A Tribute to Henry Helson

Donald Sarason

Henry Helson, a leading figure in harmonic analysis whose ideas have had an enormous impact, died on January 10, 2010. Born on June 2, 1927, Henry grew up in Bryn Mawr, where his father Harry was a professor, carrying out the research that would make him one of the eminent psychologists of his era. His mother Lida, despite her upbringing as a fundamentalist Lutheran, found her way to the Quaker faith when Henry and his sister Martha were children. All three of them became and remained committed Quakers.

Henry's exceptional mathematical abilities shone through early. Although his father Harry hoped Henry would go into physics, Henry was hooked on mathematics. He entered Harvard as an undergraduate, graduating in 1947. His education had been interrupted for thirteen months by military service, which for him, as a Quaker, was a loathsome experience. On the eve of his graduation Henry was awarded a Harvard traveling fellowship. The fellowship enabled him to fulfill an intense desire he had nurtured to visit Europe. In the academic year 1947–1948 he visited London, Paris, Prague, and Vienna, but he spent most of the year in Poland, first in Warsaw, then in Wroclaw.

Why Poland? The circumstances are explained in Henry's essay [5]. He writes: "… [I] was crazy to see the destruction caused by the war in Europe.… The most desperate place in Europe seemed to be Poland, and there were mathematicians in Poland." Another passage from the same essay reads:

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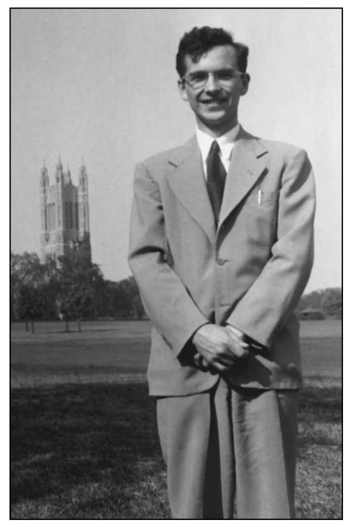


Young Henry, location and date uncertain.

> In college I had spent much time browsing in the mathematics library, and even in the stacks, and I found *Fundamenta Mathematicae* and *Studia Mathematica* of special interest. There were magic names in them: Sierpiński, Steinhaus, Kuratowski, Ulam, Ostrowski, Szegö, Borsuk, Tarski, and of course Banach, and a name that appeared in two forms, sometimes as Szpilrajn and sometimes as Szpilrajn–Marczewski, which I could not explain. Many papers seemed related, many were joint work, all were quite short. I could

not understand most of them, but I tried.

In spring 1948 Henry was in Wroclaw. He writes:

> Szpilrajn had become Marczewski by this time. In Polish law, you had to keep both names for a while if you wanted to change. I suppose that Szpilrajn was a Jewish name. Marczewski took me in hand and gave me good problems, in analysis, to work on. I solved two of them and they made my first publications, and he was pleased. (So was I.)

The publications referred to are [2] and [3], which appeared in *Studia Math.* and *Colloquium Math.*, respectively.

In fall 1948 Henry enrolled in the Harvard mathematics Ph.D. program. His thesis advisor was Lynn Loomis, but by happenstance Arne Beurling turned out to be the source of his mathematical inspiration. In an essay that did not receive wide circulation [6], Henry writes:

> When I came back to Harvard in 1948 Arne Beurling came to visit the department. His course looked hard and unrewarding but my mentor, Lynn Loomis, told me I had to take it. Of course the department had to get together some students to listen to Beurling! So I went, and that set my subsequent mathematical trail.... Beurling was full of wonderful ideas, and I soaked them up. In this time I was the twin of my friend John Wermer, whom I had known for years as an undergraduate. At the conclusion of this year, John went back to Uppsala with Beurling; I stayed another year to write my thesis.

Henry received his Ph.D. in spring 1950 but was late getting on the job market. An offer from UCLA eventually came, but this was during the California loyalty oath controversy; all University of California faculty were required to sign the oath, which Henry would not do. He wrote to Wermer in Uppsala, requesting that John ask Beurling if he could support another student. A positive answer came back, so Henry spent the academic year 1950–1951 in Uppsala, his stay there interspersed with trips elsewhere in Europe. One trip was to Nancy, France, which then housed Laurent Schwartz, Jean Dieudonné, Roger Godement, and Alexander Grothendieck (then a student), among others. The Nancy trip resulted in the offer of a fellowship for the following year, but Henry's parents prevailed on him to stay in the United States. He accepted an instructorship at Yale and was eventually promoted to assistant professor. However, Yale at that time, despite housing an impressive group of young nontenured mathematics faculty, regarded their positions as strictly temporary. In 1955 Henry accepted an assistant professorship offer from Berkeley. (The loyalty oath had by then been declared unconstitutional.) Henry remained at Berkeley for the remainder of his career, becoming associate professor in 1958, professor in 1961, and emeritus professor in 1993.

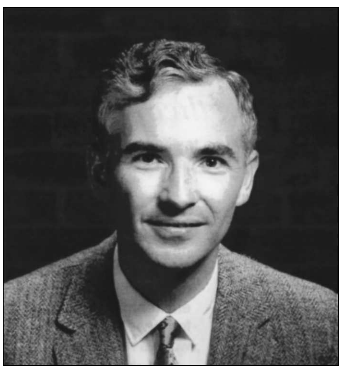
Henry carving turkey, Berkeley, 1950s.

Henry, Ravenna, and children Ravenna, David, and Harold (on Ravenna's lap), Berkeley, 1960.

Henry and his wife Ravenna were married in 1954. Ravenna had received a Ph.D. in psychology from Berkeley and was eager to return there, which enhanced Berkeley's intrinsic attractions for the couple. They raised three children, born between the years 1956 and 1960. Ravenna had a distinguished career as research psychologist at Berkeley's Institute of Personality and Social Research, including serving as director of the Mills Longitudinal Study, which she founded.

Henry's early research, which quickly gained recognition, is discussed below by Izzy Katznelson and John Wermer. I'll dwell here a bit on Henry's two most influential papers, [8] and [9], coauthored with David Lowdenslager. Bill Arveson will describe how those papers were instrumental in the development of the theory of nonself-adjoint operator algebras, a direction likely not envisioned by the paper's authors. The pieces by Kathy Merrill and Jun-ichi Tanaka concern another direction initiated in [8] and [9], involving invariant subspaces.

As the titles of [8] and [9] inform us, the papers arise from certain questions in probability. Attached to a stationary stochastic process with discrete time is its so-called spectral measure, a positive Borel measure μ on the unit circle. Certain questions about the process can be recast as questions about the geometry of the (complex) Hilbert space $L^2(\mu)$. Thus questions in probability are transformed into questions in function theory. Prior to the work of Helson and Lowdenslager, a number of those questions had been answered through the work of, for example, Gabor Szegö, Norbert Wiener, Andreĭ N. Kolmogorov, and Mark G. Kreĭn. Helson and Lowdenslager propelled this theory in two new directions.



Henry, early 1960s.

