strachey. And that's how I went to Oxford to work with him at his Programming Research Group in the fall semester of 1969. Of course, there was a lot of activity. I also met Landin then, who was a close associate of Strachey. So that was how I got to have the connection with him directly there.

Plotkin: That's part of our main story. It's not perhaps a sideline, but another part is that you did move to Princeton. Could you tell us… From Stanford. We haven't covered how that came about.

Scott: I took the sabbatical from Stanford '68-69. And while we were in Amsterdam, Donald Davidson, the philosopher who was chair of the department at Princeton, came to Amsterdam and he recruited me to go to Princeton. I had known him very well of course from Stanford, where he was a long-term professor, but then he had moved to Princeton, I forget, '67-68, sometime around there. Then it was his influence on me that convinced me to move to Princeton, where of course I'd been as a graduate student. But then after I accepted the offer, Davidson much to my annoyance moved to Rockefeller, where he got a very big appointment at the Rockefeller University, where they were starting up various academic departments. So he wasn't at Princeton when I finally arrived in Princeton.

Plotkin: You mentioned to me that when you moved to Princeton, as was natural, you continued your association with Gödel. I don't know if you want to mention anything about that further part of your association with him.

Scott: Well, of course as a graduate student when I was connected with Georg Kreisel, who was very, very close to Gödel, I met Gödel many times. Then after I had gone to Berkeley, I wrote various papers in set theory, and Gödel had taken account of those papers. So we had those connections. But there was a period when he had been somewhat ill and he was afraid he was going possibly to die, and so he had a number of notes and things like that that he wanted to have preserved. So he contacted me in Princeton to help him try to preserve those notes.

One of them was his ontological proof of the existence of God. Then I very unfortunately at a seminar at the Princeton philosophy department spoke about the proof in modal logic that he had for the existence of God, and the one page of notes from that somehow got out into the outer world. So hundreds of people by now have commented on Gödel's proof in modal logic of the existence of God.

It's an interesting sidelight that my good colleague Christoph Benzmüller in Berlin worked very hard in implementing modal logic in the Isabelle theorem prover, and he showed that Gödel's original proof had a logical flaw in it, but the proof that I had given in the seminar using just my usual background in modal logic was a correct proof from the assumptions. So he has a very amusing lecture on that, updating the information on the ontological proof. *[0:10:00]* There's also quite a section in Gödel's *Collected Works* that Sol Feferman edited about the ontological proof.[6]

I would like never to hear about the ontological proof again. I don't accept it. My feeling is if you assume what you want to prove, you'll eventually prove it. So I really don't think that it's a conclusive proof. But I'll let other people decide that, and please don't ask me anymore about it.

Plotkin: Was that how you got interested in computer theorem proving? Perhaps not.

Scott: Well, no, no. The work of Christoph Benzmüller is much, much later, just since 2000 there. So that has no connection with it. It's just that he is an outstanding expert in various theorem-proving systems, and particularly Isabelle that was developed with Larry Paulson and the people in Munich. That's very recent history there.

Plotkin: I see. Okay, okay. Thank you. Cycling back, you went then to Oxford, with a semester leave to Oxford, and there you produced a spectacular series of papers, which laid I think the foundation – a lot of people do – for the modern scientific study of programming languages, including the needed underlying mathematics. One place to begin a discussion, which is perhaps the first of those papers, is your paper on LCF, which brought in the use of partially ordered sets in connection to work on recursion theory. Can you talk about this really important work?

Scott: Well, things leading up to that go back to some of the original researchers like Kleene and then Myhill and Shepherdson, and also Hartley Rogers and Friedberg, because they wrote about operators on spaces of recursive functions. In particular, both Myhill and Shepherdson and Rogers and Friedberg wrote about enumeration operators, and I knew about that work.

