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Photo courtesy of California Institute of Technology/Bob Paz.

AMS
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FEATURES

Our August issue features the recent counterexample to the conjectured topological version of Tverberg's 1966 theorem and 2016 Steele Prize winner Barry Simon on the occasion of his 70th birthday conference this month.

—Frank Morgan, Editor-in-Chief

ALSO IN THIS ISSUE

THE GRADUATE STUDENT SECTION

FROM THE SECRETARY'S OFFICE

The background figures on the cover show reflections in the Poincaré Disk (left) and a spiraling line compactification that adds a circle at infinity (right) © Mihai Stoiciu, from Barry Simon's 5-volume set *A Comprehensive Course in Analysis.*

bookstore.ams.org/simon-set

From Mathematical Physics to Analysis: A Walk in Barry Simon's Mathematical Garden

Editor's Note: *Fritz Gesztesy kindly accepted our invitation to put together this feature in honor of Barry Simon on the occasion of Simon's 2016 AMS Leroy P. Steele Prize for Lifetime Achievement and his 70th birthday conference this August 28–September 1.*

Fritz Gesztesy

This is a collection of contributions by collaborators, postdocs, and students of Barry Simon of the California Institute of Technology on the occasion of Simon's receiving the 2016 AMS Leroy P. Steele Prize for Lifetime Achievement. The citation for the award mentions his tremendous impact on the education and research of a whole generation of mathematical scientists, and we will underscore this by demonstrating his penetrating influence on topics ranging from quantum field theory, statistical mechanics, the general theory of Schrödinger operators, spectral and inverse spectral theory to orthogonal polynomials.

But we should start at the beginning: Barry was born to parents Minnie and Hy Simon in 1946, and together with his older brother, Rick, grew up in Brooklyn, New York. There he attended James Madison High School, obtaining a perfect score on the MAA's American High School Mathematics Examinaton in 1962 and thus becoming the subject of an article in the *New York Times* at the tender age of sixteen. Under the influence of Sam Marantz, an inspiring physics teacher in high school, he applied to Harvard and was admitted. While at Harvard he was a top five Putnam Competition Winner in 1965 and received his BA summa cum laude in physics in 1966. George Mackey at Harvard recommended Barry pursue a doctorate with Arthur Wightman at Princeton because Wightman was well known for advocating the application of rigorous mathematics in physics.

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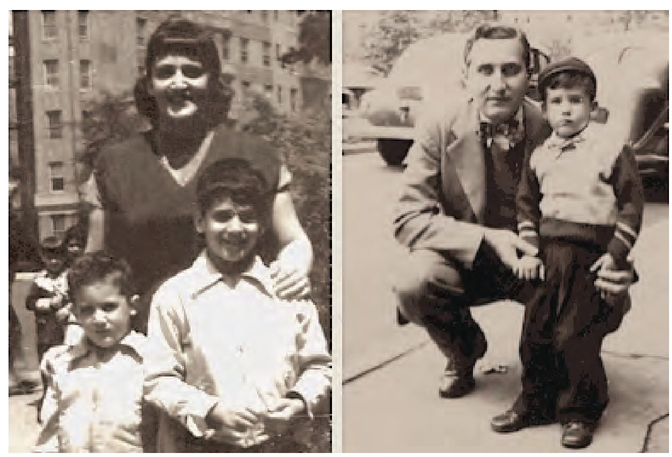

Barry Simon with his mother and brother, Rick (left), and with his father (right) (ca. 1950).

Barry Simon completed his doctorate in physics at Princeton under Wightman's supervision in 1970. The body of his work during the time of his doctoral research was of such importance that he was immediately appointed to assistant professor, jointly in mathematics and physics, at Princeton. He rapidly rose to the rank of full professor by 1976. Several contributions below will attest to the electric atmosphere at Princeton in those days, making it a thriving center for quantum field theory, statistical mechanics, and nonrelativistic quantum mechanics. Barry joined Caltech in 1981, holding the position of IBM Professor of Mathematics and Theoretical Physics since 1984. At Caltech, Barry's interests further broadened into areas such as random and ergodic Schrödinger operators, exotic spectra, inverse spectral theory, and the analytic theory of orthogonal polynomials.

Honorary doctorate, Ludwig-Maximilians-University of Munich, 2014.