First, Helson and Lowdenslager replaced the classical one-parameter stationary stochastic processes referred to in the preceding paragraph by multiparameter stationary stochastic processes of a certain kind. In those processes, the unit circle from the classical theory is replaced by a compact abelian group B dual to a dense discrete subgroup of $\mathbb{R}$—a torus of dimension larger than 1, possibly of infinite dimension. The Bohr compactification of $\mathbb{R}$, the dual of the entire discrete real line, is a prime example. Fourier analysis on B leads to the Fourier series in several variables of the papers' titles.

Helson and Lowdenslager established versions in their setting of some basic theorems from the classical setting, for instance, Szegö's theorem from prediction theory and the famous theorem of F. and M. Riesz on analytic measures. This required new ideas, because Helson and Lowdenslager did not have available the classical techniques.

While Helson and Lowdenslager emphasized the group setting in which they worked, it was quickly realized that their ideas extended far beyond that context. In particular, the ideas were employed by people working in the then emerging theory of function algebras, providing decisive impetus. The ideas also suggested new ways to approach the study of Hardy spaces in the unit disk, ideas adopted by Kenneth Hoffman in his well-known book [11].

Helson and Lowdenslager's F. & M. Riesz theorem attracted particular attention. Earlier, Solomon Bochner [1] had established a generalization of the original F. & M. Riesz theorem in which the circle is replaced by the two-torus. A maximal theorem underlies Bochner's method, which can be applied to give a proof of the original F. & M. Riesz theorem, a proof different from the several that had been previously found. Bochner's F. & M. Riesz theorem is a very special case of Helson and Lowdenslager's theorem, which they proved with what were at the time completely new methods, an elegant blend of function theory and operator theory. Those methods can be applied to give yet another and a remarkably simple proof of the original F. & M. Riesz theorem.

In a second direction, Helson and Lowdenslager studied multivariate stochastic processes, replacing the scalar-valued processes from the classical theory by vector-valued processes. This led to questions about factoring matrix-valued functions on the unit circle and corresponding new results. This part of their work hooks up with what eventually became the theory of operator models of Belá Sz.-Nagy and Ciprian Foiaș, a central part of operator theory.

The brief remarks above are but a very rough sketch of the basic papers of Helson and Lowdenslager. The papers are too rich to review adequately in a few lines. They are well worth studying even today; in them one will find the seeds of much of Henry's subsequent research.

David Lowdenslager died tragically in September 1963 at the age of thirty-three. This must have been a terrible blow to Henry. But Henry never spoke of it to me, and of course I never pressed him to do so.

As anyone who has read some of his papers knows, Henry was a masterful expositor. His style is exceptionally clear, economical, vigorous, and fluid, altogether worthy of emulation.

In 1992 Henry started his own publishing firm, Berkeley Books, motivated by his distaste for our present-day crop of calculus textbooks, by the high cost of mathematics books at all levels, and also by what he terms "a passion for entrepreneurial activity". Berkeley Books was a one-man operation except for the actual printing of the prepared text, which Henry contracted to a local printer. He published mostly his own textbooks, but also two by Berkeley colleagues (George Bergman and me). Clearly, Henry's overhead was far below that of mainstream publishers, but also clearly, Henry was at a severe disadvantage when it came to marketing his products. Henry describes his experiences as a publisher in [7]. As he states there, Berkeley Books finished in the black every year (at least until [7] was written).

Mathematics was not Henry's only passion. Another was music. He was an accomplished violinist and violist, and after his retirement, freed from many prior responsibilities, he practiced daily for two hours to enhance his skills. He joined a local music group, Amphion, and performed publicly, especially with a talented and congenial quartet he assembled. Henry's last performance, at Amphion, and the last time he played the violin, was in September 2009, just a few months before his death. He bravely fought off the terrible effects of his disease and chemotherapy and performed Brahms's First Violin Sonata.

Travel was another of Henry's passions. He was keenly interested in other places and other cultures. While on the Berkeley faculty he spent sabbaticals in Sweden, Orsay, Ghana, Montpellier, Florence, Bonn, Marseille, and India. India held a special fascination for Henry; between 1980 and 2000 he made four extensive visits there.

My own indebtedness to Henry is profound, both for the mathematics I learned from him and for his support and kindness during our forty-six years as colleagues. I have illustrated the last remark elsewhere [13], but the story will bear repeating here, in abbreviated form.

The story involves a fundamental theorem of Zeev Nehari about Hankel operators [12]. For present purposes, a Hankel operator is an operator on $\ell^2(\mathbb{Z}_+)$ induced by a Hankel matrix, a complex matrix with constant cross diagonals (the $(j,k)^{th}$ entry depends only on $j+k$). Nehari's theorem answers the question of when such an operator is bounded. The answer, in qualitative form, is that boundedness happens if and only if the matrix entries in the first column are the forward Fourier coefficients of a function in L^∞ of the circle. The quantitative version adds that the L^∞ function can be so chosen that its norm equals the norm of the operator.

As a by-product of his joint paper with Szegö [10], Henry found an elegant and insightful proof of Nehari's theorem using a duality argument facilitated by a factorization result of Frigyes Riesz for functions in the Hardy space H^1. Henry showed me his proof in fall 1965, during one of our frequent mathematical chats. The following day it suddenly dawned on me that Henry's technique was exactly what I needed to complete the proof of a theorem I had been working on for two years. The paper that grew out of this breakthrough [14], I would guess, was a big help in my getting tenure at Berkeley, as was no doubt Henry's support behind the scenes.

Henry's final paper [4] appeared recently in *Studia Mathematica*, sixty-two years after his first paper appeared there. In it, Henry addresses the question of whether Nehari's theorem can be generalized from its original one-dimensional setting to an infinite-dimensional setting. He surmises that the answer is negative, and he maps out a possible way to produce the needed counterexample. At the end he states: "But we cannot go further."

The paper was submitted on October 20, 2009, less than three months before Henry died. He must have feared death was imminent and known that he had better tell other harmonic analysts what he knew before it came to pass.

Henry is survived by his wife Ravenna, sister, Martha Wilson, daughter Ravenna Helson Lipchik, sons David and Harold, and three grandchildren.

(I was guided in composing the preceding remarks, in addition to the sources cited, by a sketch of Henry's life distributed at his memorial service on February 20, 2010, at the Berkeley Friends Meeting Hall, and by advice from Ravenna, Steve Krantz, and several of my coauthors.)

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Y. Katznelson

Some of Henry's Early-Fifties Work

Much of Henry's work in the early 1950s dealt with two problems that, retrospectively, turned out to be very closely related.

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1) The description of homomorphisms of group algebras (in terms of homomorphisms of the underlying groups).

Beurling and Helson [5] prove that automorphisms of $L^1(\mathbb{R})$ are trivial, i.e., given by a linear change of variables on $\hat{\mathbb{R}}$ and, using some earlier results of Beurling and Henry's paper [4], show:

If G and H are locally compact abelian groups at least one of which has a connected dual, and if their group algebras are isomorphic, then G and H are isomorphic.

2) The description of idempotent measures (under convolution) on locally compact abelian groups.

A theorem of Szegö [2], states that if a function $f = \sum a_n z^n$ is holomorphic in the unit disc and admits some analytic continuation across some arc, and if its Taylor coefficients $\{a_n\}$ assume only a finite number of distinct values, then f is rational: $f(z) = P(z)/Q(z)$, with all the zeros of the denominator on the unit circle. The conclusion is equivalent to the statement that the sequence $\{a_n\}$ is ultimately periodic to the right.