But a key thing that motivated me before I got to Strachey came from the thesis of Richard Platek at Stanford. Kleene had introduced, had gone on from ordinary computation to infinitary computation, because he was interested in descriptive

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[6] Christoph Benzmüller, Bruno Woltzenlogel Paleo. "Automating Gödel's Ontological Proof of God's Existence with Higher-order Automated Theorem Provers." In: ECAI 2014 (Torsten Schaub, Gerhard Friedrich, Barry O'Sullivan, eds.), IOS Press, Frontiers in Artificial Intelligence and Applications, vol. 263 (2014), pp. 93–98

set theory and higher-order operators. Richard Platek had transferred that into working on partially ordered spaces of functionals, and I was one of the advisors on his thesis, so I had had a lot of that there. Of course, much earlier Kleene had pointed out that if you take ordinary recursion and think of operators, they have a finitary principle that any single value of an operator on functions is obtained by only a finite amount of information about the input function that you're putting on. If you're thinking about operators say from partial functions to partial functions, there's a reduction to finite amounts of information. Of course, Kleene's advanced theory and Platek where you have to go up to a higher-order computation, that isn't true. It doesn't depend on a finite amount of information. It can depend on the whole function.

But I had that background in mind, and so after I met Strachey and found out that he was depending so much in a very formal way on type-free lambda calculus, I told him, "It would be much better if you thought about operators typed." In other words, you start with say partial functions, and then you think of functionals mapping partial functions to partial functions, and that formulation could be set up in a way analogous to what Platek was using for the infinitary computations.

So that's how I wrote that paper on LCF – to try to convince Strachey that it would be better to use for modelling; because it had a simple mathematical foundation there, that it would be better to use those monotone functionals. Of course, they should be monotone because you're taking as a computation, so as you find out more and more about your input functions, then you get more and more output about your output functions. So that monotonicity, which was similar to what Platek used in the higher-order cases, would be natural. So that's why I wrote that paper, that LCF paper.

But then in November of that year, I think it was a Saturday morning, I was lying on the bed in the guestroom of the flat that we rented in Oxford, I thought to myself that morning in November that "Wait a minute. I know for all these functions of functions of functionals of functionals that I wrote about in the LCF paper, that at each level there's a notion of finite amount of information, there's a countable basis for the functions such that every one of those functions of every type is the limit of the finite approximations to the things and that this idea of finite approximations passes on from one type to the next type when you take functionals over the previous type." And I thought to myself, "Wait a minute. This is very similar to what originally Cantor did for the rational numbers. The rational numbers as an ordered set can be thought of as the limit of finite ordered sets. As you subdivide things into smaller and smaller pieces, eventually you get the infinite set as the limit. Maybe there's a space of monotone functions that's the limit of all the spaces at the finite types, because the bases of things of finite amount of information would keep expanding and complicating itself, but in a monotone way so that it would pass to the limit." And so I realized that there must be a limit space, and then I worked out the details of that very shortly.

So I had to come to tell Strachey, "Oh no, look what happened. After all the criticisms I made of untyped lambda calculus, it turns out that there is a mathematical meaning to the untyped lambda calculus by thinking of a function space of infinite dimension." That's how I developed the idea of the D-infinity model, and then I lectured on that and many people came to hear about that in the late fall of 1969.

Plotkin: I'm curious, how did Strachey react?

Scott: Oh, he was very pleased, and he immediately adopted thinking of things in that way. There was no question. Because I mean he had lots of experience of utilizing lambda calculus in discussing properties of programs. But then this provided a model for the lambda calculus in the model based on principles of recursive function theory that were well understood. So he didn't have to think of it as an abstract formalistic trick. It really had a mathematical meaning.

Plotkin: Just cycling back to LCF, *[0:20:00]* **I just wanted to check something. I saw some correspondence between you and Robin Milner once. So you were helpful and influential in Robin's initial work on LCF, which became a major thing. Can you speak about your relationship with Robin at all?**

Scott: Well, he was at Stanford, and this is 10 years later.

Plotkin: Was it 10 years later? Oh, I see.