… rigorous mathematics in physics

Since many of Barry's major research accomplishments are discussed in depth and put into proper context in the various contributions to follow, we itemize only a few brief comments at this point, focusing on some key results he proved, fundamental concepts he advocated, and some of the important terms he and his collaborators first coined with lasting impact:

- a rigorous framework for resonances, complex and exterior scaling, Fermi's golden rule, proof of the Oppenheimer formula for the Stark effect, convergence of time-dependent perturbation theory;
- constructive (Euclidean) quantum field theory in two space-time dimensions, connections to statistical mechanics, lattice approximations and correlation inequalities, $P(\phi)_2$ spatially cutoff field theories;
- hypercontractive and ultracontractive semigroups;
- magnetic fields, diamagnetic inequality, Kato's inequality;
- a proof of continuous symmetry breaking in classical and quantum statistical models;
- Thomas–Fermi theory, semiclassical bounds, non-Weyl asymptotics;
- asymptotic perturbation theory of eigenvalues: Borel and Padé summability, Zeeman effect, anharmonic oscillators, instanton tunneling, Birman–Schwinger principle, coupling constant thresholds;
- general theory of Schrödinger operators: essential self-adjointness, pointwise bounds on eigenfunctions, path integral techniques, absence of singular continuous spectrum in N-body sytems;
- Berry's phase and holonomy, homotopic interpretation of the Thouless integers and topological structure in the integer quantum Hall effect;
- random and almost periodic Schrödinger and Jacobi operators, exotic spectral phenomena (Cantor, singular continuous, and dense pure point spectra) and their transition to becoming a central object in mathematical physics (the singular continuous revolution), Wonderland theorem, Thouless formula, almost Mathieu equation;
- trace formulas for potential coefficients in terms of the Krein–Lifshitz spectral shift function, uniqueness theorems in inverse spectral theory for Schrödinger and Jacobi operators, oscillation theory in gaps of the essential spectrum, inverse spectral analysis with partial information on the potential;
- a new approach (the analog of the continued fraction method) to inverse spectral theory of Schrödinger operators, his local Borg–Marchenko theorem;
- a systematic application of operator theory techniques to orthogonal polynomials on the real line (OPRL) and on the unit circle (OPUC), CMV matrices, Verblunsky coefficients;
- sum rules for Jacobi matrices and applications to spectral theory (Killip–Simon theorem), perturbations of OPRL and OPUC with periodic recursion coefficients;
- Szegő asymptotics, a proof of Nevai's conjecture and its finite gap extension, the finite gap analog of the Szegő-Shohat-Nevai theorem, the fine structure of zeros of orthogonal polynomials (clock behavior), higher-order Szegő theorems.

Barry Simon's influence on our community by far transcends his approximately four hundred papers, particularly in view of 126 coauthors, 50 mentees, 31 graduate students, and about 50 postdocs mentored. In this context, one must especially mention his twenty books, the first fifteen of which have educated scores of mathematicians and mathematical physicists, two generations by now, and continuing into the foreseeable future.

One cannot overestimate the influence of Reed and Simon's four-volume series, *Methods of Modern Mathematical Physics, I–IV* (1972–79). It took on the same level of importance that Courant–Hilbert's two volumes had for previous generations, and it continues to fill that role all over the globe to this day. To gauge the importance of Reed and Simon behind the Iron Curtain, we contacted Albrecht Böttcher (TU Chemnitz, Germany). Like so many in our generation, Albrecht has a very personal relationship with Volume I and underscored (his words) "the truly ingenious selection and presentation of the mathematical topics." The latter sentiment, however, is by no means unique to colleagues who read Reed-Simon in Russian translation; it is just as prevalent in the West.

One cannot overestimate the influence of Reed and Simon's Methods of Modern Mathematical Physics, I–IV

Cumulative sales figures for all four volumes to date top 37,000 copies. Barry's other books, most notably *Functional Integration and Quantum Physics* (1979), *Trace Ideals and Their Applications* (1979, 2005), *Orthogonal Polynomials on the Unit Circle, Parts 1, 2* (2005), and *Szegő's Theorem and Its Descendants* (2011), profoundly influenced research in these areas. Finally, we have not even begun to understand the legacy which will be created with his newest five-volume set, *A Comprehensive Course in Analysis* (2015), which offers a panorama from real to complex and harmonic analysis all the way to operator theory.