Henry [1] extended the theorem, showing that the condition "holomorphic f with some analytic continuation" is too strict and can be replaced by a "harmonic function $u(r,t) = \sum_{-\infty}^{\infty} a_n r^{|n|} e^{int}$ for which there exists an arc (α, β) on the unit circle such that $\int_\alpha^\beta |u(r,t)|dt$ is bounded for $\{r : r < 1\}$". The statement now is that if the coefficients assume only a finite number of distinct values, then the sequence $\{a_n\}$ is ultimately periodic to the right and ultimately periodic to the left.

In the special case, which Henry proved in 1953 [6], the arc (α, β) is the entire circle, the boundedness of $\int_{\mathbb{T}} |u(r,t)|dt$ implies that u is the Poisson integral of a measure μ on the unit circle whose Fourier coefficients $\hat{\mu}(n) = a_n$ assume only a finite set of distinct values,

The stage is now taken by measures whose Fourier coefficients assume only a finite number of distinct values, and in particular idempotent measures (whose Fourier coefficients are necessarily 0 or 1).

The ultimate periodicity of $\hat{\mu}$ is shown to imply that μ is the sum of a discrete measure carried by a finite subgroup of the circle and an absolutely continuous part whose density is a trigonometric polynomial.

This description of idempotent measures (on $\mathbb{T}$) was the basis for Rudin's work on isomorphisms of group algebras and paved the way for Cohen's eventual complete description of idempotent measures (on general groups) and solution of the homomorphism problem.

Helson Sets

Another theorem that Henry proved in 1953 [3] inspired much interest and research activity. It has to do with perfect sets $E \subset \mathbb{T}$ (the circle group) that have the property that every continuous complex-valued function on E is the restriction to E of an absolutely convergent Fourier series. (Such sets are now commonly referred to as *Helson sets.*) Henry proved that these are *sets of uniqueness for measures,* that is, a nontrivial measure μ carried by such sets cannot have its Fourier-Stieltjes coefficients $\hat{\mu}(n)$ tend to zero as $n \to \infty$.

Top, left to right: Hans Lewy, Ravenna, Henry; bottom, left to right: Harold Helson and Michael Lewy. Drakes Bay near Berkeley, early 1960s.

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John Wermer

Henry Helson's work has played a central role in modern harmonic analysis and its applications since the 1950s.

In [1] Henry proved a conjecture of Steinhaus. The conjecture states that if the partial sums of a trigonometric series are all nonnegative, then the

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coefficients of the series tend to 0. Henry in fact proved something stronger: If the trigonometric series $\sum_{-\infty}^{\infty} a_n e^{inx}$ satisfies

$$\sup_{N>0} \int_0^{2\pi} \left| \sum_{n=-N}^{N} a_n e^{inx} \right| dx < \infty,$$

then $a_n \to 0$ as $|n| \to \infty$.

Another early theorem concerns the Banach algebra A of Fourier–Stieltjes transforms $f(t) = \int_{-\infty}^{\infty} \exp(itx) d\sigma(x)$ with $\|f\| = \int_{-\infty}^{\infty} |d\sigma(x)|$, where σ is a finite measure on $-\infty, \infty$. The problem is to find all $f \epsilon A$ such that $\|f^n\|$ is bounded for $-\infty < n < \infty$. The obvious such f are the functions $\exp(a + bt)i$, where a, b are constants. Are these all? Henry worked on this question jointly with Arne Beurling during his stay in Uppsala in 1950–51. Making use of a very ingenious argument Henry found back in the States, they jointly showed that the answer is Yes [2].

In a related paper [3], Henry classified isomorphisms of group algebras of certain abelian topological groups.

During the next decade, Henry studied the following problem: Let μ be a finite positive measure on $[0, 2\pi]$. Form the weighted L^2 space on this, with weight μ. Under what conditions on μ is the conjugation operator on this space a bounded operator? Jointly with Gabor Szegö, Henry found a necessary and sufficient condition on μ [4]. This result has been important in further developments in real analysis.

Moving to a different terrain, Henry and Frank Quigley made a contribution to the study of function algebras. They gave some results on maximal function algebras and found conditions on a smooth closed curve γ in C^n in order that every continuous function on γ can be uniformly approximated by polynomials in the complex coordinates [5].

Among Helson's most influential papers were [6] and [7], written jointly with David Lowdenslager. They deal with a generalization of classical harmonic analysis from the circle to a class of compact abelian topological groups. Observe that the integers Z form a discrete, ordered abelian group with dual group the circle Γ. The characters of Γ are the functions $\exp(in\theta)$ on Γ, labeled by n in Z. Using only half of the characters, we can form functions $\sum_0^{\infty} c_n \exp(in\theta)$, whose Fourier coefficients with negative index are 0. Such functions are boundary functions of holomorphic functions in the unit disk. The closure of these functions in the L^p norm on the circle, using the measure $d\theta$, are the Hardy spaces H^p. A beautiful, powerful theory of these Hardy spaces was developed in the 1920s and 1930s, based on the classical theorems of Hardy, F. and M. Riesz, Szegö, Nevanlinna, Beurling, and others.

In the 1950s people started to extend Fourier series theory by replacing the circle group by a compact abelian group G with suitably ordered dual group and using the characters χ_λ on G, labeled by λ in the dual group: the counterparts of the series $\sum_0^{\infty} c_n \exp(in\theta)$ on Γ are the series $\sum_{\lambda \geq 0} c_\lambda \chi_\lambda$, where $\lambda \geq 0$ is taken in the sense of the ordering on the dual group of G.

Using Haar measure on G to replace the measure $\frac{1}{2\pi} d\theta$ on Γ, one then defines H^p spaces on G to replace the Hardy spaces on Γ. An example is provided by taking G to be the 2-torus, with dual group the group $Z + Z$, and $(p, q) > 0$ in $Z + Z$ if (p, q) lies above the line $n + \alpha m = 0$, where α is a fixed positive irrational number. The characters χ_λ now are the functions $\exp(in\theta)\exp(im\phi)$ on the torus G.

Helson and Lowdenslager succeeded in generalizing classical H^p theory to this new context in a remarkably elegant and complete way. Their work had important consequences both for stochastic processes and for the study of a class of commutative Banach algebras, the "sup norm" algebras. Kenneth Hoffman in [8] was able to use their ideas to make significant progress in this field.

Henry with violin bow, Berkeley, 1980s.

I myself came to interact with their work in my effort to solve a problem in the theory of sup norm algebras posed by the work of Andrew Gleason in [9]. Gleason had introduced the notion of "parts" in the maximal ideal space of a sup norm algebra and conjectured the existence of complex analytic structure in parts that are not a single point. I was able, using methods from the Helson–Lowdenslager theory, to prove this conjecture for the case of "Dirichlet" algebras. These are sup norm algebras on a compact space X such that the real parts of the functions in the algebra are uniformly dense in the real continuous functions on X [10].

