Outstanding Contributions to Logic 13

Gerhard Jäger
Wilfried Sieg *Editors*

Feferman on Foundations

Logic, Mathematics, Philosophy

Springer

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Volume 13

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Foreword

Sol Feferman and I have quite similar backgrounds. We were both born in the Bronx in 1928 to East European Jewish parents. As teenagers we had similar interests: we both thought of theoretical physics as a likely professional goal, and we both read the same science fiction authors as well as the same popularizers James Jeans and Arthur Eddington. And we both moved from physics to mathematics and indeed to logic and foundations.

I was at the 5-week Institute for Logic at Cornell during the summer of 1957 that Sol wrote about in his autobiography. I do not remember whether I heard Sol's talk there about his dissertation on the arithmetization of metamathematics, but I certainly read the manuscript and was delighted at the clarity of the exposition, which, in particular, eliminated the penumbra of vagueness about the concept of a formula expressing the consistency of a formal system. Sol was part of what some of us thought of as Tarski's cohort. Tarski had been allocated an afternoon speaker slot every day for the remarkable Berkeley logic group he had developed. His habit of commenting aggressively on talks by non-Berkeley speakers did not always go over well, and there was some tension in the air. But all in all, it was an exciting, stimulating event and an important influential experience for all of us.

As Sol mentioned in his autobiography, during the academic year 1959–1960, he and I were regular attendees at the logic seminar at Princeton that Church led. I particularly enjoyed Sol's talks on progressions of theories. Sol's work was based on and extended Turing's *Ordinal Logic* paper which I had found quite difficult. I admired the clarity and rigor of Sol's exposition with his previous work on the arithmetization of metamathematics in the background.

Although I frequently met Sol at conferences over the years, my next professional interaction occurred in connection with the publication of Gödel's "Collected Works". Sol was the chief editor of this daunting project which resulted in five meticulously produced volumes with introductions by experts for each article. Sol asked me to write the introduction for a manuscript found in Gödel's Nachlass. The third volume consists of such previously unpublished work, following the first two that cover his published contributions. The fourth and fifth volumes are devoted to

Gödel's correspondence. The superb result of this undertaking is a tribute to the care and precision to be found in all of Sol's endeavors.

This same insistence on precision and rigor may not be unrelated to Sol's well-known agnosticism regarding the concepts of set theory. This skeptical attitude is perhaps most directly evident in his characterization of the continuum hypothesis (which after all only asks whether there is an uncountable set of real numbers which cannot be mapped one–one to the set of all real numbers) as "inherently vague".

I was on a panel with Sol on the future of logic in Padua in 1988. As I recall, I spoke of possible connections of Gödel incompleteness with mathematical practice. Sol took a much more practical point of view, speaking of problems one might suggest to a student with some reasonable hope for success. In fact, Sol has been quite openly skeptical about the need for axioms going beyond Zermelo–Fraenkel to decide important mathematical questions. Citing the success with Fermat's Last Theorem, he stressed the need to "try harder".

Sol was instrumental in making Stanford University a world center for proof theory. His autobiography mentions that Georg Kreisel, who had already done important work in this area, was a "second mentor" for him. Sol admired Hermann Weyl's predicative development of classical analysis and worked on extending it. His determining the proof-theoretic ordinal of the resulting system was a major achievement. Not completely satisfied because of the ramified character of the extended system, Sol found an equivalent unramified system. Despite his admiration for Weyl's system, Sol insisted that he was not a predicativist, and indeed he (necessarily) used non-predicative methods for obtaining the proof-theoretic ordinal of such systems.

Grigori Mints was an expert in Hilbert's ε-calculus and the ε-substitution method in proof theory. Educated in Leningrad, he applied to leave the Soviet Union because of the pervasive antisemitism that had become endemic in Russian mathematics. Subsequently denied employment, he moved to Estonia when the Soviet Union dissolved. Sol was determined to have him for Stanford. I was a very small part of this effort, writing a letter in support when Sol asked me to do so. Sol's effort was successful, and "Grisha" became a vibrant part of the Stanford logic community.

Sol's skepticism regarding set theory did not lead him to question the use of Grothendieck universes in Wiles's proof of Fermat's Last Theorem. Indeed, he considered even larger structures to provide a set-theoretic foundation for category theory and has expressed confidence in the consistency of the Zermelo–Fraenkel axioms based on the iterative concept of set. In Koellner's essay in this volume, he presents Sol with a challenge: can he really coherently hold, together with his set-theoretic skepticism, his acceptance of the natural number concept as sufficiently clear that every sentence of arithmetic can be said to have a definite truth value?

Alas Koellner's challenge will remain unanswered, and this volume is published with a huge lacuna because of Sol's death. As set out, the plan for this series would have had Sol provide his own comments on the various essays making up the book. But this is not to be. Instead of a full autobiography, we have but an initial fragment. And we will never know what Sol would have written to explain his set-theoretic views. I myself would love to know what he thought about projective determinacy and the picture of the projective set hierarchy that emerges from assuming it.

The depth and breadth of Sol's work are reflected in the variety of topics found in this volume. His influence over the course of his long career on directions taken in foundational investigations has been immense. He has had a number of outstanding students who have been making their own valuable contributions to the field. Sol's clear precise voice is deeply missed.

Berkeley, USA Martin Davis

Martin Davis was born in 1928. He studied with Emil Post at City College in New York and with Alonzo Church in Princeton. He is known for work on automated deduction and on Hilbert's Tenth Problem. His book "Computability & Unsolvability" has been called "one of the few real classics in computer science." Davis is a Professor Emeritus at New York University. He and his wife of 66 years now live near the campus of the University of California at Berkeley where he is a Visiting Scholar. Davis's book "The Universal Computer: The Road from Leibniz to Turing," intended for a general audience, is about to appear in a third updated edition.