In short, Barry has been a phenomenal force in mathematical physics, encyclopedic in his knowledge and a grand master of mathematical structure and abstract analysis. Above all, he truly enjoys collaboration and the human interactions that come with it. As a sign of his tremendous influence on our community we note that to date MathSciNet lists 15,325 citations by 6,602 authors, and Google Scholar lists 61,680 citations and an h-index of 104.

Barry has been recognized with honorary degrees from the Technion, Haifa; the University of Wales–Swansea, and LMU–Munich. He was awarded the Stampacchia Prize in 1982 with M. Aizenman, the Poincaré Prize of the International Association of Mathematical Physics in 2012, and the Bolyai Prize of the Hungarian Academy of Sciences in 2015. He is a fellow of the American Physical Society (1981), the American Academy of Arts and Sciences (2005), and the AMS (2013). He has served as vice president of the AMS and of IAMP.

The books Barry Simon has authored …thus far.

Finally, on a personal note: Having been a frequent collaborator of Barry's, I have often been approached with the assumption of being one of Barry's students or postdocs, but this is not the case. On the other hand, like so many of my generation, I learned about the tools of our trade through his writings and especially from Reed and Simon I–IV, so of course it feels like I was Barry's student, even though the proper term appears to be that I'm one of his many mentees. Barry has been a constant inspiration to me for about forty years now; I feel incredibly fortunate that he became my mentor and friend.

Evans M. Harrell

Singular Perturbation Theory and Resonances

The very first article in Barry Simon's publication list, which appeared in *Il Nuovo Cimento* when he was a 22-year-old graduate student, was concerned with singular perturbation theory. This paper showed that a certain regularized, renormalized perturbation expansion for a two-dimensional quantum field theory model converges with a positive radius of convergence. As Barry candidly admitted in that article, in itself the result was of limited significance, but in a subject for which at that time "all the mathematically suitable results…are of a negative nature," it announced a new, more constructive era.

To the reader familiar with Barry Simon's works on mathematical physics of the 1970s, it is striking how many of the hallmarks of his technique are already apparent in this first article. Before entering deeply into the research, Barry first carried out a thorough and penetrating review of the entire literature on the subject. This signature of his method was something those of us who were students at Princeton in the 1970s would witness every time Barry began a new research project: Seeing him emerge from the library shared by Jadwin and Fine Halls with a mountain of books and articles, it was humbling to realize that Barry was not merely brave enough to collect all of the knowledge about the next subject he wished to study, but seemingly overnight he would absorb it in detail and carefully assess each contribution for its mathematical appropriateness. True to form, in that first article, Barry laid out which claims in the literature were established with mathematical rigor, which were plausibly to be believed, perhaps with some extra attention to assumptions, and which were frankly dubious. Finally, Barry's own way of formulating the problem was sparse and clear, and his reasoning incisive.

The perturbation theory that applies to nonrelativistic quantum mechanics is a linear theory, allowing straightforward calculations of systematically corrected eigenvalues

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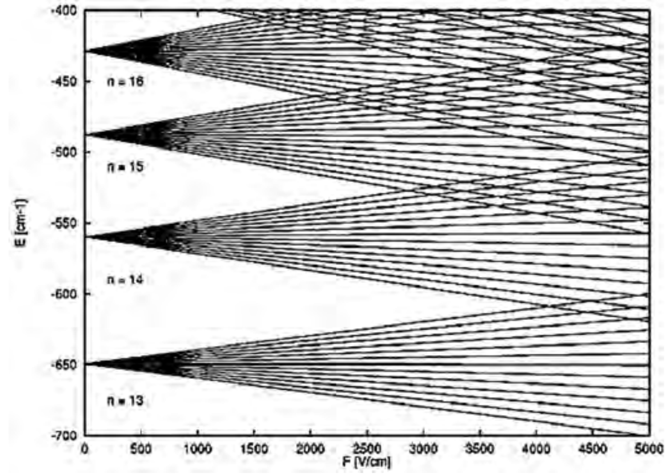
Lorenzo Sadun, Yosi Avron, Evans Harrell, Barry Simon (ca. 1988).

and eigenfunctions. Schrödinger's adaptation of the procedures of Rayleigh to calculate the shifts in hydrogen emission spectrum in the presence of an electric field (known as the Stark effect) went a long way in establishing the validity of his new quantum theory. This success is ironic, given that the series for which Schrödinger and Epstein calculated the first terms has radius of convergence zero, and the spectrum of the Stark Hamiltonian for any nonzero value of the electric field is purely continuous, containing no discrete eigenvalues at all. The instant the interaction is switched on, the nature of the spectrum changes radically, and the perturbed eigenvalue becomes a resonance state! The Stark effect is known today to belong to the realm of singular perturbation theory.