My own personal connection with Henry goes back a long way. We were classmates in the Harvard class of 1947, and for some years in the early 1950s, we were fellow instructors at Yale. In that period we shared an apartment in New Haven. Henry was a quite competent cook, and I was a loyal assistant, and we ran some quite successful dinner parties. In the spring of 1952 I got married, and Christine and I needed an apartment. Henry, very generously, was willing to move out and allow me to trade apartment mates. He also said to me at that time, "You are lucky, John. She thinks you are funny." I took it as a compliment.

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William A. Veech

Henry Helson was a wonderful human being. An invitation to contribute to this memorial article immediately brought to mind a very important episode from the early part of one career.

After spending a first academic year, in this case 1966–67, in the legendary permanent temporary building, T4, a newcomer to Berkeley would be relocated to Campbell Hall, as often as not in the office of a permanent faculty member on leave for the year. In 1967–68 Jack Feldman's office was made available thanks to his being on leave for a year in Russia. However, Jack cut short his leave and returned to Berkeley at midyear with the prospect of being cramped in a small office with a visitor. While Jack accepted this imposition with grace, Henry Helson saved the day with an unexpected invitation to join him in his much larger office. This was a little disconcerting to a young mathematician who barely knew Henry and who, having read Helson, especially Helson-Lowdenslager, in his graduate school days, had viewed Henry as something of a hero.

During the several months of joint officing, my "hero" became a great friend. Henry offered constant encouragement to an officemate who was absorbed, even fixated, with a problem about $\mathbb{Z}_2$-skew products. The relaxed atmosphere created by daily conversations with Henry about all matters, mathematical and nonmathematical, probably contributed as much to the solution as did my own efforts. Thank you and Godspeed, Henry!

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William Arveson

Helson and Subdiagonal Operator Algebras

Commutative Origins

Henry Helson is known for his work in harmonic analysis, function theory, invariant subspaces, and related areas of commutative functional analysis. I don't know the extent to which Henry realized, however, that some of his early work inspired significant developments in noncommutative directions—in the area of nonself-adjoint operator algebras. Some of the most definitive results were obtained quite recently. I think he would have been pleased by that—while vigorously disclaiming any credit. But surely credit is due; and in this note I will discuss how his ideas contributed to the noncommutative world of operator algebras.

It was my good fortune to be a graduate student at UCLA in the early 1960s, when the place was buzzing with exciting new ideas that had grown out of the merger of classical function theory and the more abstract theory of commutative Banach algebras as developed by Gelfand, Naimark, Raikov, Silov, and others. At the same time, the emerging theory of von Neumann algebras and C^*-algebras was undergoing rapid and exciting development of its own. One of the directions of that noncommutative development—though it went unrecognized for many years—was the role of ergodic theory in the structure of von Neumann algebras that was pioneered by Henry Dye [Dye59], [Dye63]. *That* Henry would become my thesis advisor. I won't say more about the remarkable development of noncommutative ergodic theory that is evolving even today, because it is peripheral to what I want to say here. I do want to describe the development of a class of nonself-adjoint operator algebras that relates to analytic function theory, prediction theory, and invariant subspaces: subdiagonal operator algebras.

It is rare to run across a reference to Norbert Wiener's book on prediction theory [Wie57] in the mathematical literature. That may be partly because the book is directed toward an engineering audience and partly because it was buried as a classified document during the war years. Like all of Wiener's books, it is remarkable and fascinating, but not an easy read for students. It was inspirational for me and was the source from which I had learned the rudiments of prediction theory that I brought with me to UCLA as a graduate student. Wiener was my first mathematical hero.

Dirichlet algebras are a broad class of function algebras that originated in efforts to understand

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the *disk algebra* $A \subseteq C(\mathbb{T})$ of continuous complex-valued functions on the unit circle whose negative Fourier coefficients vanish. Several paths through harmonic analysis or complex function theory or prediction theory lead naturally to this function algebra. I remind the reader that a *Dirichlet algebra* is a unital subalgebra $A \subseteq C(X)$ (X being a compact Hausdorff space) with the property that $A + A^* = \{f + \bar{g} : f, g \in A\}$ is sup-norm-dense in $C(X)$; equivalently, the real parts of the functions in A are dense in the space of real valued continuous functions. One cannot overestimate the influence of the two papers of Helson and Lowdenslager ([HL58], [HL61]) in abstract function theory and especially Dirichlet algebras. Their main results are beautifully summarized in Chapter 4 of Ken Hoffman's book [Hof62].

Along with a given Dirichlet algebra $A \subseteq C(X)$, one is frequently presented with a distinguished complex homomorphism

$$\phi : A \to \mathbb{C},$$

and because $A + A^*$ is dense in $C(X)$, one finds that there is a unique probability measure μ on X (of course I really mean unique *regular Borel* probability measure) that represents ϕ in the sense that

$$\phi(f) = \int_X f\, d\mu, \qquad f \in A. \tag{1.1}$$

Here we are more concerned with the closely related notion of weak*-Dirichlet algebra $A \subseteq L^\infty(X, \mu)$, in which uniform density of $A + A^*$ in $C(X)$ is weakened to the requirement that $A + A^*$ be dense in $L^\infty(X, \mu)$ relative to the weak*-topology of L^∞. Of course we continue to require that the linear functional (1) should be multiplicative on A.

Going Noncommutative

Von Neumann algebras and C^*-algebras of operators on a Hilbert space H are self-adjoint—closed under the $*$-operation of $\mathcal{B}(H)$. But most operator algebras do not have that symmetry; and for nonself-adjoint algebras, there was little theory and few general principles in the early 1960s beyond the Kadison-Singer paper [KS60] on triangular operator algebras (Ringrose's work on nest algebras was not to appear until several years later).

While trolling the waters for a thesis topic, I was struck by the fact that so much of prediction theory and analytic function theory had been captured by Helson and Lowdenslager, while at the same time I could see diverse examples of operator algebras that seemed to satisfy noncommutative variations of the axioms for weak*-Dirichlet algebras. There had to be a way to put it all together in an appropriate noncommutative context that would retain the essence of prediction theory and contain important examples of operator algebras. I worked on that idea for a year or two and produced a Ph.D. thesis in 1964—which evolved into a more definitive paper [Arv67]. At the time I wanted to call these algebras *triangular*; but Kadison and Singer had already taken the term for their algebras [KS60]. Instead, these later algebras became known as *subdiagonal* operator algebras.

Here are the axioms for a (concretely acting) subdiagonal algebra of operators in $\mathcal{B}(H)$. It is a pair $(\mathcal{A}, \phi)$ consisting of a subalgebra $\mathcal{A}$ of $\mathcal{B}(H)$ that contains the identity operator and is closed in the weak*-topology of $\mathcal{B}(H)$, all of which satisfy

- SD1: $\mathcal{A} + \mathcal{A}^*$ is weak*-dense in the von Neumann algebra $\mathcal{M}$ it generates.
- SD2: ϕ is a conditional expectation, mapping $\mathcal{M}$ onto the von Neumann subalgebra $\mathcal{A} \cap \mathcal{A}^*$.
- SD3: $\phi(AB) = \phi(A)\phi(B)$, for all $A, B \in \mathcal{A}$.

