Leon Henkin and a Life of Service

Benjamin Wells

Abstract For 45 years, Leon Henkin provided dedicated, unstinting service to people learning mathematics. During most of that time, the author had personal contact with him. Henkin's seminars, projects, letters, and advice influenced the author's career path on many control points.

Keywords Henkin · Logic · Mathematics education · Service · Personal recollection

1 Seminars in Berkeley and Montreal with Leon Henkin

My first impressions of Leon Henkin were permanent. He had little hair, slightly impeded speech, and constant good cheer signaled by an instant yet lasting smile—indeed, he beamed. Even when serious or taking one to task, he conveyed sympathy, warmth, and genuine interest. Although our relationship was neither social nor personal, there was great fondness and appreciation on both sides.

In 1962, I began graduate work at the University of California in Berkeley having completed three courses in topology (and audited two more) and two courses in logic at MIT as an undergraduate. All of these except the first topology course were listed as graduate courses. At Berkeley, I enrolled in Metamathematics from John Addison, Topological Groups from Gerhard Hochschild, General Algebraic Systems from Alfred Tarski, and a seminar on Algebraic Geometry from Claude Chevalley. I considered topology at the top of the list, but there were no advisors in the field, so I chose Shiing-Shen Chern's geometry listing as the closest thing. He remained unfailingly interested in whatever I was doing for decades after advising me that first year. I signed up for Tarski's course because of a partial knowledge of and fascination with the Banach–Tarski paradox; I wanted to meet him.

I also audited (not for credit) a seminar on Metamathematics and Field Theory jointly offered by Leon Henkin and Tarski. Although Henkin and Tarski played equal roles in the conversation of the seminar, Henkin was the structural leader. He made the schedule of talks and kept the course organized. This would be the pattern in future Henkin–Tarski seminars I attended. I never took a course lectured by Leon, but I read cover to cover his printed notes for the undergraduate course in mathematical logic and found them lucid beyond measure. This was also my assessment of his talks in seminars and conferences.

M. Manzano et al. (eds.), *The Life and Work of Leon Henkin*, Studies in Universal Logic,
DOI 10.1007/978-3-319-09719-0_6

> Wells was a Ph.D. student in our Department and I first got to know him in 1962, when he enrolled in a seminar on the metamathematics of field theory which Alfred Tarski and I conducted jointly. In the next two years I saw him in action in further seminars on universal algebra and on cylindric algebras, and he also worked in a seminar on equational logic with Tarski in 1965.[1]

For the 1963–1964 Henkin–Tarski seminar on algebraic logic, Henkin suggested I try to rewrite the Daigneault–Monk proof of the representability of infinite-dimensional polyadic algebras "in the style of the seminar." He outlined the approach, and I painstakingly worked through the conversion. But there was a snag, reported in [19, p. 228]. I still think Henkin's impression of the possibility and worthiness of this alternate proof are correct, but it was not mine to give.

In spring 1966, Henkin and Tarski offered a seminar on metamathematics and algebra. Both were happy that I was willing to translate and discuss Yuri Yershov's result on the decidability of the elementary theory of p-adic fields. That summer in Montreal, at an institute sponsored by Séminaire de Mathématiques Supérieures, I presented Paul Cohen's sketch of a simplified proof of that result to Simon Kochen, coauthor with James Ax of a published independent and lengthy proof of this and much more.

> Tarski and I considered him a very promising student, and were well pleased with the seminar talks that he gave. As a result, we appointed him to work on an NSF research project in metamathematics and foundations; he served as a Research Assistant and Postgraduate Research Associate during 1965–1967, and again in 1971. Between 1967 and 1971 Wells visited Poland as a Stanford Exchange Fellow. This visit came about because Wells met the world-famous logician Andrzej Mostowski at a Séminaire de Mathématique Supérieure that was held in Montreal for a period of several weeks in Summer, 1966; I gave a series of talks there, subsequently published as a monograph [5], and Wells's dissertation is related to some of those ideas.[2]

Leon's lectures introduced predicate logic with a finite number of variables. This novel development raised many issues that were solved collaboratively in and beyond class. More than any other seminar that summer, his talks stimulated a continuous interactive discussion. My dissertation [18] studied equational logic with finite numbers of variables, based on an issue arising from my translation of a Mal'tsev paper appearing as [15, Chap. 29]. Equational theories are constructed that are not recursive despite every fragment with a bounded finite number of variables being recursive. Although unrelated in substance, I have often mused how this was foreshadowed in tone by Henkin's topic in Montreal.

Leon correctly recalls that my wish to visit Poland arose from my meeting Mostowski (and his student Janusz Onyszkiewicz, later to be the foreign press spokesman for Solidarity) in Montreal. A corrective outcome from our first encounter is related in [22, p. 422].

2 Leon Henkin's Help with a Visit to Poland

Both Henkin and Mostowski were consulted in Montreal about whether it made sense for me to visit Warsaw in 1966–1967. Both were encouraging. Next, I wrote Tarski for his view.

[1] See [9].

[2] See [9].

July 23, 1966

Dear sir,

Montréal has proved to be a delightful experience: mathematically, culturally, gastronomically, and a bit socially. I have been writing lecture notes for Kochen. He is speaking on the relations among completeness proofs by model completeness, elimination of quantifiers, and ultraproducts. Unfortunately he will not get to talk about p-adic fields in class, but we are discussing such problems on the sly. Also I have written the Mal'cev review and clarified some of my work relating to it—and some new problems. I have also been reading and thinking about the recursive real numbers and trying to write up an explanation of Cohen's p-adic number decision procedure. There are fine restaurants and beautiful girls.