Some physicists were "using methods of unknown validity"

In the two decades after Schrödinger's work, mathematicians created a comprehensive theory of linear operators on Hilbert space. In the 1930s and 1940s Rellich and Kato produced a mathematically rigorous theory of regular perturbations of linear operators and some aspects of singular perturbations. In their hands, perturbation theory was concerned with analytic operator-valued functions of a complex variable, defined initially as convergent power series with operator coefficients, which are most typically self-adjoint in applications to quantum mechanics. These functions exhibit the range of behavior of ordinary scalar analytic functions of a complex variable, with manageable complications when the spectrum consists of discrete eigenvalues of finite multiplicity, and some new phenomena when there is an essential spectrum. Physicists and chemists at this time solved problems and developed perturbative techniques that sometimes fell into the domain of regular perturbation, but just as frequently produced series that could be calculated term by term while exhibiting singular features, as in a



The Stark effect: The spectrum of the hydrogen atom under an increasingly intense electric field, from the Courtney-Spellmeyer-Jiao-Kleppner article appearing in *Physical Review A*, vol. 51 (1995).

zero radius of convergence or, worse, convergence but to the wrong answer. (For instance, a resonance eigenvalue associated with tunneling may have a nonzero imaginary part that, typically, is represented in perturbation theory by a series of the form $\sum_n c_n \beta^n$ with $c_n = 0$ for all n.)

At the time Barry Simon hit the scene, an industry was thriving in attempts to get information from such expansions, whether by replacing the series by other expressions, especially Padé approximates $P[m,n]$, which are ratios of polynomials of m and n degrees, or by the use of analytic-function techniques like Borel summability to make sense of divergent series. Many of Barry's early works addressed these topics.

One of the important models in quantum mechanics with a singular perturbation is the quartic anharmonic oscillator. Its Hamiltonian is

$$p^2 + x^2 + \beta x^4, \tag{1}$$

and it was the subject of a landmark study by Bender and Wu in 1968-69 in which, "using methods of unknown validity"—in Barry's memorable phrase—they painted a fascinating and largely correct picture of the analytic structure of the eigenvalues of the anharmonic oscillator as functions of the coupling constant β, considered as a complex variable. Among other things, Bender and Wu conjectured that the power series expansion for the ground-state eigenvalue of (1) had radius of convergence 0. Then-new computational capabilities in symbolic algebra had allowed Bender and Wu and others to calculate perturbation series at high orders, and with the first seventy-five coefficients a_n for the ground-state eigenvalue in hand, Bender and Wu specifically conjectured that

$$a_n \sim \pi^{-\frac{3}{2}} \sqrt{6}\, 3^n \Gamma\left(n + \frac{1}{2}\right). \tag{2}$$

Barry's response to the explorations of Bender and Wu was to pen the definitive rigorous analysis of the analytic

Some of Barry's coauthors, SimonFest 2006, on the occasion of Barry Simon's sixtieth birthday.

properties of the anharmonic oscillator in an extended article in the *Annals of Physics* in 1970. This work is a timeless classic, a textbook model for how to do singular perturbation theory, and it remains one of Barry's most highly cited works. Most of the claims of Bender and Wu were put on a firm footing, and many further facts were established. For example, it was shown that $\beta = 0$ is a third-order branch point and an accumulation point of singularities. Moreover, Barry obtained sufficient control on the growth rate of perturbation coefficients to show that both the Padé and Borel methods were valid to determine the eigenvalues for nonzero values of β. A few years later, with the aid of a dispersion relation derived using this understanding of the Riemann surfaces of the eigenvalues, Barry and coauthors proved the formula (2).

Later, several other models of singular perturbation theory, including the Stark and Zeeman effects, received similar treatments in the hands of Barry and his associates, especially Yosi Avron and Ira Herbst. These gems provided a foundation for developments in singular perturbation theory and deepened the understanding of many of the touchstones of quantum mechanics.