What SD2 means is that ϕ should be an idempotent linear map from $\mathcal{M}$ onto $\mathcal{A} \cap \mathcal{A}^*$ that carries positive operators to positive operators, is continuous with respect to the weak*-topology, and is faithful in the sense that for every positive operator $X \in \mathcal{M}$, $\phi(X) = 0 \implies X = 0$.

We also point out that these axioms differ slightly from the original axioms of [Arv67] but are equivalent when the algebras are weak*-closed.

Examples of subdiagonal algebras:

(1) The pair $(\mathcal{A}, \phi)$, $\mathcal{A}$ being the algebra of all lower triangular $n \times n$ matrices, $\mathcal{A} \cap \mathcal{A}^*$ is the algebra of diagonal matrices, and $\phi : M_n \to \mathcal{A} \cap \mathcal{A}^*$ is the map that replaces a matrix with its diagonal part.

(2) Let G be a countable discrete group which can be totally ordered by a relation $\leq$ satisfying $a \leq b \implies xa \leq xb$ for all $x \in G$. There are many such groups, including finitely generated free groups (commutative or noncommutative). Fix such an order $\leq$ on G and let $x \mapsto \ell_x$ be the natural (left regular) unitary representation of G on its intrinsic Hilbert space $\ell^2(G)$, let $\mathcal{M}$ be the weak*-closed linear span of all operators of the form ℓ_x, $x \in G$, and let $\mathcal{A}$ be the weak*-closed linear span of operators of the form ℓ_x, $x \geq e$, e denoting the identity element of G. Finally, let ϕ be the state of M defined by

$$\phi(X) = \langle X\xi, \xi \rangle, \qquad X \in M, \quad \xi = \chi_e.$$

If we view ϕ as a conditional expectation from $\mathcal{M}$ to the algebra of scalar multiples of the identity operator by way of $X \mapsto \phi(X)\mathbf{1}$, then we obtain a subdiagonal algebra of operators $(\mathcal{A}, \phi)$.

(3) There are natural examples of subdiagonal algebras in II_1 factors $\mathcal{M}$ that are based on ergodic measure-preserving transformations that will be familiar to operator algebraists (see [Arv67]).

In order to formulate the most important connections with function theory and prediction theory, one requires an additional property called *finiteness* in [Arv67]: there should be a distinguished tracial state τ on the von Neumann algebra $\mathcal{M}$ generated by $\mathcal{A}$ that preserves ϕ in the sense that $\tau \circ \phi = \tau$. Perhaps we should indicate the choice of τ by writing $(\mathcal{A}, \phi, \tau)$ rather than $(\mathcal{A}, \phi)$, but we shall economize on notation by not doing so.

Recall that the simplest form of *Jensen's inequality* makes the following assertion about functions $f \neq 0$ in the disk algebra: $\log |f|$ *is integrable around the unit circle, and the geometric mean of* $|f|$ *satisfies*

$$(2.1)\qquad |\frac{1}{2\pi}\int_{\mathbb{T}} f(e^{i\theta})\,d\theta| \le \exp \frac{1}{2\pi}\int_{\mathbb{T}} \log |f(e^{i\theta})|\,d\theta.$$

In order to formulate this property for subdiagonal operator algebras, we require the determinant function of Fuglede and Kadison [FK52]—defined as follows for invertible operators X in $\mathcal{M}$:

$$\Delta(X) = \exp \tau(\log |X|),$$

$|X|$ denoting the positive square root of X^*X. There is a natural way to extend the definition of Δ to arbitrary (noninvertible) operators in $\mathcal{M}$. For example, when $\mathcal{M}$ is the algebra of $n \times n$ complex matrices and τ is the tracial state, $\Delta(X)$ turns out to be the positive nth root of $|\det X|$.

Corresponding to (2.1), we will say that a finite subdiagonal algebra $(\mathcal{A}, \phi)$ with tracial state τ satisfies *Jensen's inequality* if

$$(2.2)\qquad \Delta(\phi(A)) \le \Delta(A), \qquad A \in \mathcal{A},$$

and we say that $(\mathcal{A}, \phi)$ satisfies *Jensen's formula* if

$$(2.3)\qquad \Delta(\phi(A)) = \Delta(A), \qquad A \in \mathcal{A} \cap \mathcal{A}^{-1}.$$

It is not hard to show that (2.2) $\Longrightarrow$ (2.3).

Finally, the connection with prediction theory is made by reformulating a classical theorem of Szegö, one version of which can be stated as follows: For every positive function $w \in L^1(\mathbb{T}, d\theta)$ one has

$$\inf_f \int_{\mathbb{T}} |1 + f|^2 w\,d\theta = \exp \int_{\mathbb{T}} \log w\,d\theta,$$

f ranging over trigonometric polynomials of the form $a_1 e^{i\theta} + \cdots + a_n e^{in\theta}$. In the noncommutative setting, there is a natural way to extend the definition of determinant to weak*-continuous positive linear functionals ρ on $\mathcal{M}$, and the proper replacement for Szegö's theorem turns out to be the following somewhat peculiar statement: For every weak*-continuous state ρ on $\mathcal{M}$,

$$(2.4)\qquad \inf \rho(|D + A|^2) = \Delta(\rho),$$

the infimum taken over $D \in \mathcal{A} \cap \mathcal{A}^*$ and $A \in \mathcal{A}$ with $\phi(A) = 0$ and $\Delta(D) \ge 1$.

In the 1960s there were several important examples for which I could prove properties (2.2), (2.3), and (2.4), but I was unable to establish them in general. The paper [Arv67] contains the results of that effort. Among other things, it was shown that every subdiagonal algebra is contained in a unique *maximal* one and that maximal subdiagonal algebras admit *factorization*: *Every invertible positive operator in* $\mathcal{M}$ *has the form* $X = A^*A$ *for some* $A \in \mathcal{A} \cap \mathcal{A}^{-1}$. Factorization was then used to show the equivalence of these three properties for arbitrary *maximal* subdiagonal algebras.

Resurrection and Resurgence

I don't have to say precisely what *maximality* means because, in an important development twenty years later, Ruy Exel [Exe88] showed that the concept is unnecessary by proving the following theorem: *Every (necessarily weak*-closed) subdiagonal algebra is maximal.* Thus, factorization holds *in general* and the three properties (2.2), (2.3), and (2.4) are *always* equivalent.

Encouraging as Exel's result was, the theory remained unfinished because no proof existed that Jensen's inequality, for example, was true in general. Twenty more years were to pass before the mystery was lifted. In penetrating work of Louis Labuschagne and David Blecher [Lab05], [BL08], [BL07a], [BL07b], it was shown that not only are the three desired properties true in general but virtually all of the classical theory of weak*-Dirichlet function algebras generalizes appropriately to subdiagonal operator algebras.

I hope I have persuaded the reader that there is an evolutionary path from the original ideas of Helson and Lowdenslager, through forty years of sporadic progress, to a finished and elegant theory of noncommutative operator algebras that embodies a remarkable blend of complex function theory, prediction theory, and invariant subspaces.

Henry with Jun-ichi Tanaka, Berkeley campus, May 1984.