I want to go to Warsaw this fall. Before you count me totally mad, read on, please.

(1) The situation in Berkeley. [. . .] I have committed myself to a parttime RA appointment as you know. Mr. Henkin says that as far as he is concerned I would not be bound to it if you too released me. [. . .] At this point let me state that while I do want to convince you of the wisdom of this project, I do not intend to undertake it without your blessing and am prepared to consider alternatives with an open mind. Roughly speaking then, there are no strong reasons of an "administrative" sort why I could not be away from Berkeley for at least 6–9 months.

(2) The situation in Poland. [. . .] A seminar will be given throughout the year on model theory for infinitary logics. Mr. Mostowski assures me that I will be well-received. I am already making plans to go climbing in the Tatra.

(3) The wider context. [. . .] If I stay in Berkeley I may or may not finish, and if not it will be another two years (from now) before I can go anywhere. The last few months in Berkeley were very bad; there are too many diversions; I tried to shut myself off from the world, but could not. Even though it may seem paradoxical, I think the change of scene would stimulate my mathematics rather than my procrastination. In evidence of this I might point to the situation here—while not great it must be seen in the context of 10–15 lectures per week, weekends often elsewhere, and mild climate. And yet here I can sit down and work steadily for hours. In Berkeley it happens, but it's difficult to control. [. . .]

(4) The arrangements. I have discussed this matter with Mostowski and Henkin. [. . .] I have asked Dana Scott to get Stanford to send me information on the exchange program. [. . .] As far as finances go, I have talked to Mostowski's student Janusz Onyszkiewicz, and he thinks one can live well on about $50/month. [. . .] University rules permitting, Henkin thinks I could hold my RA in Warsaw, if, of course, you both approve. [. . .] The prospect of spending a fruitful season in Warsaw is an exciting one. But a year in Berkeley is not without hope and promise. [. . .]

Here is Tarski's response in a letter imperfectly recalled in [3, p. 325] and not rediscovered until after that book was published. Part of it appeared in [21].

July 29, 1966

Mr. Benjamin F. Wells III, c/o Seminaire de Mathematiques Superieures, Universite de Montreal, Montreal 3, Quebec CANADA

Dear Pete,

I must say frankly that your letters have caught me somewhat by surprise. My reaction to their contents is rather mixed.

First let me say that there is certainly no "administrative" obstacle to your desire to go to Warsaw for the next year. I am ready to release you from your commitment. The only thing I wish is that you inform me of your final decision as soon as feasible so that we can appoint somebody else in your place. As regards the possibility of your retaining the research assistantship and the connected salary for the period of your studies in Poland the matter seems to me rather dubious. [. . .] Also the idea could not be realized without an explicit agreement on the part of the National Science Foundation. I shall discuss this matter with Professor Henkin on his return to Berkeley.

You will certainly have inspiring scientific contacts in Warsaw. Let me say however that you really could have had more such contacts in the Bay Area than you actually had if you tried hard enough.

This brings us to the heart of the whole problem. Both you and I realize and have realized for a long time that your greatest weakness is the lack of ability to "shut yourself off from the world",

> to resist diversions and distractions coming from outside. This single factor probably accounts for the insignificant progress which you have made so far in your work on the thesis. I wonder whether you will find in Warsaw a more favorable atmosphere from this point of view. I have learned from your letter with interest that there are fine restaurants and beautiful girls in Montreal. This information may be quite important for me if I ever get an invitation to teach there. I can assure you however that Warsaw is not a monastery and at any rate not a nunnery. There are fine restaurants there [...] and as regards the amount of beautiful girls Warsaw claims to be second to none among the cities of the world. If after your return from Warsaw you do not lend wholehearted support to this claim you will hurt my deepest feelings.
>
> To speak now seriously, if I am to evaluate the problem from a purely rational point of view I do not see much point in your going to Warsaw, at any rate not at this stage of the game. I might feel differently about this matter if you had now just finished the work on your thesis or at least were very close to its completion. I know however that reactions of human beings are usually not rational and that their actions are not motivated by rational factors. People in that part of the world are claimed to have various secret weapons. Maybe they will make some of them available to you and you will learn how to beat down the intrusion of the outside world.
>
> As you see this letter is something less than blessing but I am equally far from wishing to keep you by force in Berkeley. If the decision were entirely up to me I would probably suppress all my misgivings and have you go to Warsaw, treating the whole venture as a calculated risk. [...]
>
> Sincerely, Alfred Tarski

In fact, I did not visit Poland until 1967–1968, and then indeed as a Stanford Exchange Student. In the meantime, I learned some Polish and worked as a Research Assistant in the Group in Logic and the Methodology of Science (L&M) for the Tarski–Henkin NSF project as planned. Because my NSF Graduate Fellowship was not renewed, I also taught several UC Berkeley undergraduate math sections, including a Fortran class that would ultimately lead me to Meher Baba and India on my return from Europe.

In Warsaw, Leon's student Diane Resek and I shared an office in the Palace of Culture and Science, Stalin's wedding-cake building given to the Polish people. It dominated the Warsaw skyline. According to Dana Scott [17], S.C. Kleene was very pleased to tell this story about the Palace. When Adlai Stevenson visited Warsaw, he was shown the Palace and asked how he liked it. He replied: "Small, but in perfect style." Diane and I had many conversations about mathematics and life with our thesis advisors. Our paths crossed often after that.