Barry has always been quick to recognize others' good ideas when they appear and generous in promoting them in the community of mathematical physicists and beyond. A very pretty method introduced in that era by Aguilar, Balslev, and Combes in 1971 came to be called complex scaling. The original version made use of the dilatation symmetry and complex analysis to move the essential spectrum of a Schrödinger operator into the complex plane, while leaving discrete eigenvalues unaffected. The unitary group of dilatations can be defined via

$$[U(\theta)f](\mathbf{x}) := e^{\nu\theta/2} f\left(e^{\theta}\mathbf{x}\right) \tag{3}$$

in terms of a real parameter θ. If the scaled potential energy depends in an analytic way on θ, then the parameter can be complexified, and it is easy to see that with a compactness condition on the potential energy the essential spectrum of the complex-scaled Laplacian for nonreal θ is simply rotated in the complex plane. Meanwhile, isolated eigenvalues are analytic as functions of θ, but since they are constant for real variations in θ, by unique continuation they are also constant for any variation of θ, except that they can appear or disappear when they collide with the essential spectrum, at which point analytic perturbation theory ceases to apply.

With this procedure the ad hoc tradition in physics of treating resonances as nonreal eigenvalues somehow associated with a self-adjoint Hamiltonian became mathematically solid and canonical. I cannot do better in describing complex scaling further than to point to Chapter XIII.10 of Reed-Simon, to which the reader is referred for further details and context. Barry assiduously promoted this excellent tool for understanding resonances, evangelizing the technique to physicists and chemists, by whom it was adopted and used in realistic problems.

I recall in particular when Barry took a delegation of mathematical physicists to the 1978 Sanibel Workshop on Complex Scaling, organized by the noted quantum chemist Per-Olof Löwdin, at which the discussions between the chemists and the believers in mathematical methods "of known validity" were quite fruitful and informative on all sides. Of course, Barry not only recognized, clarified, and promoted the idea of complex scaling but made his own fundamental advances in the subject, especially by greatly extending the set of problems to which it applied by his discovery that it suffices to perform complex scaling on an exterior region.

A similar tale could be told of Barry's recognition of the importance of the microlocal analysis of tunneling phenomena by Helffer and Sjöstrand in the 1980s, which Barry again promoted, clarified, and in certain ways transformed. But space here is limited, and besides, for the reader interested in learning more about singular perturbation theory and resonances, there is an excellent 1991 review article entitled "Fifty years of eigenvalue perturbation theory," written by a master of the genre, Barry Simon himself.

Percy A. Deift

Princeton in the 1970s; Exponential Decay of Eigenfunctions and Scattering Theory

The 1970s were a very special time for mathematical physics at Princeton. One can read a lively account of those days, written by Barry himself, in the July 2012 edition of the *Bulletin of the International Association of Mathematical Physics*. The main thrust of the activity

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Some of Barry Simon's students, SimonFest 2006, on the occasion of Barry Simon's sixtieth birthday.

was in statistical mechanics, quantum field theory, and nonrelativistic quantum mechanics. The list of people who participated in math-phys at Princeton University in those years as students, postdocs, junior faculty or senior faculty, or just visitors for a day or two reads like a who's who of mathematical physics. Leading the charge were Arthur Wightman, Elliott Lieb, and Barry Simon. But there were also Eugene Wigner, Valentine Bargmann, and Ed Nelson. And in applied mathematics, there was Martin Kruskal, still flush with excitement from his seminal work on the Korteweg-de Vries equation, and across the way at the Institute were Tulio Regge and Freeman Dyson, doing wonderful things. Barry was a dynamo, challenging us with open problems, understanding every lecture instantaneously, writing paper after paper, often at the seminars themselves, all the while supervising seven or eight PhD students.

I was one of those students. I had an appointment to meet with Barry once every two weeks. I would work very hard preparing a list of questions that I did not know how to answer. Say there were ten questions; by the end of the first ten minutes in Barry's office, the first six questions were resolved. Regarding questions seven and eight, Barry would think about them for about two or three minutes and then tell me how to do them. Regarding questions nine and ten, Barry would think about them, also for about two or three minutes, and say, "I don't know how to do them. But if you look in such and such a book or paper, you will find the answer." Invariably he was right. So in less than half an hour, all my questions were resolved, and as I walked out of the door there was the next student waiting his turn!

