A Fortuitous Year with Leon Henkin

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Abstract This is a personal reminiscence about the work I did under the direction of Leon Henkin during the last year of my graduate studies, work that proved to be fortuitous in the absence of Alfred Tarski, my thesis advisor.

Keywords Completeness of predicate calculus · Henkin's proof of completeness · Incompleteness theorems · Formal consistency statements · Arithmetization of metamathematics · Interpretability of theories

In September 1955, I returned from a two-year stint in the US Army to continue and hopefully complete my graduate studies in mathematics at UC Berkeley. When I was drafted in 1953, I had been working on a thesis with Alfred Tarski under considerable strain and with only partial results. As it happened, Tarski was on sabbatical in Europe the year I returned, and he had asked Leon Henkin to act as my supervisor in his absence, to which Henkin agreed. This would prove to be crucial in helping me bring my doctoral work to a successful conclusion. Unlike my often late night discussions with Tarski, Henkin requested that we meet every Thursday afternoon for an hour or so, easy enough to do and even enjoy; I recall standing at the blackboard with him in his light-filled office in Dwinelle Hall. He also said that he wanted to hear something new from me each time; that was more challenging and a powerful constant prod. And finally, Henkin readily accepted my proposal for a major change in my thesis topic, a change that Tarski might well have resisted. So all that proved to be fortuitous, but there were also deeper connections with Henkin's own work.

I first met Leon Henkin in 1952 when he came to Berkeley to consider an offer for a tenured position in the Mathematics Department. He was then teaching in Los Angeles at the University of Southern California (USC), where he was working in comparative isolation in the field of logic. One reason for the offer was that he had made some of the earliest applications of model theory to algebra, a direction that had great appeal to Tarski. In those days, model theory, set theory, and algebraic logic were the main topics of research in the group of students and faculty surrounding Tarski. Being part of such a center of activity had great appeal to Henkin, but he refused to come while the infamous Loyalty Oath was still in force. This special and quite controversial oath was a requirement that had been laid down in 1950 by the Board of Regents for employees of the University of California system. It declared that one was not a member of the Communist Party or any other organization dedicated to the overthrow of the United States government. With it the university had aimed to forestall the McCarthy-era threats of investigation by such

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entities as the House Un-American Activities Committee. The institution of the Loyalty Oath had thrown the faculty into an uproar, among other things on the grounds that one already had to swear to uphold the US Constitution and that it was a clear violation of academic freedom. A number of distinguished faculty members who refused to sign on reasons of principle were fired, some left, while others stayed on but objected strenuously. Despite the great attractions of the Berkeley offer, Henkin sided with those who opposed the Loyalty Oath, and he decided to bide his time while the matter played out in the courts.

When the oath requirement was overturned in 1953, Henkin accepted the offer and came to Berkeley. By then I had already left for the army, but the personal contact I had made with him in 1952 laid the ground for our later work together. In fact, our encounter during his initial visit had been very friendly, and Leon had encouraged me to get in touch with him if I happened to be in Los Angeles. On the next occasion when my wife, Anita, and I were there visiting family and friends, I did just that, and he and his wife, Ginette, invited us to a casual dinner at their small apartment and made us feel at ease. With a difference in age of seven years, Leon was much closer to me than Tarski, and our similar ethnic and cultural backgrounds—both of us descendants of Eastern European Jewish immigrants—was a common touchstone that was understood without needing to be discussed. Also, he had grown up in Brooklyn, whereas I had grown up in the Bronx before my family moved to Los Angeles in the latter part of the 1930s.

Though at the height of the Cold War, my period of service, 1953–1955, in the US Army fortunately fell between the hot wars of Korea and Vietnam. And thanks to my mathematical background, I ended up being stationed in a Signal Corps research lab at Ft. Monmouth, New Jersey, where we mainly spent the time calculating "kill" probabilities of defensive Nike missile batteries; that did not exclude one from being assigned KP ("Kitchen Police") or night guard duty from time to time. I lived off base with my wife in a tiny house where our first daughter was born; finances were more than tight, and there was much to do at home to help out. Still I managed to keep my logical studies alive (when sleep deprivation and breathing space allowed) by reading Kleene's Introduction to Metamathematics (see [14]) 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 that work, I received a postcard from Alonzo Church asking if I would review for The Journal of Symbolic Logic an article by Hao Wang [22] on the arithmetization of the completeness theorem for the classical first-order predicate calculus. (I can still visualize what turned out to be the characteristic card from Church, meticulously handwritten in multicolored ink with wavy, straight, and double-straight underlines.) 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 to study with Church in Princeton. Quite fortuitously, my work on that review led me directly down the path to my dissertation. I have described that in some detail in an article "My route to arithmetization" (see [6]) and so will only give an idea of some of the main points here.