3 Some Conferences; the Political Situation in Poland

In 1963, Addison, Henkin, and Tarski organized the very important International Symposium on the Theory of Models at Berkeley. The focus on model-theoretic applications in algebra and set theory was innovative and trend-setting. The three organizers edited the proceedings [2].

In 1968, the third International Congress for Logic, Methodology, and Philosophy of Science was held in Amsterdam shortly after I arrived in Poland. Mostowski and several of his students, including me, came from Warsaw. The proceedings do not list Henkin as an organizer or speaker, but I have a sense that he was there. He is not mentioned as a participant, and neither am I.

During my stay in Poland, there was much political unrest. There was an anti-Soviet atmosphere, but also government-stimulated antisemitism. In fall 1967, protesting students were arrested after the banning and closing of an anti-Soviet production of Mickiewicz's

anti-Russian play “Dziady”; I was among them. Student demonstrations led to beatings by police and mass arrests at the University of Warsaw starting on 8 March 1968. Protests and strikes followed, accompanied by governmental claims that Zionists were the fomenters. Official promotion of antisemitism increased. There were consequent coerced departures—call them exiles—from Poland of many Jews during the spring and summer.

For summer 1968, Mostowski organized a Conference on the Construction of Models for Axiomatic Systems in Warsaw. In spring 1968, Leon wrote Abraham Robinson, then president of the Association for Symbolic Logic (ASL), expressing dismay at the Polish government’s behavior and requesting ASL to consider withdrawing sponsorship of the meeting—according to Robinson’s reply [16] to Leon.

ASSOCIATION FOR SYMBOLIC LOGIC

ABRAHAM ROBINSON
President

DEPARTMENT OF MATHEMATICS
YALE UNIVERSITY
NEW HAVEN, CONNECTICUT 06520

May 14, 1968
Professor Leon Henkin, Department of Mathematics University of California Berkeley, California 94720

Dear Leon:

Thank you very much for your letter of May 7.

I wrote to Mostowski about a month ago telling him that I had decided not to attend the Warsaw meeting. I offered to give my reasons for doing so but have not heard from him since. Of course it did not escape me that if several people in this country came to the same decision, the phenomenon might be noticed in Poland. At the same time I did not want to harm our friends there.

However, the question of taking official action in this matter on behalf of the Association for Symbolic Logic is of an entirely different nature. To withdraw our sponsorship at this date would as far as I know be without precedent and might in the long run work against the return of sanity to Poland rather than for it. Although I agree that the behavior of the Polish authorities is quite obnoxious, many other governments make mistakes now and then, and I think that official bodies like the ASL have to react more prudently than individuals in such cases.

The situation will be changed if it appears that the Polish authorities actually refuse to admit prospective participants in the meeting for reasons of race, creed, etc. If such cases come to my knowledge I should consider it my duty to take action.

Yours sincerely,
Abraham Robinson
c.c. Prof. Martin Davis Prof. Dana Scott Prof. Alfred Tarski

I was planning to revisit Warsaw for this meeting. As I hitchhiked with a friend to the Austrian–Czechoslovakian border on my way to Prague to pick up a visa and then Warsaw, we learned that the Soviet Union had invaded Czechoslovakia the night before. Austrian border guards let us pass, and we met confused, worried, scared Czech guards in the buffer zone. They turned us back. Our attempt to cross was shown on national Austrian television that night. By the time I could return to Warsaw, the conference had ended, and there were no direct observations of any effects from the political situation or this letter. After a final interview with Mostowski, I left Poland for 33 years.

I have learned [17] that there were other cancelations besides Robinson’s (and presumably Henkin’s). Mostowski’s feelings were very hurt by people’s reactions. He said “Why me?” to Dana Scott after he canceled, meaning that people were somehow blaming

Mostowski or making him partly guilty. According to Dana, he meant that his whole history before, during, and after the war had nothing to do with anti-Jewish actions in Poland. And he also suffered under communism very much. So Robinson's sensitivity did not outweigh the insensitivity and the pain that Mostowski felt and some who canceled continue to feel. I choose to read courage on both sides.

The next (and last) time I would see Mostowski would be at the 1971 Tarski Symposium that celebrated Alfred's 70th birthday. Henkin participated, offering a talk and paper on cylindric algebras jointly with Don Monk. He is also listed as the principal editor of the published proceedings [11]. He and John Addison hired me as translator of Yershov's contribution and technical copy editor for several other chapters. But his major work was behind the scenes in the organization of this complex conference with visitors from seven countries and many states.

4 The Mal'tsev Book

> One of the aftermaths of Wells's sojourn in Europe was his translation of A.I. Malcev's papers on the metamathematics of algebraic systems, from Russian. Published in the prestigious Studies in Logic series, the volume represents an important contribution by Wells to research on the border between algebra and logic.[3]

The story of how Mostowski guided me to translate and compile this book is recounted in [22, p. 426]. The first three chapters of the Mal'tsev book [15] are of particular interest here because of controversy raised by the joint Henkin–Mostowski review [13] of the original papers in Russian for the second and third chapters and their historical note on the German paper [14] I translated as the first chapter. The controversy involves whether Henkin (and Mostowski) treated the Mal'tsev papers fairly and even whether Henkin should have given more, and more appropriate, credit to Mal'tsev for a compactness theorem anticipating in some sense his own completeness result in his dissertation. Here are some recent comments [23] I wrote to Sol Feferman on this matter.