Preface

On January 27, 2014, we received this e-mail from our teacher, mentor, colleague and friend Solomon Feferman:

> Dear Gerhard and Wilfried,
>
> I am forwarding a message below from Sven Ove Hansson proposing a volume devoted to my contributions to logic for a series that he is editing for Springer, along with my (tentatively) positive response. The attraction is obvious but, I must say, I have a couple of reservations about proceeding with this, first because the overall character of my work is somewhat different from that of the others that are already out or lined up for the series. (Though perhaps my addition would signal a shift in that direction to include some other obvious senior choices not mentioned by him.) The second reservation I have is that the series is being published by Springer, and I am afraid of it getting lost in their impersonal sea of volumes. I would be very interested to hear your thoughts about this.
>
> Hansson was fully in agreement with my suggestion to have two editors if we proceed with this, and of course both your names were the first that came to my mind. It would mean a great deal to me if you saw your way to accepting. I realize that that would mean for each of you taking on an extra burden that should not be considered at all lightly. But if you agreed, I would help in any way I could, for example by organizing my work into a number of useful categories and suggesting authors. I myself would be responsible for providing a full bibliography (no problem about that), an autobiography, and–later–responses to individual chapters, as Hansson suggested in a further message. (The whole organization of these volumes is reminiscent of the *Philosopher of the Century* series, the closest being the one for Hintikka.) Also, given my age, the sooner we could work together on this if you are willing to go ahead, the better.
>
> I hope to hear from you soon, but at the same time want you to take your time that it all deserves.
>
> Warmest best wishes,
> Sol

By the very beginning of February both of us "were in"—with great enthusiasm. We turned our attention to "substantive questions"; WS formulated three in a note to Sol on February 3.

> The first question seems to be: Is the volume to be systematically organized (according to major categories of your work)? The second crucial question: Who is going to (be invited to) contribute? A third very practical issue: What is the timeline for our work?

WS had forgotten one prior question, namely, what should the title be for the volume?—That was very important to Sol.

The three of us had quite a bit of e-mail exchange concerning the forgotten question and, in particular, also concerning the systematic structure of the volume that was indeed to be shaped by the major categories of Sol's work. In any event, on March 23 we sent a tentative table of contents to Sven Ove Hansson and suggested with Sol's full approval "Feferman on Foundations" as the title for our volume with the extension "Logic, Mathematics, Philosophy". The table of contents underwent changes in the subsequent weeks until Sol was really happy with it. The title, in contrast, had obviously been fixed forever. By the beginning of July, all the contractual matters were settled with Springer, and we wrote to potential contributors. It was wonderful for us and extremely gratifying for Sol that almost all potential contributors turned themselves very quickly into real ones. The self-imposed deadline for getting all the contributions was the end of March 2015; we hoped that we would complete the volume by the end of 2015. As usual, circumstances were in many different ways extremely challenging. On September 5, 2015, we wrote to Sven Ove answering his inquiry concerning the status of FoF:

> Dear Sven Ove,
>
> Thanks for your note. As to the title of the book, it is simply to be: "Feferman on Foundations". We have made progress (or rather, the contributors to the volume have made progress). We have some papers in hand, have requests by some to extend the deadline to the end of this calendar year, and don't know the status of some. We intend to write a brief note to our colleagues asking them about progress and likely completion date. When we have heard from everyone, we'll write to you again.

Here we are in December of 2016 more than a year later, expecting the final version of all the contributions to arrive within the next couple of days. We hope to submit the volume to Springer by the beginning of next year, i.e., January 2017 almost exactly 3 years after we received Sol's note asking us to serve as editors of a volume on his contributions to logic.[1]

In the PS to his note from January 2014, Sol mentioned that we would find as an attachment "a short autobiographical fragment" he had just finished for a volume in honor of Leon Henkin; that fragment was to be the starting point for the autobiography he had agreed to write for this volume. He had been working on it again at the beginning of 2016 and reached the mid-1980s, with quite a bit of his life's work still to be covered. The much-expanded autobiographical fragment was to remain, however, a fragment. After a difficult trip to New York in April, where he participated in a Workshop at Columbia to honor Charles Parsons, he was diagnosed as

[1] This note was obviously written in December of 2016. One year later, the volume is nearing completion and should be published at the very beginning of 2018.

having had a "mild stroke": Sol was hospitalized, underwent some rehabilitation, and finally returned home. His health deteriorated and he died on July 25. We are still mourning his death: the loss of a great logician, a thoughtful scholar, a man of integrity, and a dear friend. This volume is a testimony to him.

Bern, Switzerland Gerhard Jäger
Pittsburgh, USA Wilfried Sieg

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Introduction: Solomon Feferman's Autobiography from 1928 to 1981 and Extensions

In 2014, Solomon Feferman began drafting an autobiography to be included in this volume. The draft built on two earlier biographical essays, namely, *A fortuitous year with Leon Henkin* and *Philosophy of mathematics, 5 questions*. Sol used as a title for the draft, *An Intellectual (mostly) Autobiography*. Indeed, it gives a detailed account of his intellectual development and his professional work, but it covers also key events in his personal life. The draft is quite polished, but Sol never completed it: at the time of his death, he had traced developments through the very early 1980s. This partial autobiography is presented first as *Part A*. The next part, *Part B*, contains his CV with important milestones, accomplishments, and honors; it also has a full list of his Ph.D. students.

The expanding range of topics for his research can be gleaned from his bibliography that he had prepared for this volume early on.[2] The latter indicates also the great number of his collaborators. Sol, of course, interacted with many colleagues in the departments of mathematics, philosophy, and computer science. During the time of Sol's work at Stanford, the university evolved into a world center for mathematical logic and the foundations of mathematics. The list of associated Stanford colleagues and of short and long-term visitors we are aware of is extremely impressive; but we decided not to construct it, as it would most likely be incomplete and, in addition, it would require judgments about the significance of interactions we are not confident in making.

[2]Indeed, he sent the bibliography to Jäger and Sieg on August 12, 2015 with this note: "Dear Gerhard and Wilfried, Attached is the latest version of my list of publications for FoF. Some items have been added and I have uniformized the format of the citations and expanded the publication information in various ways. I do not plan to do any more work on this until we are closer to the volume publication. So I think it could be useful to the authors of the individual chapters and would be happy to have you circulate it to them now." He added the remark: "The work on my autobiography is progressing slowly. I have only 60 years to go."