Scott: Yes. It was '78-79 when I was on sabbatical at Xerox PARC. Milner had been in Stanford for some time, and that was when he started thinking about the uses of LCF and making a connection with computer-based theorem proving. But there's a 10-year gap before he started that after the discovery of the original model. Of course, Milner was familiar with many things in the meantime, but I don't think he started on his theorem proving until '79, if my memory doesn't fail me. … Seventy-eight, seventy-… Probably it was '78 then. I was on sabbatical at Xerox PARC '78-79. I had gone there at the invitation of Jim Morris, who then eventually came back to Pittsburgh when I was at Carnegie Mellon. But at that time, at '78-79, I had gone there, to Xerox PARC, to work with Jim Morris, but he was trying to decide whether to become a manager and we never had any time to work together at the time.

What happened instead was that… See, this was in the period when I had left… After three years, I left Princeton to go to Oxford, because I had, completely out of the blue as far as I was concerned, the offer of the chair to be the first Professor of Mathematical Logic at Oxford. So in '72-73 about, I went to Oxford in hopes to work with Strachey. But then in '78-79, I took sabbatical from Oxford to come to California at the invitation of Jim Morris at Xerox PARC.

As I said, we didn't have any time to work together. However, while I had been at Oxford, I wrote up the papers on continuous lattices. You can think of it this way. The original D-infinity model was a very special case of a partially ordered set and a lattice that had some interesting properties. But it so happens in mathematics, if there's one interesting example of a structure, there must be lots of interesting examples of the structure. So I expanded the idea that included the model for lambda calculus to more general idea of partially ordered lattices, which I called "continuous lattices," because everything depended upon analysis of the topology of the lattices and the continuity of the function spaces, the use of the continuous functions in order to get the category of function spaces that was appropriate to those kind of structures.

After I wrote my paper on continuous lattices, I was contacted by mathematicians at Tulane University in Louisiana. Karl Hofmann was the main professor there, and he had various associates and students with him there. They had been working on topology and lattice theory for a very long time, and they realized that their category of lattices was the same as my category of lattices. And so that's how I connected up with the Karl Hofmann school.

Then at Xerox PARC, we put our work together, and I used the secretary and the computer at Xerox PARC in order to typeset this book with Hofmann and his associates, and myself as a co-author there.[7] I took their notes and my notes and other things, and we put together this book. I was then able to get Springer-Verlag to publish it, but they wouldn't publish it in a standard series. They published it as a special publication only because I'd had a so long association with Springer-Verlag. And so they very kindly decided to publish it, but they didn't put it in any of the standard series.

But Klaus Keimel, who's one of the authors there… Not long after that, Karl Hofmann went to Darmstadt. He was originally from Germany and so he had a call from Germany and decided to go back to Germany, and so he moved to Darmstadt where had the rest of his career, and Klaus Keimel was a faculty member there. So then at the end of the century, Klaus Keimel worked incredibly hard… the late, I'm sorry to the say the late Klaus Keimel worked terribly hard to update everything and make a new edition with many, many new results, and also he put in a gigantic bibliography of the development there. That was published in 2003 now by Cambridge University Press[8]. So the bibliography of this book will give you historical background of many things, not only from my work but also from the topological lattices point of view there. So I recommend that for tracing the history of development. And it's a testament to the very hard

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[7] Gierz, Gerhard, Karl Heinrich Hofmann, Klaus Keimel, Jimmie D. Lawson, Michael Mislove, and Dana S. Scott. A compendium of continuous lattices. Springer Science & Business Media,1980.
[8] Gerz, Gerhard, Karl Heinrich Hofmann, Klaus Keimel, Jimmie D. Lawson, Michael Mislove, and Dana S. Scott (2003). Continuous Lattices and Domains (Encyclopedia of Mathematics and its Applications, Vol 93). Cambridge: Cambridge University Press. doi:10.1017/CBO9780511542725

work that Klaus Keimel did in order to preserve and update the development of the ideas.