> Dear Sol,
>
> It seems there are three issues: (1) Did Henkin fail to give credit (or at least acknowledgment) to Mal'tsev? (2) Did Henkin have an on-the-face conflict of interest in the joint Henkin–Mostowski JSL review? (3) Did that bias the review? My Mal'tsev translation of the 1936 German "Untersuchungen" paper ([14] and Chapter 1 of the Mal'tsev book) reveals nothing relevant and merely states there are issues with Skolemization, deferring to the 1959 Henkin–Mostowski review, which is NOT a review of the 1936 paper but two later ones—translated as my Chapters 2, 3. But it includes a historical note dealing with the early paper. (There is also a 1937 JSL review by Rosser of the 1936 paper.) I'm not sure Henkin should be commenting (and that, strongly) on 1936, but he was not reviewing it, very strictly speaking. On the other hand, I am disturbed that he sends the readers to his own proof (or A. Robinson's) for a "satisfactory proof." Maybe one could say that was Mostowski's voice.
>
> In an email exchange with you before the publication of ATL&L [3], you sent me an early version of this passage on p. 306:
>
> > A student of Kolmogorov's in Moscow during the 1930s, Mal'cev had done pioneering work on the applications of logic to algebra. His 1941 paper (in Russian) made use of a compactness theorem for first-order languages allowing uncountably many symbols, which

[3] See [9].

> anticipated much that was done after the war by Leon Henkin and Abraham Robinson. This work was largely unknown in the West until the 1950s; and only in 1971 would Mal'cev's papers become widely available with the publication of a volume of English translations by Tarski's student, Benjamin ("Pete") Wells.

In response, I wrote:

> I have reread my note (2) on page 14 of my book [15] and I have reread my notes and corrections written on the reviews of the 1936 German paper [15, Chap. 1] by Rosser and of the 1941 and 1956 papers [15, Chaps. 2, 3] by Henkin and Mostowski. First, Rosser is plain wrong: he misunderstood "equivalent." I think it is clear that Henkin and Mostowski are giving a generous (and not unjustified) interpretation when they reflect on the nearness of proof in 1936 but I think they are being picky when they say it is not formulated there. It takes no great leap to put results of Section 4 and Theorem 1 together, as I point out in the note (2). They don't flat out deny that Mal'cev proved compactness with arbitrary symbol sets in 1936, but they refer the reader to a "satisfactory" proof in Henkin or Robinson. My own introduction by Tarski to reviewing for JSL (and further experience) tells me there is a little turfing going on here. Who cares?
>
> My impression is that Tarski thought a compactness result was there in Mal'cev. My note (2) refers to "some difficulty" and specifically two types of difficulty. I gather from my notes on the Henkin–Mostowski review that that was my understanding of why the stronger unstated/unproved form of the SNF was required.

[...] —— cites the footnote in Henkin's 1950 JSL article [4, p. 90] on "Completeness in the Theory of Types":

> A similar result for formulations of arithmetic within the first order functional calculus was established by A. Malcev [14]. Malcev's method of proof bears a certain resemblance to the method used above. I am indebted to Professor Church for bringing this paper to my attention. (Added February 14, 1950.)

as if that means he knew about the 1936 paper in advance of his thesis. Of course he knew about it in advance of his 1996 historical article [10] on completeness. [...] Apparently it does not mention Mal'tsev, and that seems to be where the most definite finger can be pointed—given his 1950 footnote and his 1959 review, Leon should have said SOMETHING about the Mal'tsev result in a 30-page paper.

It is worth noting that the Rosser review antedates everything but its subject, Mal'tsev's paper. Although German would be more generally readable, the paper appeared in a Russian journal. But here is a very early reference in JSL. It may be that this review (or more probably arranging it) alerted Church, who was of course reviews editor from 1936 to 1979. [...]

Henkin was a friend, an incredibly valuable and valued advisor on several matters, and a politician. I am surprised to learn that his behavior is questioned so boldly [...], but I cannot defend him specifically on this matter. I do not recall if we ever discussed the Mal'tsev 1936 paper and the reviews, but I am sure Alfred and I did and maybe I spoke to Leon. I am sure I was alerted to the joint review by someone other than my own digging, although I did dig up all reviews (and all other translations) of papers in the book. Unfortunately, that sheds no light on these issues.

My initial answers to the questions above are: (1) Yes, but did he owe credit? In the dissertation probably not, but in the history paper, he should have offered recognition of Mal'tsev's work if only to say he felt it was too flawed to count. Even if he felt that had been clearly put in the joint review, it should have been revisited. (2) Yes. (3) Maybe, but it may still all be true and all in accord with Mostowski's views.

This may contribute to resolving the controversy, but it is not a solution. With the passing of the players, that may prove unsolvable.

5 Mathematics Education and Leon Henkin

Leon spent over 45 years supporting and improving mathematics education through a variety of initiatives, organizations, courses, and projects. My own involvement with Project SEED and Project APT (Alternatives through Peer Teaching) stemmed from a letter Leon wrote graduate students in February 1971.

DEPARTMENT OF MATHEMATICS University of California, Berkeley

TO: Selected graduate students
FROM: Leon Henkin
RE: Part-time positions teaching mathematics in the schools

I am writing to call to your attention the existence of four programs which from time to time have available part-time positions for which graduate students in the mathematical sciences are eligible. All of these positions involve teaching mathematics to pre-college students.

Most graduate students recall their early mathematics training as dull, and perhaps even painful. I think it is fair to say that this widespread feeling, together with the slow pace of mathematics learning in the early grades, is at least in large part due to the fact that teachers in the early grades know very little mathematics, and often dislike—sometimes even fear—grappling with this part of their teaching duties. On the other hand, there is every reason to believe that during those early years school children have an enormous capacity to learn, and can easily be interested in mathematical ideas when presented enthusiastically by competent people. In an attempt to apply these observations, the following programs have been developed to provide supplemental mathematics education for pre-college students at various grade levels by specialized mathematics teachers, over and above the normal mathematics courses for those grade levels taught by the regular teacher.