However, we did try to indicate the projects Sol was actively engaged in at the beginning of 2016; there was, first of all, his work on this very volume: the autobiography was to be completed during the summer of 2016; after that, Sol intended to respond to the individual contributions to *Feferman on Foundations* in the fall of the same year. In *Part C* we list the other projects Sol was pursuing during that period. (The photo below was taken by Sommer around 2004 on the balcony of the Library of the Mathematics Department.)[3]

Pittsbursh, USA Wilfried Sieg
Stanford, USA Rick Sommer

[3]We thank Julie Feferman Perez and Ivano Caponigro for their support and detailed, helpful information. We have not edited the text, except for correcting obvious mis-spellings and completing some references. Thus, it is the document as Sol left it.

Part A: An Intellectual (Mostly) Autobiography

Early years. I was born on December 13, 1928 in the Bronx to working class parents who had emigrated to the United States after WW I and had met and married in New York: my father, Leon Feferman, was from Omsk, Russia, and worked as a housepainter, while my mother, Helen Grand Feferman, came from Warsaw, Poland, and was a dressmaker. Neither had had any advanced education. They identified themselves as Jewish culturally but were not religious. Besides the English that they acquired in the U.S., their languages were Yiddish and Russian. We lived in a brick walkup of four or five storeys not far from the Bronx Zoo. I played in the streets but preferred reading, and in school was good at Arithmetic and Spelling.

When I was 9 years old, in 1938, my family moved to Los Angeles in the hopes of a better life and a (then) more salubrious climate. At the outset, we lived in Boyle Heights in East Los Angeles, at that time a Jewish enclave (that turned into a Latino enclave in later years). I finished sixth grade there at the age of ten (I had been "skipped" a couple of times), and started middle school in Boyle Heights. I was perhaps first attracted to science through serials such as "Buck Rogers in the 21st Century" at the Saturday afternoon movie matinees. I wondered if I would ever live to see the 21st century, since I calculated that I would have to still be alive at age 72 in order to achieve that. The 1939 World's Fair in New York fascinated me from a distance since I never got to go. But at that remove I thrilled to the iconic Trylon and Perisphere structures and the marvels of the Futurama exhibit, the first television set, and the Time Capsule that was to be opened 5000 years hence. On top of all that, Albert Einstein gave a lecture on cosmic rays, and Superman (so they said) made an appearance in person.

In 1940, we moved to Hollywood, not at all in the glamorous part, but modestly comfortable: that was to be our home for many years. On December 7, 1941, our middle school principal called a general assembly to tell us that the Japanese had bombed Pearl Harbor; that meant we would enter WW II. Six days later my parents had a party to celebrate my 13th birthday. I met my wife to be, Anita Burdman, for the first time at that party, but she hardly spoke to me there since she was a year older and I was just a "kid" in her eyes. So she hung around with my sister (4 years my senior) and her friends, while I hid in the back with mine.

In those years, I started reading science fiction, going back to Jules Verne and H. G. Wells but then moving on to the stories in the then current pulps such as *Amazing Science Fiction* and *Astounding Science Fiction* and authors such as Robert Heinlein and Isaac Asimov. I would also take a streetcar downtown to the Main Library where I could venture into the "adult" section for detective fiction and such topical authors as Sinclair Lewis and John Dos Passos. My most ambitious reading was Thomas Mann's *The Magic Mountain.* In general, I was puzzled by the adult relationships in these novels.

At Hollywood High School, which I entered in 1942 at age 13, I excelled in Mathematics, English, History, and Art, and did well in Physics, Chemistry, and

German. My Algebra teacher was primarily a gymnastics coach and did not know what the Calculus was; the most advanced mathematics one could take at Hollywood following Algebra and Plane Geometry and Trigonometry was "Solid Geometry". Somewhere along the line I somehow decided that I was going to be a theoretical physicist and a professor, and I read popular books about relativity theory and quantum mechanics by such authors as James Jeans and A.S. Eddington. I also read philosophy by Bertrand Russell and John Dewey, and for a few years took seriously the so-called General Semantics movement of Alfred Korzybski, whose book consisted of a mish-mash of type theory, non-Aristotelian logic, and colloidal chemistry. Then I somehow discovered Rudolf Carnap's *Der logische Aufbau der Welt* and carried that around with me to show off but could not really penetrate it, though it had some sort of magical hold on me.

College years; CalTech. The high school schedule was accelerated due to the war and the necessity of some of the male students to finish before being drafted or joining up with the military. By taking summer classes, I graduated in the middle of the school year 1944–1945 soon after I had turned 16, and was ready to go to college. The only choice I considered was between UCLA and CalTech (California Institute of Technology); the former was free and co-educational and was where most of my friends were going or had already gone, while the latter was relatively expensive and (then) for men only but was the place to study physics and science more generally. I did well on an entry exam at CalTech, was accepted to begin in early 1945, and was offered a partial fellowship that I could supplement with waiter work in the Athenaeum, the faculty club. But it was still a financial burden for my family, gladly undertaken out of pride for my being a student there.

In my first semester that spring at CalTech, I took Calculus and loved it, and Physics—which was mostly about systems of pulleys that was as far from the romance of relativity and quantum theory as one could get—and did not love it. The students were a mix of naïve youngsters like me and returning veterans who knew the ways of the world and women. The war was still going on though winding down in Europe; in April 1945 we were shocked when Franklin Delano Roosevelt died; he had been our rock through all of the depression years and the war. The Vice President and Missourian, Harry Truman, became President, and we did not know how he could possibly fill Roosevelt's shoes. Within a month, Germany surrendered, and in early August, Truman ordered atomic bombs to be dropped on Hiroshima and Nagasaki, thus horrifically ending the war with Japan. Within a year, as the Communists took over Eastern Europe, the Cold War was underway.