Plotkin: **Yeah, absolutely. Klaus was a wonderful man.**

Scott: Also, he spent a lot of time with the Domains workshop. I hope someone will take up after the pandemic is over the Domains workshops – there were 13 or 14 of them, I don't remember now – that we'll have get-togethers again once we can travel.

Plotkin: **So, there's many threads to take up in that story. One last thread though is you talk of lattices, but eventually people began to think things should be more general than lattices, and in particular you invented things which I think are called "Scott domains." Can you say a little there? How did you come to that invention?**

Scott: No, no, no. That's a direct outgrowth of the original, our construction. Those are… Well, you don't necessarily have to have a lattice with a top. You can have just a lower part of a lattice, a semilattice, which is so to speak half of a complete lattice. So it turned that using the top element in computability theory – it's a kind of idea of a breakdown or inconsistency or something – really isn't very appropriate for understanding how you would compute with these operators. So maybe the Scott domains are just what you get by taking the original domain theory, which is lattices, and taking only the lower half of it so that you don't have to fool around with the top, which doesn't have very natural interpretations when you think of any kind of compiling or operational activities with the things that you're computing with. That's a probably incomprehensible explanation, but it was the elimination of the top element that led to the other domain theory. So of course that's included and that's fully mentioned in the Keimel biography and group there. Background on that is fully covered in that.

What I should mention *[0:30:00]* was that Strachey, while I was at Princeton in the early '70s, he came for a visit, and so we finished our paper on "mathematical semantics," as we called them. It was only a little bit later that it seemed better to say "denotational semantics" to distinguish more clearly from axiomatic semantics of the kind that Tony Hoare was promoting. And of course to distinguish it from operational semantics that you promoted so very, very strongly. In fact, you pretty well with your Pisa[9] notes eliminated people working on denotational semantics for a long time because it was more important to use the implementational ideas that you put into operational semantics in order to get results. Of course, I would say today axiomatic, denotational, operational semantics all meld together, and the question is to take which aspects of which

[9] Gordon Plotkin corrects this, indicating it was actually his Aarhus notes on structural operational semantics that Scott is referring to.

you want to do for an analysis or a proof or for giving the foundations for some kind of implementation. You choose what is appropriate for the particular thing you want to accomplish.

Plotkin: Yeah, absolutely. I just want to perhaps emph-… maybe just make a point that is too obvious for you to say, but in doing the mathematical/denotational semantics of programming languages, that was a kind of continuation or extension of Tarski's ideas on logic and, in a different direction, Montague's ideas on natural language. So with that, you really completed the idea of giving semantics to languages artificial or natural. I think that's a very important thing to have done. Did you see yourself as working in such a way, or was it just a technical problem?

Scott: No, no. No, I think it was… it absolutely does go back to Tarski for having the need to have semantics. You see, Gödel himself after Tarski's definition of truth said, "Oh, he knew that it was obvious anyway," but it was Tarski trying to make it how to have general theories of semantics which also then carried over to other kinds of logic, modal logic and so on.

Plotkin: Okay. Now a trivial question. I'm just curious if you know the answer or what the answer might be. As you said, you were the very first Professor of Mathematical Logic at Oxford. There's a huge tradition of Oxford, a huge tradition of logic going back goodness knows how many years in Britain. Do you have any idea why they got around to having a chair in logic at Oxford? What happened there?

Scott: Well, Michael Dummett, the philosopher of course who is very, very important in the history of logic – again, the late Michael Dummett, alas – but also at Merton College, John Lucas, the philosopher who also was very much concerned with logic, and there was a need felt for teaching of formal logic. There were lots of philosophers who spoke about philosophical logic over many, many decades, but there was a feeling that there was a need for teaching of formal logic there. So I'm sure it was Lucas and Dummett who proposed the idea that there should be a professorship. That's eventually the chair that I went to.