(1) Project SEED. This program operates in various school districts throughout California (and in a few other states). Instructors must have the equivalent of an A.B. degree with a major in mathematics or a closely related field. Instructors currently earn about $2,000 for teaching one class 40 minutes per day 5 days a week through the school year. Often two classes are available. Most job opportunities start in September, with applicants chosen in the spring or summer; a few jobs open up during the year. Instructors are employees of individual school districts, but beginning March 1, 1971, will be recommended through a University office.

This program was developed by William Johntz ... Instruction is by the so-called "discovery method", wherein no text or syllabus is prescribed, but education proceeds by oral questions and answers. After visiting such a class several times, a potential instructor is invited to try his hand at it. It is normally limited to grades 2–6 (ages 7–12). [...]

(3) The Peer Teaching Program. [Alternatives through Peer Teaching, or Project APT] This has developed on a very small scale as an offshoot of Project SEED; the NSF has just funded a project to begin next fall on the Berkeley campus, through the Special Scholarship Committee of the Academic Senate. Louis Schell [...] will be the Director.

In this program school students normally in the junior high school grades (7–9) are given small-group and individual help to bring them to the point where they can teach mathematics by the discovery method to a group of students either at their own grade level or perhaps one or two years younger. Instructors for this program require qualifications similar to that of Project SEED; they can earn up to $ 5,000 during the next academic year, although a heavier commitment of time is called for. Each instructor will work with 8 "peer teachers" of junior high school age, who will be divided into two groups of 4. The instructor will meet each group for one period each day, and will have to put in a certain amount of additional time with individual peer teachers. Instructors who have not had experience in using the group discovery method of instruction (as in Project SEED) will be required to gain a limited amount of such experience this spring, before they can be employed in this project.[4]

[4] See [6].

I immediately requested interview appointments for both of these positions. Louis Schell conducted the two interviews as one. I began training that spring and was hired for both projects, based on observations of my teaching and reports I wrote. It is likely that Henkin played a role in the hiring, especially with Project SEED. It is worth noting that the Special Opportunity Scholarships office, which was the first home of APT at UC Berkeley, administered a program that served as a model for the federal Upward Bound Program launched a year or so after Henkin founded the former [1].

I taught with Project SEED for a year and with Project APT for nine years.

> At this point in his career, Wells suddenly turned his interest from mathematical research to mathematics education. He was employed on a university project, directed by Louis Schell, which involved him in training ninth-grade "peer teachers" to conduct a complete mathematics class of seventh-grade students twice a week. This project was funded by the NSF [National Science Foundation] during 1971–1973, and then there was a follow-up evaluation year funded by NIE [National Institute of Education]; I was the Principal Investigator for both projects and know that Wells did fine work, and gained invaluable experience, through his involvement. I was then teaching a course in our Department for prospective elementary-school teachers, and I several times asked Wells to bring a peer-teacher and a group of seventh-graders to the University, so that my students—the future teachers—could gain some appreciation for the potential of this unusual mode of instruction.[5]

In particular, Leon was PI for the NIE study [12] on the effectiveness of our peer-teaching program. This showed that under numerous measures, there was no significant loss of performance when 8th and 9th grade peer teachers took over entire scheduled 7th or 8th grade math classes for two of five periods per week. In many of the experiments, there was significant gain. This is why Leon was enthusiastic about showing the method to the future teachers in his classes. He was also a frequent visitor to our peer-teacher-taught classes in the junior high schools.

In an effort to communicate across the entire peer-teaching project, we invited all peer teachers and trainers to a day at UC Berkeley, with a talk by Leon on logic and a swim at Strawberry Canyon. It is interesting that the peer teachers found Leon to be a clear and eloquent explainer but could not grasp why he did not ask more questions, the basis of their own teaching style. I would frequently recall this later in my own college classes when I talked more than I asked. Leon stayed in touch with the program after the first two years of federal support as the school district picked up the funding.

6 Leon's Help with University Administrations

Leon Henkin eagerly provided graduate students his advice and help with administrative issues. When it came to getting anything done, it was always Leon who wrote the letters. For a number of years, he documented that graduate assistants did not need to pay income taxes because he affirmed that their services were those expected of all degree candidates (US income tax guidelines specifically exclude this reasoning now). He held the post of NSF project co-PI and administrator, first chair of L&M, chair of the Department of Mathematics, and president of Association for Symbolic Logic (ASL) at various times.

[5] See [9].

Not only was he a great help with university administrations, he was himself a great administrator.

Leon was the chair or cochair for fifteen students awarded Ph.D.s. Of these, he was sole dissertation committee chair for only two Ph.D. students in mathematics and one in mathematics education, three women named Carol Karp, Diane Resek, and Nitsa Hadar.

> From about 1976, without slackening his work with school mathematics, Wells began to resume work on his doctoral dissertation with Professor Tarski. This is an enormously difficult thing to do—I've seen many dozens of advanced mathematical students drop out of a Ph.D. program with the intent of completing their research while doing other kinds of work, but I know only one other, besides Wells, who succeeded.[6]

Leon and Alfred served as joint chairs for Haragauri Narayan Gupta's dissertation committee. He is not the other successful returnee, because he finished quickly and continuously, but he may be one of the strongest examples of Tarski's tendency to overwork students. Wanda Szmielew's student Zenon Piesyk was also researching geometry based on Tarski's axioms. Tarski and Szmielew drove their two students crazy by repeatedly telling them that one had surpassed the other's results. The consequence was that Gupta's thesis approached a ream in length, well over 400 pages.