Coming out of a bookstore near Hollywood and Vine late in the summer of 1945 I ran into Anita Burdman. I had just finished the summer session at CalTech, thus completing my first year there. Anita and I had not had any contact in high school, except to say "Hi" in passing on the way to classes. I thought she was beautiful and smart and so appealing, and would have liked to date her, but she was just leaving for studies at U.C. Berkeley, having already finished a year at UCLA. But we were able to see each other when she returned home for Christmas vacation; we started dating then and in the later summer vacations, the relationship began to become serious.

Meanwhile, at CalTech I continued to enjoy doing mathematics in such applicable courses as differential equations and vector analysis, but physics itself was turning out to be a disappointment. I found that I did not have the requisite physical intuitions, and the mathematics involved in the physics courses was make do, not treated as a subject for its own interest. A high point was a course on general relativity by Linus Pauling that attracted a large audience of both undergraduates and graduate students, and was pitched way over my head. Meanwhile, I had decided by default to switch my major to mathematics. But the upper division courses in that turned out to be somewhat of a culture shock; it was no longer techniques to master and problems to solve, but now abstract concepts ("group", "linear space", "topology") to understand and proofs to follow. My place in that subject did not begin to open up until I took a course on logic by Eric Temple Bell, the number theorist and popular historian of mathematics (and author of science fiction novels under the pseudonym, John Taine). The course was a hodgepodge because Bell did not really understand the modern logic (I learned later that he was a fan of Lukasiewicz' three-valued logic). While the material was of great appeal, I did not then see where it would take me. I thought my greatest personal achievement while in college was reading James Joyce's *Ulysses*. My greatest impact may have been through a job I had one summer collecting air samples on top of the roof of a seven-storey downtown Los Angeles building for a study of smog. That had already begun to be a serious air pollution problem and the chemical analysis of the samples (carried out by others) showed that it was primarily due to the unfiltered contents of automobile exhausts.

Having decided on an academic career, at the end of my undergraduate studies at CalTech in 1948, I applied for graduate work in mathematics at the University of Chicago and U.C. Berkeley. Accepted at both, it did not take much for me to decide which to choose, since I was offered a teaching assistantship at Berkeley and since Anita Burdman was still there. Within 4 months of my arrival, we were married, 4 days shy of my 20th birthday. Having already finished at UC, she had a job at a psychiatric institute in San Francisco as an assistant teacher on the pediatric ward. After a year of that demanding work she decided to return to school and get a credential as an elementary school teacher.

Graduate studies, pursuit, and interruption. I spent my first year at Berkeley, 1948–1949, taking the required mathematics courses for the Ph.D. program in real and complex variables, and in modern algebra. Teaching assistant duties consisted in holding office hours and helping grade papers and exams for undergraduate courses offered by faculty in the department. But in the summers I received extra income teaching basic algebra and calculus; I found that I was good at it and enjoyed teaching a lot. The Math graduate students, irrespective of level, were housed in the wooden "T" buildings in the North end of campus; those structures had been put up during the war and were supposedly temporary, but lasted for decades after. One of the students I became friendly with there was Frederick B. Thompson, who was working on a Ph.D. thesis with Alfred Tarski. He raved about

Tarski and encouraged me to take his year-long course on metamathematics, which I proceeded to do in my second year, 1949–1950.

As I have related elsewhere, when I did do so I knew immediately that this was to be my subject and Tarski would be my professor. He explained everything with such passion and, at the same time, with such amazing precision and clarity, spelling out the details with obvious pleasure and excitement as if they were as new to him as they were to us. He wrote on the blackboard with so much force that the chalk literally exploded in his hand, but step by step a coherent picture emerged. Methodically yet magically, he conveyed a feeling of suspense, a drama that managed somehow to leave us with a question hanging in the air at the end of the hours.[4]

That same year I started taking part in Tarski's logic seminar that was attended by novices like me along with doctoral students in various stages of progress; besides Fred Thompson, these included Julia Robinson, who was close to finishing, Wanda Szmielew, also well advanced, and Anne Davis Morel. Within a year we were joined by Robert Vaught and Chen-Chung Chang. And not long after that, coming up rapidly from behind, were the bright as can be youngsters, Richard Montague and Dana Scott. In that first seminar, I impressed by making a good presentation of some simple results about Boolean algebras. Such presentations were a trial by fire, especially for those who could never state things with the clarity, exactitude, and adherence to his notation that Tarski demanded; they would be endlessly interrupted by him and forced to go through things until they got them right, if they could manage that at all. For some reason, I never had difficulties of that sort and could see that Tarski looked favorably on me as a result. But it took me most of the year to work up the courage to ask him to be my dissertation advisor; to my relief, he readily agreed.

However, in order to be advanced to candidacy for a Ph.D. in mathematics, I had first to pass an unusual and demanding qualifying exam. The setup was that the chair of the department, Griffith C. Evans, would assign a topic in the research literature far distant from one's own expertise and direction of interests; the material in question was to be studied on one's own and then presented to a committee in an oral examination, for which no time limit was set. The topic Evans chose for me was "Asymptotic eigenvalues of vibrating membranes". It took me much of a year to master to my satisfaction the substantial underlying material in partial differential and integral equations and Tauberian theorems and their specific applications to the given problem; all the while, Tarski kept urging me to get done with that work and move on to logic. In the event, in the spring of 1951 I impressed the examining committee of mathematical analysts with my command of the material and clarity of presentation and was duly advanced to candidacy.

[4] Anita B. Feferman and Solomon Feferman, *Alfred Tarski. Life and Logic*, Cambridge University Press 2004, p. 171. In recent years I found by looking at the detailed notes that I took for the course in question that it was quite slow going. A painful amount of time—close to a full term—was spent on developing an elementary theory of concatenation as a basis for syntax. Nowadays, if one gave a nod to that material at all, it would be done in a week.