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Kathy Merrill

I first met Henry Helson on a bus commuting to a regional AMS meeting in Salt Lake City one spring morning in 1983. Later that morning, Henry told the group attending his talk in our special session that I knew more about cocycles of an irrational rotation than he did. This of course was not true, but I was to learn that it was typical of Henry's kindness and generosity. I was just finishing my Ph.D. at the University of Colorado and was on the job market, hungry for praise but not so deserving of it. Henry, on the other hand, was already well known for using cocycles to mine information about invariant subspaces of functions on compact groups with ordered duals [2]. Many of his results had important applications to ergodic theory and Diophantine approximation as well.

Our conversations in Salt Lake City began a mathematical friendship that added joy as well as practical support to the beginning of my career. Henry helped me get a (non-tenure-track) job offer at Berkeley, which was quite helpful to my confidence even though I had to turn it down. He went on to grace me with a regular stream of correspondence over the next ten years, including conjectures, examples, references, and odd bits of mathematical beauty. One result of this interchange was a joint publication [4], leading to my enviable Helson number of 1. Henry also wrote to me about his philosophy of teaching: "It is simply not true that ordinary students are incapable of abstraction, and must be jollied with a constant stream of pseudo-applications." He sent reassurance when I expressed self-doubt. When I told

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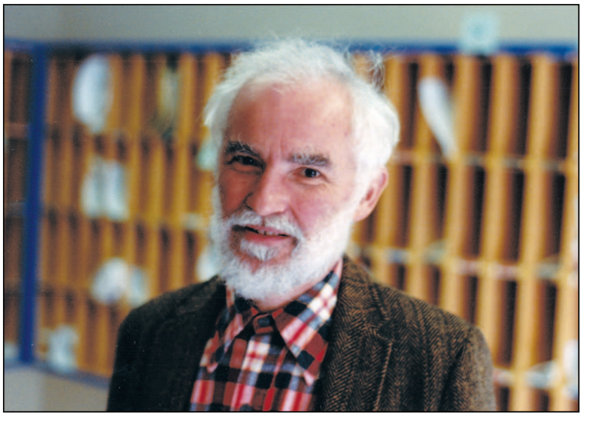
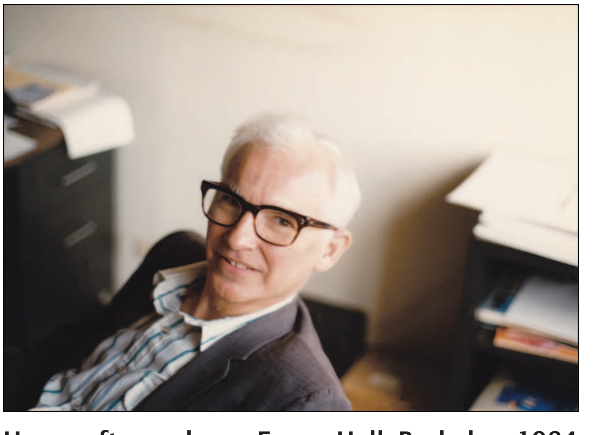


Henry before a shave, Evans Hall, Berkeley, 1984 (from George Bergman's collection, with his permission).



Henry after a shave, Evans Hall, Berkeley, 1984 (from George Bergman's collection, with his permission).

him I was afraid I would never have another new idea, he told me he always feared that each paper he wrote would be his last, but so far it never was. When I apologized for being slow to understand something he had sent me, he assured me that he was even slower. He said that while he could write mathematical papers, he could not read them.

I spent part of my first sabbatical in 1991 visiting Henry at Berkeley. He arranged for me to speak in the functional analysis seminar there and afterward chided me for making cocycles appear too simple. We spoke of difficulties faced by women in mathematics, and particularly of the controversy brewing at Berkeley at the time. Henry's perspective, as fit his character, was thoughtful and kind, avoiding angry rhetoric and supporting a valued colleague. During that visit, he introduced me to his student Herbert Medina, with whom I began a fruitful and pleasurable collaboration. My work with Herbert and fellow coauthor Larry Baggett soon moved from cocycles to wavelets. Henry Helson's work was lurking under the central idea there as well. Our approach to wavelets depended on applying the multiplicity function of a spectral measure to the representation of the integers as translations in the core subspace of a multiresolution analysis. In his book, *The Spectral Theorem* [3], Henry gives a simple and elegant account of the multiplicity function, using range functions instead of direct integrals. Indeed, a referee for our wavelet paper [1] commented that he had to read Henry Helson to understand our approach. Many other wavelet researchers have also found Helson's range functions a useful tool for understanding the shift-invariant subspaces associated with wavelets and multiresolution analyses.

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Jun-ichi Tanaka

Recollections of Henry Helson

In early spring of 1980, Ravenna and Henry stopped at Tokyo on their way to Calcutta. M. Hasumi and I met them at the airport, which was the first time I saw Henry in person. At that time I was studying function algebras and got interested in function theory associated with flows, developed by F. Forelli and P. Muhly. This research direction was pioneered primarily by the series of works by Helson and Lowdenslager.

A few years later, 1984–1985, I had a chance to stay at Berkeley as a postdoc, and Henry was willing to take me in as an advisee. It was a fantastic experience, although my poor English caused a lot of trouble. I joined in the Arveson seminar and occasionally attended colloquia with Henry. I remember vividly D. Sarason's lecture on the Bieberbach conjecture, P. Jones's one on the corona problem, and so on.

I felt rather shy around Henry, but he was always kind and friendly. He would look at what I was reading over my shoulder, then explain to me related topics. I felt very lucky to learn function theory on groups directly from one of its founders. Henry sometimes talked about mathematicians whom he had met in the past. I was surprised a little to learn that he knew S. Kakutani and his family well.

Helson and Lowdenslager developed their theory on a compact abelian group K dual to a dense discrete subgroup Γ of $\mathbf{R}$. (This setting is more general than it may look.) Let σ be the normalized Haar measure on K. The *Hardy space* $H^p(\sigma), 1 \le p \le \infty$, is defined to be the space of all functions in $L^p(\sigma)$ whose Fourier coefficients with negative indices vanish. Since $H^\infty(\sigma)$ is a weak*-Dirichlet algebra in $L^\infty(\sigma)$, we are led to study the structure of invariant subspaces, which can be investigated in detail by virtue of the group structure. An ergodic flow $x \to x + e_t$ is introduced naturally on K, called an *almost periodic flow*. Then ϕ lies in $H^p(\sigma)$ if and only if for almost every x, $t \to \phi(x + e_t)$ lies in $H^p(dt/\pi(1 + t^2))$, the closure of $H^\infty(\mathbf{R})$ in $L^p(dt/\pi(1 + t^2))$. This observation enables us to extend such Hardy spaces to the case of general ergodic flows. Recall that a closed subspace $\mathfrak{M}$ of $L^p(\sigma)$ is *invariant* if $H^\infty(\sigma) \cdot \mathfrak{M}$ is contained in $\mathfrak{M}$. Denote by $H_0^p(\sigma)$ the subspace of all functions in $H^p(\sigma)$ with mean value zero. This invariant subspace $H_0^p(\sigma)$ plays an important role. Dealing with invariant subspaces, we may restrict our attention to the case of $p = 2$. There is a correspondence between invariant subspaces and certain unitary functions on $K \times \mathbf{R}$, called *cocycles*.