Barry's first PhD student was Tony O'Connor. O'Connor's thesis concerned exponential decay for eigenfunctions ψ of N-body Schrödinger operators H, $H\psi = \lambda\psi$, for λ below the essential spectrum of H. Here $H = H_0 + V$, where H_0 is the kinetic energy and V is the interaction potential. For Schrödinger operators in one dimension, such results go back to the nineteenth century, but for $N > 1$ particles moving in three dimensions, completely different techniques were necessary. Over the years many people have worked on the problem, including, to name a few, Stanislav Merkuriev in the former Soviet Union, and John Morgan and Thomas and Maria Hoffmann-Ostenhof in the West. O'Connor had the idea of using the analyticity of the Fourier transform and obtained results in the L^2 sense (i.e., $e^{\alpha|\cdot|}\psi \in L^2$, $\alpha > 0$). Such bounds are optimal for isotropic decay.

O'Connor's paper motivated Jean-Michel Combes and Larry Thomas to introduce an approach that has now become standard under the general rubric of "boost analyticity," and in a set of three papers in the mid-1970s, Barry further developed these ideas to obtain pointwise exponential bounds on eigenfunctions under various assumptions on the asymptotic behavior of the interaction potential V, proving eventually that if $V(x)$ was bounded below by $|x|^{2m}$, say, then one obtained superexponential decay for $\psi(x)$,

$$|\psi(x)| \le c\, e^{-\alpha|x|^{m+1}} \quad c,\, \alpha > 0.$$

Schrödinger operators typically involve interaction potentials V which are sums of two-body interactions

$$V(x) = \sum_{1\le i<j\le N} V_{ij}(x_i - x_j)$$

for particles x_i in $\mathbb{R}^3$, $i = 1, \dots, N$. Although typically

$$V_{ij}(y) \to 0 \text{ as } |y| \to \infty,$$

$V(x)$ clearly does not decay if $|x| \to \infty$ in such a way that $x_i - x_j$, say, remains bounded for some $i \neq j$. Such nonisotropy in the potential V suggests that isotropic bounds of the form

$$|\psi(x)| \le c\, e^{-\alpha|x|}$$

are not optimal amongst all possible bounds.

In a fourth paper on exponential decay in 1978, together with Deift, Hunziker, and Vock, Barry constructed optimal nonisotropic bounds for eigenfunctions of the form

$$|\psi(x)| \le c\, e^{(\alpha,x)}$$

for suitable $\alpha = (\alpha_1, \dots, \alpha_N)$, $\alpha_i \in \mathbb{R}^3$. The α's reflect the geometry of the channels where

$$V(x) \nrightarrow 0 \text{ as } |x| \to \infty.$$

Eventually, in 1982, Agmon showed that for a very general class of elliptic operators H in $\mathbb{R}^{3N}$, there was a naturally associated Riemannian metric on $\mathbb{R}^{3N}$ such that eigenfunctions ψ,

$$H\psi = \lambda\psi,$$

with λ below the essential spectrum of H, satisfied the bound

$$|\psi(x)| \le c_\epsilon\, e^{-(1-\epsilon)\,\rho(x)} \quad \forall \epsilon > 0,$$

where $\rho(x)$ is the geodesic distance from x to the origin in $\mathbb{R}^{3N}$ with respect to Agmon's metric. The Agmon metric can be used to derive, and so explain, the bounds in the 1978 work of Deift, Hunziker, Simon, and Vock.

The scattering problem in quantum chemistry, going back to the 1920s, can be stated informally as follows: In a chemical reaction, do molecules go to molecules? In other words, suppose in the distant past that the particle system is described by a collection of noninteracting molecules.

SimonFest in 2006 for Barry Simon's sixtieth birthday. Percy Deift is in the front row, second from the right.

As time goes on, the atoms in the different molecules begin to interact with each other, and the molecules break up. At large positive times, is the particle system again described by molecules?