In the meantime, I was attending Tarski's graduate course in Set Theory, another essential branch of mathematical logic, and continued to take part in his seminars. Much time that year and next was spent on his then primary interests in algebraic logic via so-called cylindric algebras and in model theory. From these, he proposed two possible thesis problems for me: the first was to obtain a representation theorem for locally finite cylindric algebras, and the second was to obtain a decision procedure for the first-order theory of ordinals under addition. The former would provide the completeness of the axioms for cylindric algebras as an algebraic analogue of the completeness of first-order logic, while the latter would be an extension of a decision procedure that Tarski and his former student Andrzej Mostowski had established some years earlier for the first-order theory of the ordering of the ordinals.

In the years 1951–1953, I acted both as Tarski's Course Assistant for his graduate courses in Metamathematics and in Set Theory, and as his Research Assistant. The former relieved me of routine teaching assistant responsibilities and at the same time allowed me to gain a greater command of the material, which were their subjects. The latter often involved working with him—as was his wont—into the wee hours in his smoke-filled study at his home, with no concern for one's stamina or one's personal life outside of his demands. One frequent task was to help put his articles in final form in preparation for the typist and thence for publication, going over all the details and advising as to the choice of words in English, since Polish was his native tongue. But his own English was excellent (spoken accent aside) though rather "correct", and most often, after considerable discussion, he would choose his own word in preference to my suggestions.

A more substantial long-term task assigned to me was to reformulate in terms of Tarski's theory of arithmetical classes the work of Wanda Szmielew's thesis providing a decision procedure for the first-order theory of Abelian groups that had been obtained via the syntactic method of elimination of quantifiers. As I engaged in that I began to come to the conclusion that the aim in question was a greatly misguided attempt to put Szmielew's procedure in so-called ordinary mathematical terms. The background to that goal was Tarski's constant efforts to try to interest mainline mathematicians in the work of logic, and especially in that of his school. He thought (perhaps rightly so) that an obstacle to their appreciation of such was in the constant use by logicians of the notions of formal symbolic languages and in particular in the notions of formulas and sentences for such languages. But what the theory of arithmetical classes did was to provide model-theoretic surrogates for those notions while kicking away the traces of the formal languages that dictated their choice. In principle, translating Szmielew's work into the language of arithmetical classes should have been routine, painful as that might be. What it would not do, and what puzzled me in the whole enterprise, was that it would provide no illumination for why her procedure worked in the first place. I thought there should be some underlying explanation for that from the algebraic facts about finite and infinite Abelian groups as looked at in model-theoretic terms, and spent a lot of time trying to see what that would look like without really doing what I was assigned to do. In the end, Tarski was extremely annoyed with me (justifiably so) and had

Szmielew herself carry that through. But as later work showed, my instincts (the first where I was thinking for myself) were sound: Abraham Robinson found simple model-theoretic necessary and sufficient conditions for the eliminability of quantifiers in general, and then Paul Eklof and Edward Fischer established those conditions for the theory of Abelian groups by making substantial use of the known mathematical facts about the structure of countable Abelian groups.

Meanwhile, I was supposed to be doing my own research toward a dissertation and the first thing I tackled was a representation theorem for locally finite cylindric algebras. (The pursuit of cylindric algebras was another attempt to recast logical notions in "ordinary" mathematical terms.) I turned first to reexamination of proofs of the completeness theorem for first-order logic of which the representation theorem would be an analogue. The simplest such proof was that provided by Leon Henkin in a modified form due to Gisbert Hasenjaeger; I found that the ideas for that proof could be converted into prima facie algebraic terms and lead to the desired result. However, Tarski thought that my proof was not algebraic enough and pushed me to improve on it. I did not see how that could be done, and left it at that. Years later, I learned that what I had found (but never published) was the eventual "standard" proof of the representation theorem in question provided by Henkin himself.

The second thesis problem on which I worked and that Tarski had proposed was to provide a decision procedure for the theory of ordinals under addition. In that case, building on Mostowski's work on powers of theories as a means to reduce the decidability of the theory of natural numbers under multiplication to that of the numbers under addition ("Presburger arithmetic"), I introduced a notion of generalized powers of theories that could be applied to my problem. What I ended up showing by those means was that the decision problem in question could be reduced to the decision problem for the weak second-order theory of ordinals under the less-than relation, but I did not succeed in establishing the latter itself. Still I thought that the combination of that with the representation theorem would be satisfactory for a thesis, but Tarski refused to accept it.

Meanwhile, the Los Angeles draft board was breathing down my neck and wanting to know why I was taking so long with my graduate studies. The draft had continued from WW II through the Korean War and I had received regular deferments all along as a graduate student. The board thought that 5 years should be enough for a Ph.D. and could not be persuaded that I should be deferred any longer. Thus it was that I was drafted into the US Army beginning in September 1953 and my graduate work was suspended.

The army years and completion of graduate studies. While up to then I had always been the youngest in my group, in basic training at Fort Ord, California, I found myself surrounded by 18- and 19-year olds who regarded me at age 24 as an old man. Physically, too, years of sitting at a desk had exacted their toll, and it took a while to toughen up and manage the long marches and runs with rifles. On the firing range, I was lucky to hit the target. Fortunately, the fighting in Korea had ended with an armistice in July 1953, and the prospects of being sent into battle while in the army were considerably lessened. At the end of the 3 months in basic training, I was assigned to the Signal Corps in Fort Monmouth, New Jersey and my wife and I drove there in December 1953. On top of everything else, we had learned that she was pregnant on the eve of my being drafted. At Fort Monmouth, married soldiers could live off base, and we found a small house that was just right for us and our child to be; our first daughter was born there in May 1954.

Thanks to my mathematical background, I was assigned to a research group where we mainly spent the time calculating "kill" probabilities of Nike missile batteries around New York and Washington against possible incoming missile attacks. The fact that these were never even close to 100% had to give one pause. My fellow workers in the research office were mostly draftees like me who were closer to me in age, having been student deferees too, but we had a civilian boss. There was not much real work to do and there was a lot of time for casual conversation or to read William Feller's book on probability theory. But any sort of political discussion was highly discouraged, since Senator Joe McCarthy and his group had come through Ft. Monmouth earlier in the fall as part of his witch hunt to unearth communists under every possible rock, and that had left a residue of fear.