Of course, I accepted the chair to work with Strachey. But at the same time that I came there, Strachey was given a personal chair, not an established chair, but a personal chair just for him in "computing," as he called it. He didn't like the phrase "computer science." He said, "I do 'computing.'" So he was Professor of Computing there. But it turned out, much to my surprise, that Oxford is ruled by its academics through an infinite number of committee meetings. They don't have administrators the same way that universities in states have. The academic community really pretty much governs itself through all kinds of boards and committees. So Strachey had to be on many committees, I had to be on many committees. Gandy, who was a reader there, had built up a lot of teaching on the mathematics side in logic and recursive function theory, so there were lots and lots of students fortunately to supervise.

But it meant both for Strachey starting a new department and my coming into being both a professor of mathematics and a professor of philosophy, there was all the administrative and supervision work to do, and Strachey and I never had a long period to work together again. Unfortunately, he became ill with a liver ailment and died in about 1975 or so, very sadly. So I didn't have any chance to work closely with him again there. Of course, there were various associates and graduate students around him, and there was quite a lot of activity. But it was sad that I really didn't have the close collaboration with Strachey again. After his death, the university decided to have an established chair in computing, eventually turned into "computer science," and the first one to be appointed to that was Tony Hoare. So Tony Hoare came from Ireland, Belfast, to Oxford then after Strachey's death when that appointment was made.

Plotkin: Yes, that was very sad. You mentioned Robin Gandy in passing. He's an important figure. He was Turing's only PhD student, if I understood correctly. I understand you were a close colleague of Gandy's, or Robin's.

Scott: Yes, over many years. Even before my coming to Oxford, I knew him. Of course, all the time that I was at Oxford, that decade, absolutely.

Plotkin: Yeah. Also a wonderful man. A slight… Well, going back to logic just a little bit, another interest of yours in Oxford… well, it was a continuing interest of yours, in intuitionistic logic and sheaf models, and you started a Peripatetic Seminar which, I counted, has now over a hundred editions. It would be 106 if it wasn't for the pandemic. How did this renewed interest come about and in which way did the sheaf theoretical approach present a significant new opportunity for you?

Scott: Well, I had lots of interchanges and colleagues and things connected with category theory. And so it happened that in the UK there were a number of different groups – Cambridge, Sussex, other places – that were interested in the connections between category theory and logic, and students at Oxford that were working with Gandy and me. A principal one is Mike Foreman, your colleague now in Edinburgh. While he was a student there, we became very taken up with the questions and the things that people were raising connecting category theory and logic. So it was informal connections between people in various places that started the Peripatetic Seminar. We called it "Peripatetic" because it was informally organized, everybody on his own funds. They just got together once a term or so in order to have these meetings to exchange ideas. It was also very, very helpful to students in various places to have a place *[0:40:00]* to present ideas and to give talks and to meet people. And so it became a very popular thing to do.

Then the group decided that it would be good to have a summer conference, so we applied for support from the government to have the conference in Durham. So that arose from our connections with each other in the UK at the Peripatetic Seminar. Then we had the big conference in Durham that brought people from all over the world and was a very successful conference.

You mentioned the other thing. It's the connection with intuitionistic logic, because the Lawvere–Tierney idea of topos theory, which came from topological considerations, led to logical notions of higher types that were intuitionistic, not classical in the sense of the law of the excluded middle. So that was what brought up intuitionistic logic through topos theory. But going back to Tarski, Gödel, other people, modal logic led to interpretations of intuitionistic logic, and so it was quite clear that models that people had thought of in connection with modal logic and what people thought of in terms of intuitionistic logic… of topos theory, made that connection with intuitionistic logic, and models from one side or the other were appropriate for discussion there. The strands there from modal logic and from category theory melded then through the impetus of studying topos theory.

Plotkin: Yeah. It was a vast unification. One connection there to computer science, talking about category theory, was you looked at the connection between category theory and type theory. Also, Lambek was involved in that. And that's another very important connection which completes the "propositions as types" connection, it makes another connection. How did that come about for you?