Henry raised repeatedly the problem of whether every invariant subspace can be represented as the smallest one containing a definite element, the so-called *single generator problem* (SGP). And he showed me a lot of equivalent and sufficient conditions. In his survey article [5, 1975], he writes "Is every simply invariant subspace of L^2 generated by one of its elements? Has H_0^2 a single generator? These problems are old, and apparently untouched by all that precedes. Answers will be interesting

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and probably difficult." Henry once told me he raised the problem in 1957, but it seems to appear in [6, p. 183] for the first time, in an equivalent form related to evanescent stochastic processes. The difficulty of SGP seemed to center on the case of $H_0^2(\sigma)$.

This case had been the main subject of my research for a long time. (Yes, this sentence is written in the past perfect tense. I believe that I finally succeeded recently in settling it.) The answer is negative in the context of Helson and Lowdenslager: In the case of almost periodic flows, H_0^2 has no single generator, and there are many kinds of simply invariant subspaces of L^2 with no generator. In view of these facts it may sound strange to say that it can be shown that there is an ergodic flow on which H_0^2 has a single generator. Thus, in the setting of weak*-Dirichlet algebras, it is meaningful to characterize the extreme points of the unit ball of H_0^1, which is due to Gamelin. Henry seemed to be very satisfied with this report.

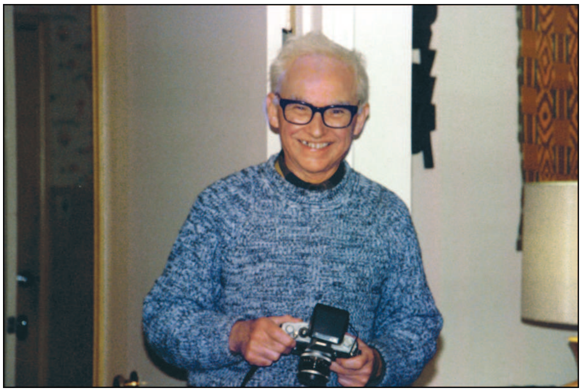
In the process of investigating SGP, I learned a lot of things and had some results on flows. M. Nadkarni once wrote me that he had also gotten interested in SGP soon after he finished his Ph.D. in 1965, and had obtained a number of results that he had been able to publish. We seemed to share the same experience in this regard.

Henry was very encouraging in spite of my rather slow progress and was always pleased when I could obtain new ideas to attack the problem. As an example of by-products of my endeavor, let me explain how one can settle, by using my ideas, both questions of Forelli's stated in his report [2] for the Nice Congress, which sits back-to-back with Henry's [3]. At the end of the 1980s, I was eager to look for an ergodic flow on which H_0^2 is singly generated. The shift on the integers $\mathbf{Z}$ induces a homeomorphism τ on the Stone-Čech compactification $\beta\mathbf{Z}$ of $\mathbf{Z}$. Let X be the quotient space of $\beta\mathbf{Z}\times[0,1]$, by identifying $(\omega,1)$ with $(\tau(\omega),0)$. Then we have a continuous flow on X being an extension of the translation on $\mathbf{R}$. I am still interested in this flow in connection with the corona theorem. Indeed, by a normal families argument, one can show that $H^\infty(\mathbf{R})$ is isometrically isomorphic to $H^\infty(X)$, the algebra of all bounded Borel functions which are analytic on each orbit. Then $X\times(0,\infty)$ represents concretely a certain part of the fiber of the maximal ideal space of $H^\infty(\mathbf{R})$. Notice that the upper half-plane $\mathbf{R}\times(0,\infty)$ is a dense open subset of $X\times(0,\infty)$. There are a lot of representing measures for $H^\infty(X)$ that are invariant, which suggests that there may exist a relation between the corona problem and the individual ergodic theorem, via Wiener's Tauberian theorem. Somehow, I felt as if I understood vaguely why Kakutani had said occasionally, "There should be a simple direct proof of the corona theorem" (I learned Kakutani's remark from O. Hatori). In any event, I had not only a desired flow in a quotient of X but also a negative answer to one of Forelli's questions: There is a minimal flow on which the induced uniform algebra is not a Dirichlet algebra, which holds on each minimal set in X. The other question was also settled in the course of looking into the absolute values of single generators. It runs "What makes a positive measure the total variation measure of an analytic measure?" (This is also a title of one of Forelli's papers [1].) The answer turned out to be: the necessary and sufficient condition for this to be true is that the distant future equals $\{0\}$. Of course, both results heavily relied on the preceding work of Muhly.

What do you imagine from these key words: "analytic almost periodic functions", "the behavior around infinity", and "the distribution of zeros"? One always confronts these phrases when dealing with the structure of invariant subspaces in the almost periodic setting. In the autumn of 1984, the rumor that a Japanese mathematician staying in France at the time had solved the Riemann Hypothesis (RH) spread fast in the Berkeley mathematics community. Henry seemed to get excited about it and wanted to know about this mathematician and the group of number theorists in Japan, although I did not know anything at all about them. As it happened, the rumor turned out to be premature, and the story died down soon thereafter. I felt that just like A. Beurling, Henry might have had RH as one of his problems he should challenge, as Dirichlet series was central to his life's work, and Cassels's book was always his favorite. He once provided another proof of the convergence of $\zeta(1)^{-1}$, which seems roughly to have the depth of the prime number theorem [4]. I learned from him how to extend $\zeta(s)$ and $\zeta(s)^{-1}$ to outer functions in $H^2(\mathbf{T}^\infty)$, and the similarity between Carleson's convergence theorem and the convergence of $\zeta(s)^{-1}$ on the critical strip, which is equivalent to RH.

Although I had no chance to visit Berkeley again after that, we became close friends, keeping in touch until his death. Henry was a good correspondent and a masterly writer. After discussing mathematical ideas, he usually wrote about the recent situation of himself and his family. I remember how he was delighted when his tender for a fine old Italian instrument was accepted (he was a well-known violinist) and how he was happy when his elder son David got married to an English lady in London. He also reported his regrets when it became too hard to continue making wine by himself.

Last autumn, in connection with Rudin's autobiography [7], we corresponded about polydisc algebras. The closed subalgebra of $H^\infty(\mathbf{R})$ obtained by restricting $A(\mathbf{T}^2)$ to one orbit is the simplest one for which the corona theorem fails. A



Henry with camera, Berkeley, early 1990s.

cocycle argument provides easily the fact that an entire inner function in $A(\mathbf{T}^n)$ is a monomial (the theorem of Bojanic and Stoll), as it gives rise to a cocycle of the form $\exp(i\lambda t)$, a weight at infinity.

At the end of last year, I sent season's greetings as usual, together with my new idea to attack the corona problem on polydiscs, depending on tensor products. However, I had no answer from him, and his poor health lay heavily on my mind.

Then, early in the new year, I received the sad news from Hasumi and Nadkarni one after another.

I feel so lucky to have met such a first-rate mind at an early stage of my career and to have received his affectionate interest in my research. He will be greatly missed by everyone who has known him.

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John E. McCarthy

Henry Helson was a transforming influence on me. In my last year of graduate school, his work on the Smirnov class [5] had led him to ask the following question: What functions in the Hardy space H^2 are in the range of every coanalytic Toeplitz operator?