My research responsibilities did not exclude me from being assigned KP ("Kitchen Police") or night guard duty from time to time. And at home, finances were more than tight and there was much to do to help out with our new baby. Still I managed to keep my logical studies alive (when sleep deprivation and breathing space allowed) by reading Kleene's *Introduction to Metamathematics* (1952) in order to get a better understanding of recursion theory and Gödel's theorems than I had obtained in my Berkeley courses. As it happened, out of the blue one day when I was well advanced in those studies, I received a postcard from Alonzo Church asking if I would review for *The Journal of Symbolic Logic* an article by Hao Wang (1951) on the arithmetization of the completeness theorem for the classical first-order predicate calculus. I do not know what led Church to me, since we had had no previous contact, and I was not known for expertise in that area; perhaps my name had been recommended to him by Dana Scott who had left Berkeley in order to study with Church in Princeton, after a breakdown of his relations with Tarski due to the dereliction of his duties as Research Assistant. Quite fortuitously, my work on that review led me directly down the path to my dissertation.

The completeness theorem for the first-order predicate calculus is a simple consequence of the statement that if a sentence of that language is logically consistent then it has a model, and in fact a countable one. Actually, Gödel had shown that this holds for any set of sentences T. A theorem due to Paul Bernays in Hilbert and Bernays (1939) tells us that any first-order sentence can be formally modeled in the natural numbers if one adjoins the statement of its consistency to PA, the Peano Axioms; Wang generalized this to the statement that if T is any recursive set of sentences, then T is interpretable in PA augmented by a sentence $\mathrm{Con}_{\mathrm{T}}$ that expresses in arithmetic the consistency of T. Wang's somewhat sketchy proof more or less followed the lines of Gödel's original proof of the completeness theorem. In my review, I noted that his argument could be simplified considerably by following the Henkin–Hasenjaeger proof instead, by then much preferred in expositions. But in addition I criticized Wang's statement on the grounds that it contained an essential ambiguity. Namely, there is no canonical number-theoretical statement Con_T expressing the consistency of an infinite recursive set of sentences T, since there are infinitely many ways in which membership in T (or more precisely, the set of Gödel numbers of sentences in T) can be defined in arithmetic, and the associated statements of consistency of T need not be equivalent. So that led me to ask what conditions should be placed on the way that a formula of PA defines membership in T in order to obtain a precise version of Wang's theorem. Moreover, the same question could be raised about formulations of Gödel's second incompleteness theorem for arbitrary recursive theories T.

By the time I was released from the army in September 1955 and returned to Berkeley to complete my doctoral studies, I had decided to devote myself to the precise study of formal consistency statements and the arithmetization of metamathematics in some generality, including both the completeness theorem and the incompleteness theorems. It happened that Tarski was on sabbatical in Europe that year, and he asked Leon Henkin to take over as acting advisor in his absence. Though Henkin's own major interests were in model theory and algebraic logic, he offered me a willing ear and a great deal of encouragement, and with the prod of weekly meetings, I soon made significant progress. It was thus that when Tarski returned from his sabbatical in May 1956, I had a body of work that I was sure was thesis worthy, and presented it to him as such. This included generalizations of both Gödel's incompleteness theorems and the Bernays–Wang completeness theorems for which I showed that there was an essential distinction between the two in terms of the conditions to be imposed on the formula used to express membership and thence provability in a system. In addition, it opened up in novel ways the study of the interpretability relation between theories, a relation that had been of particular interest to Tarski. But to my dismay, instead of pronouncing it "excellent!" he hemmed and hawed. Perhaps, he was irked that the subject was not the original one *he* had suggested and not in any of his own main directions of research; instead, it sharpened and extended considerably the method of arithmetization that Gödel had introduced in 1931 to prove his incompleteness theorems. Perhaps, the old rivalry Tarski felt with Gödel over those theorems was awakened. In any event, he decided not to decide on his own whether the work was sufficiently important and instead asked me to send a summary of the results to Andrzej Mostowski in Poland. This took more time and created more tension. To my relief, Mostowski found the results new and interesting and strongly encouraged Tarski's approval. Mostowski's intervention was decisive, and so Tarski agreed at last to accept the results of my research for the dissertation. But dotting all the i's and crossing all the t's took another year before conferral of the Ph.D., by which time I was installed as an instructor at Stanford University.

Stanford; the early years. To cap off my fortuitous year at Berkeley with Leon Henkin, I learned from him of an opening for an instructorship at Stanford to teach logic and mathematics; the information came from Patrick Suppes of the Philosophy Department at Stanford. The subject of logic was based there in that department, since mathematics was a bastion of classical analysis in those days.[5] After a personal visit to meet Suppes, an appointment with a joint position in mathematics and philosophy was made, and I came to Stanford in 1956. Except for leaves of absence of one sort or another since then, that was to become my permanent academic home. Our first personal home there was a tract house of modern open design that we purchased in South Palo Alto. Along with all our possessions, we arrived with our two daughters in tow; the second one had been born in July 1956.

Activity at Stanford in the area of logic was initially greatly spurred by the efforts of Suppes, who had come to the Philosophy Department in 1951; he was joined at that time by J.C.C. McKinsey with whom he collaborated on the axiomatic foundations of physics until the latter's tragic death (a probable suicide) in 1953. McKinsey had earlier done important joint work with Alfred Tarski, and it was Tarski who suggested to Suppes that he be invited to teach logic at Stanford. After his death, McKinsey was succeeded in the Philosophy Department by Robert McNaughton, who later became famous for his contributions to automata theory and computer science. But then Suppes, no doubt inspired by the example and influence of Tarski, aimed to establish logic as an active interdepartmental subject at Stanford. Through his great powers of persuasion and with the help of Halsey Royden in mathematics, Suppes worked to bring about faculty appointments in logic at the junior and senior levels many of them jointly in the philosophy and mathematics departments. Thus a couple of years after I came to Stanford, Georg Kreisel began spending part of each year as Visiting Professor; his appointment was made permanent in 1964. Other additions to the faculty were those of John Myhill in the early 60s, then Dana Scott, and later Harvey Friedman, while Bill Tait and Jaako Hintikka were brought into the Philosophy Departments. The period of the 60s saw, too, the beginning of a steady stream of visitors and the production of first-class Ph.D. students.