> In the fall of 1981 his [Wells's] research was largely completed, and I attended a presentation of his work that he made at our Logic Colloquium. I was very well impressed, both with the quality of his mathematical ideas and findings, and with his very clear and able presentation.[7]

The colloquium date was actually 8 May 1981. After the talk, Henkin thanked me, and Bob Solovay told me that it was very good work. I said I wished Tarski saw it that way, but he wanted much more. Bob said, "I would give you a Ph.D. for your first theorem." I was astounded, grateful, encouraged, and somewhat tempted to jump ship to the Solovay fleet.

By fall 1981, the thesis was coming along but had a way to go yet. In addition, there were several bureaucratic obstacles, primarily the lapsing of my candidacy for the doctoral degree. When the Department tried to have me reinstated, Dean Geoghegan refused, saying I needed to take the oral qualifying exams plus two language tests again. Alfred's attitude was: "I don't see why it is a difficulty to recertify the qualifying exams. I see no need for you to be present." At about the same time, I mentioned my relief that Berkeley did not require oral thesis defenses. Tarski said, "I always liked oral defenses. I will be happy to arrange one for you ... if you like."

In spring 1982, Dean Geoghagen went on leave, and Leon joined the fray by engaging his replacement, Dean Simmons.

May 17, 1982

Benjamin Wells September 1, 2014 9:08 PM
TO: Dean William Simmons, Graduate Division

> At your suggestion, I am following up our phone conversation concerning the reinstatement of Mr. Wells into candidacy for the Ph.D. degree. He was last a registered student in 1967. About one year ago, Professors McKenzie and Lam of this Department wrote to Dean Geoghegan to request reinstatement of Mr. Wells. I enclose a copy of their letter. Subsequently, Dean Geoghegan replied

[6] See [9].

[7] See [9].

> to Mr. Lam, declining to reinstate Mr. Wells. He stated that it would be necessary for Wells to take new qualifying exams, have his languages recertified, and file a new advancement to candidacy.
>
> In our conversation, I inquired as to the reason for requiring the (former) student, and a group of faculty examiners, to undertake the time and effort necessary to follow through the steps outlined by Dean Geoghegan. You replied that the purpose was to ensure that the candidate is still as knowledgeable and capable mathematically as he was at the time of his original examination. I then pointed out that the letter from McKenzie and Lam gave strong evidence, in the form of a list of the continuing mathematical activities and publications by Mr. Wells, to show that his earlier level of attainment had not fallen into neglect. However, Dean Geoghegan's letter continued with the sentence, "No matter what Mr. Wells has been doing in the interim, sixteen years is just too much." As I pointed out, this statement is not consistent with the explanation you gave for requiring a former student to retake examinations.
>
> Mr. Wells' dissertation adviser is Professor Emeritus Alfred Tarski, whose exacting standards are well-known in our Department. Professor Tarski has informed me that during the year Wells continued to make further improvements on his dissertation, and Tarski now considers that it is completely in order and deserving of the degree. For this reason, we are anxious to proceed to have the degree issued as expeditiously as possible.[8]

On 28 May, Simmons reinstated my candidacy, thanks wholly to Leon's intervention. The candidacy was valid until the fall thesis-filing deadline of 12 November 1982. Geoghegan returned to duty in fall 1982, so it became clear that this deadline was indeed drop dead. Simmons required me to register for fall classes, and although Tarski reluctantly supported a filing-fee waiver of registration on 30 September, Louise Kerr in the Graduate Division office pointed out that Geoghegan would have to approve it, so if I was smart I would register. That bought me 12 units of credit, which I asked Manuel Blum of Computer Science to grant me for time served on Turing machines. Manuel had been a fellow student in a topology course at MIT. He agreed; he was about to become much more involved.

The Logic and Methodology of Science Group as well as those associated with logic in the Department of Philosophy and the Department of Mathematics participated enthusiastically in the annual Logic Picnic held at a Berkeley or regional park, frequently Codornices Park across Euclid Avenue from Berkeley's Rose Garden. Leon was usually a gregarious host at this communal affair. It was at the picnic in fall 1982 that I learned Bill Craig was on sabbatical in England, possibly from Leon. That was a big surprise because Craig was the "outside" member of my dissertation committee, and he was due to get a near-final draft in a few days. I knew how to reconstitute the committee because I had just arranged for Ralph McKenzie to replace Gerhard Hochschild, who had no memory of even being on it after 18 years. I wrote Tarski on 3 October, developed a short list of candidates, reviewed it with him, and happily agreed with his proposal of Manuel Blum, who joined the reformed committee!

Leon was involved in many of these adjustments. His greatest gift came with the filing of the dissertation and is discussed in the next section.

During my three sabbatical leaves from the University of San Francisco, I was a Visiting Scholar in Mathematics at UC Berkeley (1989–1991, 1997–1998, 2004–2006). In each case, Leon served as the official sponsor or as a supporter of the appointment. The first sabbatical spanned an ASL meeting at UC Berkeley. As usual, Leon helped with the organization of the conference. He also attended my talk there, "Infinity on Purpose."

[8] See [7].

He thought it was humorous, a need at such meetings. I thought both judgments were positive.