The completeness theorem for the first-order predicate calculus is a simple consequence of the statement that if a sentence φ is 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, from which the compactness theorem follows directly. A theorem due to Paul Bernays in Hilbert and Bernays [13] tells us that any sentence φ can be formally modeled in the natural numbers if one adjoins the statement of the consistency of φ to PA, the Peano

Axioms; in other words, φ is interpretable[1] in $PA + Con(\ulcorner\varphi\urcorner)$, where $\ulcorner\varphi\urcorner$ is the numeral corresponding to the Gödel number of φ, and $Con(\ulcorner\varphi\urcorner)$ expresses the logical consistency of φ. Wang generalized this to the statement that if T is any recursive set of sentences, then T is interpretable in $PA + Con_T$, where Con_T expresses the logical consistency of T in arithmetic. 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 Henkin's proof (see [11]) instead, by then much preferred in expositions.[2] 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 expressing the consistency of an infinite recursive set of sentences T since there are infinitely many ways $\tau(x)$ 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 Con_τ of consistency of T need not be equivalent. So that led me to ask what conditions should be placed on the way that the formula τ defines T in PA 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 ready to return to Berkeley, I was determined to strike out on my own and deal with these issues as the subject of my thesis. There are several reasons why this would have been a problem if Tarski were not away that year.

My dissertation efforts under Tarski's direction prior to 1953 had been decidedly mixed. He had been sufficiently impressed with me in my course and seminar work with him to make me an assistant in graduate courses on metamathematics and set theory and then a research assistant on several of his projects. I had demonstrated that I could meet his exacting standards of rigor and clarity of presentation. When I came to him for a research topic for a thesis, he proposed that I establish his conjecture that the first-order theory of ordinals under addition is decidable. This would strengthen his earlier result with Mostowski that the theory of the simply ordered structure of ordinals is decidable. Moreover, it would be best possible since adjunction of multiplication would lead to an undecidable theory. I worked long and hard on this problem without arriving at the desired result, but I was able to show that the decidability of the theory of ordinals under addition was reducible to that of the weak second-order theory of the ordered structure of ordinals, using a new notion of generalized powers of structures. This was definite progress, but not by itself enough for a thesis.[3]

Meanwhile, Tarski proposed another problem to me, namely to demonstrate the representation theorem for locally finite cylindric algebras, in other words, that every nontrivial such algebra is isomorphically embeddable in the cylindric algebra of essentially finitary relations on an infinite set. That is an algebraic version of the completeness theorem for

[1] Throughout, I use "interpretable" here to mean relatively interpretable in the sense of Tarski, Mostowski, and Robinson; see [20].

[2] Actually, a further simplification of Henkin's argument due to Hasenjaeger (see [10]) became the preferred mode of presentation.

[3] I was pleased to learn in 1957 that Andrzej Ehrenfeucht obtained the sought-for decidability results; he applied back-and-forth methods rather than the elimination of quantifier methods that Tarski had expected. And, as it later turned out, the basic idea of generalized powers that I had introduced to reduce the decision problem could be combined in a fruitful way with the work of my fellow student Bob Vaught on sentences preserved under Cartesian products, leading to the paper Feferman and Vaught [7] on generalized products of theories.

first-order logic. And that led me to establish the desired representation theorem by transforming Henkin's proof of the completeness theorem into algebraic terms. I proposed to Tarski that I combine this with my reductive work on the theory of ordinals to constitute a thesis in two parts. But Tarski was not satisfied with my proof of the representation theorem; he wanted something that was more intrinsically algebraic.[4] My prospects of making further substantial improvements on either topic did not look promising, nor did they any longer hold any appeal to me, so I decided to tackle something new. Tarski might have resisted my choice to work on the problem of arithmetization of the completeness and incompleteness theorems rather than on a problem that he had proposed because it related instead to questions that basically went back to the work of Gödel, his chief rival for the honorific, "most important logician of the 20th century."[5]

On the other hand, Henkin was sympathetic because he had already raised another problem concerning arithmetization in "A problem concerning provability" (see [12]), namely whether or not the sentence ψ that expresses of itself that it is provable in *PA*, that is, for which $PA \vdash \psi \longleftrightarrow Prov_{PA}(\ulcorner\psi\urcorner)$ is provable in *PA*. This was by contrast with Gödel's sentence γ that expresses of itself that it is not provable in *PA* in the sense of $PA \vdash \gamma \longleftrightarrow \neg Prov_{PA}(\ulcorner\gamma\urcorner)$. In the latter case, all we need to know about the formula $Prov_{PA}(x)$ used to state this is that it numeralwise defines the set of provable sentences of arithmetic in *PA*. However, Kreisel [15] showed that the same condition is not sufficient to give a definite answer to Henkin's question: under one choice of the numeralwise representation of provability, the associated sentence ψ is provable in *PA*, whereas under another choice, it is not provable in *PA*.[6]

In my thesis work with Henkin I decided to move as closely as possible to canonical arithmetization by taking $Prov_\tau(x)$ to express in a standard way that x is the number of a sentence that is provable in the first-order predicate calculus from the set of sentences represented by $\tau(x)$. But even so I was able to show that that is not determinative. On the one hand, by taking Con_τ to be the negation of the sentence $Prov_\tau(\ulcorner 0 = 1\urcorner)$, I was able to obtain a precise generalization of Gödel's second incompleteness theorem to arbitrary recursively enumerable consistent extensions T of *PA*.[7] Namely, if T is represented in *PA* by an RE (i.e., Σ_1) formula $\tau(x)$, then Con_τ is not provable in T. In particular, for the standard (bi-)numeration π of *PA* in *PA*, Con_π is not provable in *PA*. But, on the other hand, I was able to construct a bi-numeration π^* of *PA* in *PA* for which Con_{π^*} is provable in *PA*; $\pi^*(x)$ expresses that x belongs to the "longest" consistent initial segment of the axioms of *PA*.