Of particular significance to me in this stimulating group of colleagues was Georg Kreisel, who was to become my second and more lasting mentor in logic. I had first met him during the period in early 1956 when I was well into the research for my hoped-for dissertation; Kreisel happened to be visiting Berkeley for a month or so at that time and Dana Scott had told him to look me up. Our initial personal contact clicked wonderfully for me: I had hardly to begin explaining what I had done and what I was in the process of working on to see that Kreisel understood immediately and that it related to things he had thought about and to a whole body of literature in which he was completely at home. His positive reception of my ideas confirmed my views of the significance of what I was up to, and added to, my determination to make this work my thesis, despite Tarski's reservations. In addition to the active encouragement and regular monitoring of the work given to me by Henkin, the boost provided by Kreisel's quick appreciation was psychologically crucial at that agonizing time. Furthermore, Kreisel opened up a new world to me

[5] Its most distinguished faculty members were George Pólya and Gabor Szegö.

through his interests in constructivity, predicativity, and proof theory, interests to which I was naturally attracted and that would come to dominate my own subsequent work.

It was not only in subject matter that Kreisel differed from Tarski. In personality, he was courtly and charming with a quick wit, sometimes sly and sometimes devastating. In his technical work and expositions, he was much more concerned to explain, at length, the significance of the work than to set it out in an organized step-by-step fashion. His attitude seemed to be that if one has the right ideas the details would look after themselves. And they did amazingly often; details bored him, and if necessary, he could rely on more disciplined collaborators to supply them or, if necessary, patch things up. He was also very quick to take in others' ideas and proofs, as well as to anticipate trouble spots. Under Kreisel's influence and thorough critiques, I learned to write papers with the main aims up front instead of plunging into the details of formalism, but I never gave up the Tarskian concern for clarity and precision in the statement of results and the spelling out of proofs.

My teaching at Stanford was at first largely in mathematics—mostly in the lower division calculus sequences—while my logic teaching in Philosophy was of some elementary introductions to logic. I greatly enjoyed lecturing and being entirely responsible for my own courses. In later years, I expanded the material I taught in mathematics to various upper division undergraduate courses, including differential equations, linear algebra, algebra, number theory, history of mathematics, and foundations of analysis. My notes for this last led me to my first book, *The Number Systems* (1964). And in Philosophy, I progressed to teaching upper division and graduate courses in logic and set theory. In 1961–1962, I gave a graduate course in metamathematics that covered model theory, recursion theory, and proof theory over three-quarters;[6] my notes for that were bound in the Mathematics Library as *Lecture Notes in Metamathematics* (1962), but never published. All of this teaching was invaluable in deepening my understanding of various areas of mathematics and logic. In subsequent years, we replaced the year-long metamathematics course by courses of two- or three-quarters devoted separately to model theory, recursion theory, proof theory, and set theory, in alternating years. In still later years, I divided my time equally between mathematics and philosophy, and added both Philosophy of Mathematics and Theories of Truth to my regular offerings.

In my first year 1956–1957 at Stanford, besides finishing up my thesis for the Ph.D. at Berkeley, I began thinking about the question of what one could obtain by transfinitely iterating the process of adjunction of consistency statements to a theory in order to overcome Gödelian incompleteness. I learned that this had been considered by Alan Turing in his dissertation with Alonzo Church at Princeton University in 1939 under the rubric of "ordinal logics," and I set out to study what he had accomplished. This was slow going for me as the presentation was couched

[6] Stanford was then and is to this date on the quarter system rather than the semester system. Quarters consisted of 10 weeks (compared to 15-week semesters), and one could teach courses lasting one, two- or three- quarters.

in the language of Church's lambda calculus, with which I had had no experience and initially found very obscure.

Another thing that was begun that year was work on a monograph in collaboration with Richard Montague on the method of arithmetization and some of its applications. The idea was, essentially, to provide a combined presentation of the results of both our doctoral theses. Montague's thesis work concerned non-finitizability results in axiomatic set theory via relative consistency proofs, making use to some extent of my precise treatment of proof predicates and consistency statements. Montague left Berkeley in early 1955 for a position at UCLA, where, like me, he continued working on the exposition of his results to meet what he took to be Tarski's exacting standards (in the end, I think they out-Tarskied Tarski). Also like me, his Ph.D. was not awarded until 1957. Over the following years, we invested considerable effort in the preparation of the joint monograph, but there were many partial drafts, and because of the technology of those days (handwritten MSS turned into typescript by secretaries) and because of the distance between us, progress was slow. At a certain point we both realized that we should publish the results of our respective theses as separate articles; mine appeared in 1960 (cf. [4][7]) and Montague's in 1961. (We had already each presented our main results to a broad audience of logicians at the 1957 Cornell Summer Institute to be described below.) But in subsequent years our paths steadily diverged and our thoughts and energies became largely directed elsewhere. Even so, since an agreement had been made early on with the North-Holland Publishing Co. for publication of the monograph, we continued to work on it sporadically, frequently prodding each other to take the next step. But even before Montague's awful murder in 1971, I had ceased to have any heart for the project. Moreover, research by others in the meantime had overtaken us and would have had to be incorporated in some way in order to remain up to date. In particular, Montague's dissertation work was pretty much superseded by a paper of Kreisel and Lévy in 1968 on applications of partial truth definitions to non-axiomatizability of various systems of arithmetic and set theory by statements of bounded complexity.

Starting in this same period I also worked with Bob Vaught on a paper on generalized products of structures. We had first begun talking of a collaboration in 1955, when I explained to him my work on generalized powers of structures that I had introduced in my (only partially successful) attack on the decision problem for ordinal addition, reducing that to the decision problem for the Boolean algebra of sets of ordinals with the less-than relation. Vaught's thesis had provided a proof of Los' conjecture that if a theory is closed under arbitrary finite ordinary (Cartesian) products of its models, then it is closed under arbitrary products of its models. Vaught recognized that my work and his could be combined to reduce properties of generalized products of structures to properties of the factors on the one hand and a certain Boolean algebra of subsets of the index set for the product on the other hand.