To be mathematically precise, consider a collection of atoms

$$x_i \in \mathbb{R}^3,\ i = 1, \dots, N,$$

with Hamiltonian

$$H = H_0 + V = H_0 + \sum_{1<j\leq N} V_{ij}\left(x_i - x_j\right),$$

in the center of mass frame. Let $C^{(1)}, \dots, C^{(m)}$ be a decomposition of the atoms into m clusters,

$$\sum_{j=1}^{m} \#(C^{(j)}) = N.$$

Now suppose that for large negative times the clusters $\{C^{(j)}\}$ are far apart and in each cluster $C^{(k)}$ the atoms are in an eigenstate ψ_k of the cluster (read "molecular") Hamiltonian, that is,

$$H^{(k)}\psi_k = E_k\psi_k, \text{ where } H^{(k)} = H_0^{(k)} + V^{(k)}.$$

Here $H_0^{(k)}$ is the kinetic energy for the cluster and

$$V^{(k)} = \sum_{\substack{1\leq i\leq j\leq \#(C^{(k)}) \\ x_i, x_j \in C^{(k)}}} V_{ij}\left(x_i - x_j\right)$$

is the interaction potential for the atoms in the cluster. So the scattering problem of quantum chemistry becomes the following: At large positive times, is the particle system again described by molecules, that is, by some decomposition of well-separated clusters $\tilde{C}^{(1)}, \dots, \tilde{C}^{(\tilde{m})}$ with the atoms in each cluster $\tilde{C}^{(k)}$ in an eigenstate of the cluster Hamiltonian $\tilde{H}^{(k)}$ (or more precisely, in some linear combination of such molecular configurations)?

In the physical and mathematical literature, the scattering problem is known as the problem of "asymptotic completeness" or the "unitarity of the S-matrix." Much work has been done on this problem by many people over the years, including the time-independent approach of Faddeev and his school, leading up to the eventual resolution of the problem in 1987 by Israel Michael Sigal and Avy Soffer using time-dependent methods pioneered by Enss.

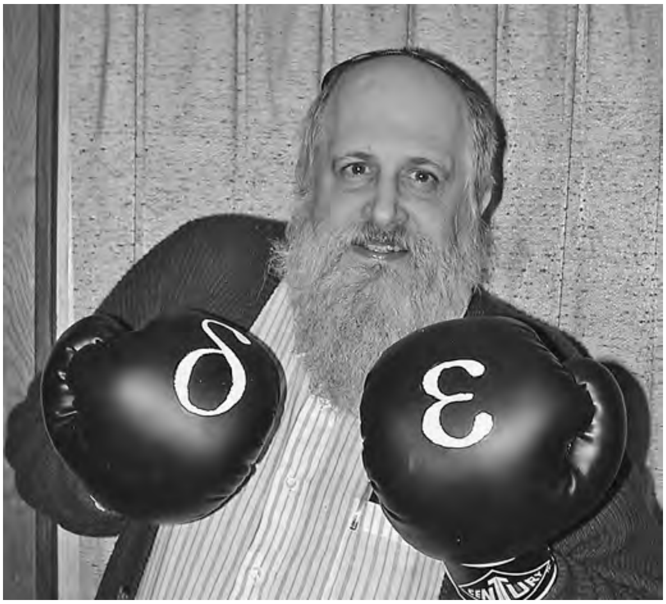
At the mathematical level, the first task in resolving the problem is to prove that such molecular states indeed exist. This boils down to proving that the so-called wave operators $W(H, H_c)$ exist, where H is again the Hamiltonian for the full system and H_c is the Hamiltonian for the molecular system corresponding to the cluster decomposition $C = \left\{C^{(1)}, \dots, C^{(m)}\right\}$. At the technical level this is a relatively easy thing to do. To prove asymptotic completeness, one must show that all states orthogonal to bound states of the full Hamiltonian H are in the linear span of these molecular wave operators $W(H, H_c)$. This is a range question, and range questions in mathematics are generically hard.

Barry is one of the most prolific mathematicians of his generation

In 1978, together with Deift, Simon introduced a new class of wave operators, $W(H_c, J_c, H)$, and showed that asymptotic completeness was equivalent to proving the existence of these wave operators. Here J_c is an auxiliary function reflecting the geometry of the cluster decomposition C. In this way the problem of asymptotic completeness was transformed from a range problem to a potentially simpler existence problem, and this is the path that Sigal and Soffer eventually followed in their resolution of asymptotic completeness. As the ranges of the wave operators $W(H, H_c)$ lie in the absolutely continuous space of H, a key ingredient in proving asymptotic completeness was to show that the singular continuous space for H was trivial. This key component was established by Barry Simon, together with Peter Perry and Israel Michael Sigal, in seminal work in 1980 using remarkable ideas of Eric Mourre [2].