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In other words, what functions f in H^2 have the property that for every nonzero function m in H^∞, there is a function g in H^2 such that

$$T_{\bar{m}}g \;:=\; P_{L^2\to H^2}\, \bar{m}g \;=\; f. \tag{1}$$

D. Sarason observed that such functions must have a certain regularity. Indeed, let $m(z) = 1 - z$ and $g(z) = \sum_{n=0}^{\infty} a_n z^n$ in (1). Then

$$f(z) \;=\; \sum_{n=0}^{\infty} (a_n - a_{n+1}) z^n.$$

So, in particular, f must have a convergent Fourier series at 1.

Replacing m by other functions, one can get more and more regularity conditions that f needs to satisfy. Conversely, one can show that if the coefficients of f decay rapidly enough, then it will be in the range of every coanalytic Toeplitz operator. The trick was to find the right rate of decay. Several people worked on this problem around the time 1988–89.

Why was this problem transformative? Because of the way Henry discussed it with me. Even though I was just a graduate student, he treated me as an equal, with courtesy and respect. He was genuinely interested in what I had to say, or if I made ε progress. This gave me the confidence I needed to think I might be able to solve it, and in the summer of 1989, just after I graduated from Berkeley, I succeeded [7]. My work relied heavily on earlier results of N. Yanagahira [8], but nonetheless I was proud of it. I saw that paper, far more than my dissertation, as my transition from student to mathematician.

After leaving Berkeley, I kept up a frequent correspondence with Henry. He gave me a lot of encouragement and advice both mathematical and personal. We wrote one small paper together [6], but mainly wrote letters about what we were thinking about. (My missives were mainly electronic; his frequently handwritten.) He had strong opinions about most things, including grammar and style. To this day, I cannot use the word "factorization", since, as he pointed out, there is no verb "to factorize"; the correct noun is "factoring".

One piece of advice he gave me was "Every mathematician should look at the ζ-function. It is a mirror." I looked, and having spent much of my early career studying the Hardy space and related objects, I decided to study spaces of Dirichlet series $\sum a_n n^{-s}$ with square-summable coefficients. This was not a new idea—a beautiful paper on this space had been written by Hedenmalm, Lindqvist, and Seip [1]—but it gave me a mental hook to pull myself into this fascinating area. Henry himself had pioneered the introduction of functional analysis into the study of Dirichlet series [2, 3, 4], and he returned to the subject in the past few years. The last letter I received from him was about the generalization of Nehari's theorem on boundedness of Hankel operators to infinitely many variables. (As Harald Bohr had observed, Dirichlet series provide a convenient setting for studying functions of infinitely many variables—essentially, for each prime p, the function p^{-s} plays the role of a variable.)

When I think of Helson's work, the adjective that comes to mind is "elegant". His proofs were always clever, insightful, and graceful. As a person, he was honest and clear-seeing. Like all those who were privileged to know him, I shall miss him.

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Herbert Medina

Reminiscences of a Former Student

Henry Helson was a mathematician who wanted his students to learn to appreciate, to work on, and to love "true analysis". He was eager for us to tackle concrete problems that usually required delicate estimates. He did not believe in analysis that used "sledgehammer" theorems not requiring the estimates and inequalities that are so important to an analyst. I remember him telling me that he thought the real interesting problems were not ones with general hypotheses (e.g., "let G be a topological group"), but ones in specific domains, function spaces, or with specific operators. He shared these views with his students, and the way in which I view mathematics has very much been influenced by Helson's views.

As far as my specific dissertation work, he was the mathematician who introduced me to cocycles and operators that arose from irrational rotations on the circle and showed me how to use the spectral theorem to study and classify these. Work on problems related to these topics was the focus of the first few years of my mathematical career.

There are many memories that I have of Professor Helson, but I'll only relate one anecdote. I remember one time going into his office feeling not terribly excited about something that I was reading and communicated this to him. He simply said, "We are interested in what we work on." In other words, I simply should get to work on the mathematics in the paper and that would get me excited about the topic. He was right! I have used that phrase with my own students several times.

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Farhad Zabihi

I was one of Henry Helson's last students. I began the research on my dissertation in the summer of 1991 amid personal and financial difficulties that had troubled me for several years. Helson was aware of my circumstances but nevertheless wanted to start working together once he returned from Toulouse in late spring. What followed were two years full of excitement and mathematical discovery.

Helson proposed that I work on a problem that interpolates between the Riemann mapping theorem and a theorem of Szegö. The Riemann mapping theorem produces an analytic function on the unit disk whose boundary values lie on the boundary curve of a simply connected region. Szegö's theorem, when interpreted geometrically, says that an analytic function in H^2 can take its values on different circles centered at the origin. Helson thought these theorems could be married together, and perhaps one could find analytic functions that take their values on prescribed curves of a general type.

In our initial approach, Helson suggested that I find analytic functions whose values lie on curves given by formulas, and he thought we should try fixed point methods. Indeed, using Banach's fixed point theorem, I was able to solve the problem for families of lines, hyperbolas, and ellipses under strict conditions. Helson was particularly interested in the ellipse case because it generalized Szegö's theorem (in a narrow sense) and indicated that a solution may exist for a more general class of curves.

With much greater effort, I was able to find solutions for a class of curves that bound convex regions. The proof involved a complicated construction of analytic functions, the limit of which

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Henry and Ravenna, Berkeley, circa 2007.

yielded a solution. I was convinced the construction worked, but Helson was not satisfied. He thought that the proof was unwieldy and computational. “The hardest problems,” he told me, “are solved with the simplest ideas.” So I continued to work on my proof and found a new approach using ideas from the proof of the Riemann mapping theorem. It cracked the problem wide open and led to the main theorem in my dissertation.

Helson’s insistence on brevity and simplicity also carried over to writing the dissertation. His writing style is clean, clear, and engaging, and he expected no less from his students. It had taken me a year to complete my research, and it took almost as long to simplify the proofs and distill them to their final form. We published these results, along with some extensions that I discovered later, in a joint paper [1]. In the course of writing my dissertation and the paper, Helson taught me as much about the art of presentation as he had about rigorous research.

By the time I graduated in 1993, the difficulties I faced early on were mostly behind me. I am sure Helson was in part responsible, for he had made a profound impression on me. I kept in touch with him through the years after my graduation. We often had lunch at his favorite Indian restaurant in Berkeley, and I later introduced him to Persian cuisine, to his delight. On reflection, he once said that he could have been more supportive of his students. I disagreed, as my own debt of gratitude to Henry Helson is immeasurable.

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Ph.D. Students of Henry Helson (all are from UC Berkeley)

Frank Forelli Jr.	1961
Michael Cambern	1963
Ernest Fickas	1964
Malcolm Sherman	1964
Stephen Gerig	1966
Iri Yale	1966
Shirley Jackson Jr.	1967
Malayattil Rabindranathan	1968
Udai Tewari	1969
Samuel Ebenstein	1970
Chester Jacewitz	1970
Bonnie Saunders	1978
Geon Choe	1987
Carlos Carbonera	1990
Trent Eggleston	1990
Zachary Franko	1990
Herbert Medina	1992
Adrian Zidaritz	1992
Farhad Zabihi	1993
Nicholas James	1996