[7] This and all the other references in square brackets refer to Feferman's bibliography in this volume, starting on p. lxix. (WS and RS).

But preparation of a joint paper was slow going because he was then at the University of Washington, and producing and exchanging typewritten drafts took considerable time as with Montague; also both Vaught and I accepted the demands of Tarski-style precision, though he was even more meticulous than I. We did not complete the paper until 1958 and it was published in 1959 (cf. [2]).

Topping off that intense first year at Stanford, a 5-week-long AMS Summer Institute in Symbolic Logic was held at Cornell University beginning on July 1, 1957. The 1950s had witnessed a great increase in activity in mathematical logic and there had been some meetings on special topics but this was the first devoted to the broad spectrum of the field. As we described it in our biography, *Alfred Tarski. Life and Logic*:

> The conference was unusual for its length and breadth; the speakers represented all branches of mathematical logic and, for the first time, a large number of computer scientists took part. Most of the participants lived in the college dormitories and ate in the communal Cornell Hotel School dining room. It was like being at a summer camp… For weeks the green, hilly campus buzzed with talk about model theory, recursion theory, set theory, proof theory, many-valued logics, and significantly, the logical aspects of computation; there was also the usual discussion about whose work was the most important. There was novelty, rivalry, conviviality, and [even] scandal. (Feferman and Feferman 2004, p.220)

The inspiration for the conference came from Paul Halmos, a younger self-described "brash" mathematician at the University of Chicago, who worked in a number of mainline fields and had taken an interest in algebraic logic, thus connecting him with Tarski and his students and collaborators' ongoing work in the subject. With the assistance of Tarski and the other American leaders in the field—Alonzo Church, Stephen Kleene, Willard van Orman Quine, and Barkley Rosser—Halmos succeeded in getting the sponsorship of the American Mathematical Society (AMS) and financial support from the National Science Foundation (NSF). Tarski of course pushed to have the institute take place in Berkeley, while Rosser was adamant that it should be in Cornell; one argument was that the majority of participants would be coming from the East Coast; reluctantly, Tarski acceded.

> The issue of who would be invited to speak also created heat. There was quick agreement about the most prominent senior scholars, but discussion—mostly by correspondence—about who to choose among the up-and-coming younger crowd went on for many months, with each of the main organizers giving preference to his own disciples. On this score, Tarski did very well; about one fourth of the speakers were under his influence in one way or another, and many of them gave two or three talks. In this way, he succeeded in positioning himself as the leading man of the occasion. Because the reclusive Gödel, whose name was first on the invitation list, had declined to attend, there was no direct challenge to Tarski's assumption of that role. (ibid., p. 221)

There were also two "wild cards", Abraham Robinson (unrelated to Berkeley's Raphael M. Robinson) and Georg Kreisel:

> [They] came to the Cornell Institute unfettered by a link to a mentor. Both would soon have enormous influence on their younger colleagues… Both men were European, like Tarski,

> but much younger than he and more recently arrived in the United States. Robinson, born in Germany, had lived in Israel, France, England, and Canada, and he would return to Israel once more for a few years before settling in the United States. Kreisel—Austrian born, educated in England, and a frequent visitor to France—had a position in Reading, England. At the invitation of Kurt Gödel, he had spent the preceding two years at the Institute for Advanced Study [IAS] in Princeton. Largely self-taught in logic and less bound by tradition, neither Robinson nor Kreisel owed allegiance to a single methodology. Both had worked in applied mathematics in England during World War II, and as a result their style was much more experimental and free-wheeling than Tarski's. Also, each of these men in his own way exuded intellectual self-confidence; neither was afraid to lock horns with Tarski. (ibid., p. 223)

Robinson worked on problems concerning the applications of model theory to algebra, which of course interested Tarski very much. He introduced novel general approaches to the subject, among which (later on) were model-theoretic methods to establish decidability of various theories without having to make use of the method of elimination of quantifiers for which Tarski had been the standard bearer. In 1960, Robinson became famous for the creation of "non-standard analysis," a model-theoretic foundation for the systematic use of infinitesimal (and infinitely large) quantities in mathematical analysis.

Participation in the Cornell Institute was extremely important for my career, first for widening my understanding of the field, and then for the intellectual and personally valuable contacts I made with both senior and junior logicians, and finally for the opportunity to present myself and my work to that group. In consultation with Tarski, I gave two talks there. The first was on the results of my dissertation on the arithmetization of metamathematics [6]. For the second, I had proposed to him to speak about my joint work with Vaught on generalized products of models. But Tarski insisted that I speak instead about some work of Andrzej Ehrenfeucht and Roland Fraïssé [7]. The background was that subsequent to my efforts, Ehrenfeucht (a student of Mostowski's in Poland) had succeeded in establishing the decidability of the theory of ordinals under addition by means of the so-called "back-and-forth" methods that had been introduced by Fraïssé (then working independently in French Algeria and later in France). I have never gotten over Tarski's insistence that should be my second talk, but there seemed to be no way that I could get around him for that. And Tarski never seemed to realize the significance of my work with Vaught, though after its publication in 1959 it became a much-cited landmark in the field of model theory. That, too, made me much more aware of his blind spots.

Back at Stanford, at the end of my first 2 years there, I was promoted from the rank of instructor to the tenure-track position of Assistant Professor of Mathematics and Philosophy. The challenge in the following years would be to make tenure and, for that, more substantial work would have to be produced and recognized. In particular, in 1958–1959, after mastering Turing's work on ordinal logics, I reframed it as the study of transfinite recursive progressions of first-order axiomatic theories with standard formalization. The theories in such progressions are indexed along paths in the Church–Kleene system O of recursion-theoretic notations for all "constructive" ordinals. I re-proved Turing's completeness result for Pi-0-1