These results turned out to have novel consequences for the relation of interpretability between theories. On the one hand, my generalization of Gödel's second incompleteness theorem could be further strengthened to show that if T is a recursively enumerable consistent extension of *PA* and $\tau(x)$ is any RE formula numeralwise representing T in *PA*

[4] Years later, I learned from Steve Givant that this was the standard route for proving the representation theorem, but I have not checked the literature to see exactly how it is usually presented.

[5] Tarski told John Corcoran that he considered himself to be "the greatest living sane logician"; cf. [1, p. 5]. My frustrations working with Tarski as a student were by no means unique as is testified to in the many stories in that biography.

[6] See Halbach and Visser [9]. Löb [18] proved that for the standard formalization of the provability predicate, the Henkin sentence is provable in *PA*.

[7] As is well known nowadays, this can be improved to arbitrary recursively enumerable extensions of the fragment Σ_1-*IA* of *PA* and even weaker theories.

then $T + Con_\tau$ is not interpretable in T. On the other hand, I obtained a precise general version of the Bernays–Wang arithmetized completeness theorem in the following form: if $\tau(x)$ is any formula that numeralwise represents T in *PA*, then T is interpretable in $PA + Con_\tau$. Moreover, it turned out by use of the formula $\pi'(x) = \pi^*(x) \vee x = \ulcorner \neg Con_\pi \urcorner$ that one has $PA \vdash Con_{\pi'}$, so $PA + (\neg Con_\pi)$ is interpretable in *PA*. This "interpretability of inconsistency" is thus a foil to the "noninterpretability of consistency."[8] With further related results obtained during that crucial year (1955–1956), Henkin was satisfied that I had enough for a thesis, and he enthusiastically recommended that to Tarski, who was still my principal advisor. But it was only when Tarski obtained a further corroboration from Andrzej Mostowski that he eventually agreed to accept it for such.

To cap off my fortuitous year with Leon Henkin, he heard from Patrick Suppes of an opening for an instructorship at Stanford to teach logic and mathematics. The subject of logic was there based in the Philosophy Department since Mathematics was a bastion of classical analysis in those days. 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; I have been there ever since, except for a number of fellowships and visiting positions elsewhere over the years.[9]

1 Coda

The results of my thesis were published in the paper "Arithmetization of metamathematics in a general setting" (see [3]), which has been cited frequently for that subject in the subsequent literature.[10] I also made use there of a further result due to Steven Orey, who realized that nonstandard representations like π^* could be used to arithmetize the compactness theorem. The following is a special case of Orey's theorem: If T is a recursively enumerable theory and each finite subset of T is interpretable in *PA*, then T is interpretable in *PA*.[11] One extensive direction of work that was fruitfully opened up by the 1960 paper but that I did not myself pursue any further concerned the general lattice structure of the interpretability relationship between theories; cf., e.g., Hájek and Pudlák [8, Chap. III.4], Lindström [16] and [17], and Visser [23]. However, I did go on to use RE representations in an essential systematic way in Feferman [4] to obtain a precise uniform formulation of the transfinite iteration of consistency statements that allowed me to extend the results of Turing [21]. And that, at the important suggestion of Georg Kreisel, opened the way for me to go on to characterize predicative provability in analysis via an autonomous progression of theories; see [5]. The rest, as they say, is history.

[8] See Visser [24] for a full exploration of the phenomenon of interpretability of inconsistency.

[9] I spent the first year at Stanford writing up my thesis (see [2]) and received the Ph.D. at UC Berkeley in 1957.

[10] Cf., e.g., Hájek and Pudlák [8, p. 2]. Incidentally, see Feferman [6, p. 177] for an explanation of why the ongoing plans to combine my thesis work with that of my fellow student, Richard Montague, in the form of a monograph were never completed.

[11] Orey had heard me talk about my thesis work at the Institute for Symbolic Logic held at Cornell in the summer of 1957, and that led him to his theorem, which he kindly let me include in my 1960 publication; cf. also Orey [19].

I am not in general one for “what ifs,” but here goes. What if my work with Tarski prior to 1953 on the problems he proposed had been successful? What if I had not been drafted? What if I had not been asked by Church to review the Wang paper? What if Tarski had not been away the year I had returned from the Army? What if Henkin had not been in Berkeley to act in his place? In fact, none of the “what ifs” held, and I am eternally grateful to Leon Henkin for his being there for me at the right place at the right time.

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