Around that time he served on the dissertation committee for Art Quaife, chaired by John Addison. Quaife had arrived at UC Berkeley the year after John and I did. He quickly acquired a pet lion and then wrote a theorem prover for Stephen Cook. He soon dropped out in favor of joining the cryonics growth industry. Many years later, he consulted Addison on the state of automated theorem proving, suggesting he could start a prover on the Goldbach Hypothesis, be on ice for several centuries, and have the proof after a reconstitution. Addison guided him discreetly and indirectly toward doctoral-level results. John made a point of telling me at the ASL conference's social event, "Your record of taking nearly 20 years to finish your doctorate is broken. Art Quaife has completed a thesis to my satisfaction. I'm telling you before I tell him, because he has taken 26 years!"

I was Alfred Tarski's last student, in the sense that my dissertation was the last he signed. He did not write me a reference letter largely because of scientific projects he devoted his waning energy to completing before his death.

> Professor Tarski, his dissertation supervisor, was known as a teacher who holds his students to the very highest standards. Despite his expressed satisfaction with Wells's work [in spring 1982], he asked Wells to polish some of the writing, and try to resolve one or two related questions for inclusion in the final dissertation. When this was completed to specifications, the degree was finally awarded in December 1982. Meanwhile, Professor Tarski's health began to weaken seriously and he passed away in October 1983. Throughout the previous year he had great difficulty in speaking with people, or in writing. For this reason, he was unable to write on behalf of Wells, as I know he would otherwise have done.[9]

Leon wrote me many letters, and this one, substituting for the traditional Professor's Letter, was the kindest.

7 The Henkin Plan and the Dissertation

Henkin's most critical, most helpful contribution to my scientific life was what came to be called the Henkin Plan for completing my degree requirements by filing a dissertation.

> 11 November 1982
>
> To: Dean, Graduate Division
> Fr: Leon Henkin
> Re: Benjamin F. Wells, III.
>
> Mr. Wells is a candidate for the Ph.D. degree in this Department. His work, and he as an individual, are well known to me.
>
> His dissertation supervisor is Professor Emeritus Alfred Tarski. The latter told me, as long ago as last summer, that he was satisfied with Wells's dissertation and its writing, although he had asked the candidate to make small changes in the Introduction and in the Abstract. This Wells did, approximately two months ago. However, Professor Tarski has not been able to look at the work he requested of Wells, or to sign the title page of the dissertation. The reason is that Professor Tarski is desperately ill.
>
> I use the word "desperately" advisedly. In mid-September, just before leaving for Europe, I visited Tarski and found him very frail. At that time his wife told me that he found it impossible

[9] See [9].

> to sleep, except for dozing in a chair for 20 minutes at a time, several times in each 24 hours. Since my return from Europe 10 days ago I have learned that Tarski was hospitalized for 2 days in October and 4 days last week, because in addition to being unable to sleep, he has the greatest difficulty in eating. I have tried to talk to him on the phone each day since my return, but each time his wife returns to tell me that he feels too weak to speak on the phone.
>
> Under these circumstances, I recommend that Mr. Wells's Ph.D. Committee be re-constituted, so as to replace Prof. Tarski. Indubitably the person most appropriate to serve as Chairman would be Professor Ralph McKenzie, already a member of the Committee. Other members of the Department who are familiar with Wells's work and could serve on the Committee include Professor Julia Robinson, and myself. Tomorrow is the last day before Wells's candidacy lapses, and I feel that in the light of all the circumstances of his case, it would be grossly unfair to delay his degree because of Professor Tarski's illness.[10]

Only changes in the abstract and introduction needed to be verified, but Tarski had delayed this for weeks. Leon had recently returned from Yugoslavia. He was outraged to hear how Tarski was dragging this out toward the deadline of my candidacy lapse, Friday, 12 November 1982. On Wednesday, 10 November, he proposed what I called the Henkin Plan: (1) he would personally urge Alfred to sign the dissertation immediately; (2) if Tarski were too ill to consider that, then Henkin would sign Tarski's name, initialed LH, as long Tarski approved this; if both of these failed, then (3) Tarski would be removed as a member of the committee, Ralph McKenzie would succeed as chair, and Julia Robinson would join the committee, with Manuel Blum of Computer Science as outside member in all cases. While changing the committee members required only an advisor's signature, changing the chair was more complex—hence the letter above. Adding Henkin to the committee was not considered because Julia had already read a recent draft, and Leon's familiarity with the results dated from the Logic Colloquium talk in May 1981.

Before Julia would sign (3), she told me that I had to guarantee her that someone on the new committee had read every page—she knew it had grown longer, and she said her scientific reputation was at stake. I went straight to Ralph and told him that I had added about 30 pages at Tarski's direction and he must read them immediately, to make my promise to Julia good. At first refusing, he reluctantly agreed to do it within three hours. Later he said the material looked fine, but there were several typos—I never found them.

The next 24 hours are hard to relive. Some reference to them appears in [3, p. 375], but the actual events were more bizarre than discernible there. The amazing story of Tarski's eventually signing the title page I brought to his house early on 12 November 1982, but his writing the date as 11 November is recounted in [20]. One aspect was absent there. I had Tarski's signature at the last moment, but Blum had refused to sign until Tarski did, a deference that did not extend to the pages with Leon's "Alfred Tarski LH" or Ralph's signatures, which he gladly signed ahead of time. Manuel and Lenore were going to a movie, and I asked him what I should do if Tarski signed and they had already left. I followed Manuel's advice (not part of the Henkin Plan), and—omitting details—it eventually involved my headlight.