Scott: Well, it was just natural to do. I mean types are already there in computability theory. I mean there are… the connections with intuitionistic logic and the realizability interpretation that Kleene originally put forward also connect. So people like Martin Hyland and collaborators that he had made the connection between the category theory and the realizability interpretation there. So all of those things came together through trying to explain on a higher level there in the style of category theory as to what were the assumptions necessary in order to have the broadest view of what was going on.

Plotkin: I see.

Scott: So it was quite a natural outgrowth.

Plotkin: Yeah. Oh, that's a very nice way of thinking about it. Thank you. Oxford has a huge number of major figures in the UK intellectual landscape quite generally, and I knew you know, or knew, Michael Atiyah, Isaiah Berlin, Michael Dummett, Roger Penrose, all kinds of people. Do you have any particular recollections that come to mind of any of those people?

Scott: I'll only tell one story about Isaiah Berlin. He was a wonderful storyteller. My wife and I had a delightful experience that we were coming back to Oxford and met him on the train platform and rode with him in the compartment, and he told story after story. It was a delightful hour with him. I felt that sometimes his stories were expanded by what I would call "creative remembering." But it was really delightful.

But then there was a certain period when the BBC made a big program of English philosophers. It was a movie and was given at the movie theatre in Oxford there. And one of the sections was devoted to Isaiah Berlin. Now he had a rather thick accent. I think he's originally Lithuanian, is that correct? In any case, he came from that side of Europe. He had quite a thick accent. And he was interviewed, this BBC program in the movie, and we bumped into him coming out of the movies. He said, "Oh, I couldn't understand a word I said! I couldn't understand a word I said!"

Plotkin: [laughs]

Scott: I have very, very fond memories of him, and of course there are many other exceptional characters there.

I have to say about comparing Oxford to the States, of course it's in a way unique to Cambridge and Oxford, the college system. It would be hard to invent a college system that developed over centuries there. But the colleges were clubs. The thing that I missed on leaving Oxford, it was not the committees. It was the college, because at the various college lunches and dinners and so on, you met the most interesting people from all possible subjects, and I had so many interesting conversations at Merton College. Merton College was a very, very fine atmosphere. And I would say that was the key thing that I miss, and still miss, from the time in Oxford in the '70s.

Plotkin: Yeah, thank you. Dana, coming to the end, as a final question which has a very "final question" type aspect to it, you've contributed… well, we've seen in these talks that you've contributed hugely, very widely over a period of almost 50 years. So that's a long time, a lot of experience. And I was just wondering what your thoughts were about anything really looking backwards or looking forwards, what you might wish to say.

Scott: I would say that the contributions I made were very much motivated by the teaching and the great luck I had of really excellent students, many of whom became very, very close personal family friends. It was the inspiration of the students that really motivated me.

That's why I'm absolutely furious with Brexit, which is breaking up the interchange of students between Europe and the UK. As we speak here, it's already reported today that there are bad things happening to Scotland on account of the Brexit arrangements there. Students can get interests for many different reasons, but then they have to move around to get to the places where the right teachers are there. So having the possibility of students is extremely important. I remember very strongly Mrs. Thatcher in the ’70s when she instituted full fees for foreign students coming to the UK. Many departments closed down because more than half of their graduate students came from other countries.

The free mobility of students is absolutely important, and I was extremely lucky just by historical accident that I was in places where I had many, many students. So whatever I did, I really think I attribute it to their inspiration. We could work on problems together. And I miss that very much now in retirement, because once you retire you become sort of a ghost, and I really wish I had some students to work with even at the present day.

Plotkin: Thank you. I almost wish to say nothing, because what you said shouldn’t be followed. But just let me take a moment just to thank you for your patience and your time for these interviews. *[0:50:00]*

Scott: Gordon, I thank you for all the work you’ve done and I thank you for this decades-long friendship that we’ve had.

Plotkin: It will continue.

Scott: Thank you. Goodbye.

[end of recording]