Louise Kerr at Graduate Division kept the office open past 5 pm till I could turn in the completed dissertation and multiple copies. In fact, her building, California Hall, was locked up, but one massive metal door was jogged out a quarter inch. I grabbed that edge, prayed and pulled it open, and entered. By then I had signed dissertation title pages for

[10] See [8].

all three committees, but of course only the first was used. Henkin's plan had succeeded in the best way!

It seemed to me very likely that Leon's intercession at (1) was crucial, that essentially and maybe solely because of Leon Henkin, Tarski signed on time. My gratitude to both of them was boundless. But this impression is not final or complete. After all, the letter above indicates Alfred would not take Leon's earlier calls. My friend Jim, who advised me and recommended implementation of the Henkin plan, said on 11 November, "We have done all that is humanly possible." I took it as a sign that greater assistance might be needed. and might be given—it always may be, kindly, justly. How else to explain the 11 November dating?

The day after Alfred signed the thesis, he told Leon, "I did not know Wells had such a strict deadline." (I had told him often.) Tarski told Steve Givant then, "If I had felt better, I would have made Wells work longer."

8 Conclusion

Leon Henkin led a lifetime of loving service to mathematics and education, to students and teachers. The Leon Henkin Citation for Distinguished Service is awarded by the Committee on Student Diversity and Academic Development of the Berkeley Division of the Academic Senate. It is given in recognition of an "exceptional commitment to the educational development of students from groups who are underrepresented in the academy." This award was first given in 2000, and the first laureate was Leon Henkin [1].

The near decade I spent working in school programs created and shepherded by him taught me the value of this contribution, beyond his staggering influence on mathematical logic and algebraic logic. This review of connections with Henkin has shown me how vital, how free, how generous was his help in so many ways for so many years.

Thousands of children, young college students, teachers in training, women, minority members, doctoral candidates, and many others received opportunities for better education and better mathematics education through Leon's decades of effort—a magnificent benefit!

References

1. Addison, J., et al.: In memoriam: Leon Albert Henkin (2006). http://senate.universityofcalifornia.edu/inmemoriam/leonalberthenkin.html
2. Addison, J., Henkin, L., Tarski, A.: The Theory of Models. North-Holland, Amsterdam (1965)
3. Feferman, A.B., Feferman, S.: Alfred Tarski: Life and Logic. Cambridge University Press, Cambridge (2004)
4. Henkin, L.: Completeness in the theory of types. J. Symb. Log. **15**, 81–91 (1950)
5. Henkin, L.: Logical systems containing only a finite number of symbols. Séminaire Mathématiques Supérieures, Université de Montréal (1967)
6. Henkin, L.: Letter to selected graduate students (February 1971)
7. Henkin, L.: Letter to Dean William Simmons, Graduate Division, University of California (17 May 1982)
8. Henkin, L.: Letter to the Dean, Graduate Division; dictated by phone to Jim's friend Shireen Kaul and signed in Henkin's name and his own by Morris Hirsch, Chair of Mathematics, University of California, Berkeley (11 November 1982)

9. Henkin, L.: Letter to whom it may concern. Reference letter for Benjamin Wells (22 January 1984)
10. Henkin, L.: The discovery of my completeness proofs. Bull. Symb. Log. **2**, 127–158 (1996)
11. Henkin, L., et al.: Proceedings of the Tarski Symposium. Proceedings of Symposia in Pure Mathematics, vol. 25 (1974)
12. Henkin, L., Amster, H., Schell, L.: Experimental investigation of peer teaching (1975). Final Report. http://eric.ed.gov/?id=ED139753
13. Henkin, L., Mostowski, A.: Review of A. Mal'cev, On a general method for obtaining local theorems in group theory (Russian), and A.I. Mal'cev, On representations of models (Russian). J. Symb. Log. **24**, 55–57 (1959). Includes an historical note concerning [14]
14. Mal'tsev, A.I.: Untersuchungen aus dem Gebiete der mathematischen Logik. Mat. Sb. N.S. **1**(43), 323–336 (1936)
15. Mal'tsev, A.I.: The Metamathematics of Algebraic Systems—Collected Papers: 1936–1967. North-Holland, Amsterdam (1971)
16. Robinson, A.: Letter to Leon Henkin on Association for Symbolic Logic letterhead with copies to Martin Davis, Dana Scott, and Alfred Tarski (14 May 1968). BANC MSS 84/69, Alfred Tarski papers, courtesy of The Bancroft Library, University of California, Berkeley; also by permission of Martin Davis and Dana Scott
17. Scott, D.: Private communication (2014)
18. Wells, B.: Pseudorecursive varieties and their implications for word problems. Ph.D. Dissertation, Univ. of California, Berkeley, CA (1982)
19. Wells, B.: Applying, extending, and specializing pseudorecursiveness. Ann. Pure Appl. Log. **126**, 225–254 (2004)
20. Wells, B.: The triumph of the One. In: Sarhangi, R., Sequin, C. (eds.) Bridges: Mathematical Connections in Art, Music, and Science; Bridges 2004 Conference Proceedings, pp. 103–108 (2004)
21. Wells, B.: Alfred Tarski, friend and daemon. AMS Not. Am. Math. Soc. **54**, 982–984 (2007)
22. Wells, B.: The prince of logic. In: Ehrenfeucht, A., Marek, V.W., Srebrny, M. (eds.) Andrzej Mostowski and Foundational Studies, pp. 421–428. IOS Press, Amsterdam (2008)
23. Wells, B.: E-mail to Sol Feferman (15 February 2014)

B. Wells (✉)
Departments of Mathematics and of Computer Science, College of Arts and Sciences,
University of San Francisco, 2130 Fulton St., San Francisco, CA 94117, USA
e-mail: wells@usfca.edu