On a Personal Note

Barry is one of the most prolific mathematicians of his generation. It was in the late 1970s, around the time that we were working on nonisotropic bounds for eigenfunctions, that I got a glimpse of the speed with which Barry did things. Soon after Volker Enss introduced his seminal time-dependent ideas on spectral theory and scattering theory, a few of us went to Barry's house in Edison, New Jersey, to discuss a potential project inspired by Enss's work. We spent the afternoon laying out in detail a list of problems that needed to be addressed and left in the late afternoon. The next morning Barry came



From about 1995 to 2000, Barry taught the first term of Caltech's required freshman calculus, where he declared that "epsilon and delta are a calculus student's finest weapons." One year, on the last day, the students presented him with the boxing gloves shown.

into the office: Not only had he solved all the problems on our list, but he had in his hand the first draft of his subsequent paper [3]! We were overwhelmed. For a young person like me, this was most discouraging. And I was doubly discouraged: Barry was younger than I was!

Barry has many fine qualities as a colleague and as a researcher, but I would like to focus on just one of them, viz., Barry's keen sense of fairness and correct attribution of results. People in orthogonal polynomials know well Barry's insistence on calling the recurrence coefficients for orthogonal polynomials on the circle Verblunsky coefficients, in recognition of the almost forgotten seminal work of Samuel Verblunsky. But I would like to tell a different story. In the early 1980s Barry was in Australia, where he met up with Michael Berry, who was also visiting. Berry began telling Barry about some curious and puzzling calculations he had been making in quantum adiabatic theory. Barry immediately understood that what was really going on was a matter of holonomy, and with characteristic speed he wrote and sent off a paper to *Physical Review Letters*, pointedly titled "Holonomy, the Quantum Adiabatic Theorem, and Berry's phase." In this way, a major discovery that could quite easily have become known as "Barry's phase" was fixed in the literature as "Berry's phase," and justly so.

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Lon Rosen

Barry and I were both young pups when we collaborated in the 1970s. It was an intense and exciting experience for me, one which I cherish and now take pleasure in recalling for you. Warning: these personal recollections have no scientific content. If that's what you're looking for, please see my contribution to the *Festschrift in honor of Barry Simon's sixtieth birthday*.

I must confess that my first meeting with Barry was far from auspicious. In 1967 I was in my first year of doctoral studies at the Courant Institute. Feeling isolated, I was reconsidering my decision not to have chosen Princeton for graduate school. I asked a friend to arrange a lunch meeting for me with a typical student of Arthur Wightman's. I knew little about the "typical student" who was chosen (Barry Simon), although I was familiar with his name because Barry and I had both been Putnam Fellows in the 1965 competition.

Some typical student! He practically tore my head off. Whatever I said about my interests or ideas, Barry would trump it. I'd never met anyone else with such extensive knowledge, amazing recall, and proofs at the ready. I still haven't. Thanks to Barry, I stayed put at Courant. Fortunately, James Glimm, who was to be my terrific thesis advisor, soon joined the faculty there. I learned later that Barry had been going through a rough patch in his personal and professional life around the time we met and that the fire-breathing dragon who had me for

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Barry and Martha Simon, LMU Munich, 2014.

lunch was actually a gentle prince in disguise, if I may be permitted a fairy tale metaphor.

Three years later I gave a seminar at Princeton on the subject of higher-order estimates for the $P(\phi)_2$ model. At the conclusion of the seminar Barry showed me a clever bootstrap trick that quickly established my most difficult estimate—or at least a weaker but perfectly acceptable version of it. I was grateful and revised the published paper accordingly.

This experience was not unique to me. As many speakers know, Barry's rapid-strike ability could be unnerving at seminars. He would sit front row centre, working on a paper, only to surface with astute observations, counterexamples, or shorter proofs. This penchant for "tricks" arises, it seems to me, from Barry's imperative to understand everything in the simplest possible way.

Barry's imperative to understand everything in the simplest possible way

Barry and I both attended the Les Houches Summer School of 1970. It was there that I gained an appreciation for Barry's sense of humour. In particular, we had a lot of fun putting on a skit which satirized the lecturing styles and idiosyncrasies of the various celebrated speakers. For example, when Barry began an impersonation by first breaking a half dozen chalk sticks into small pieces, everyone roared, knowing that "Arthur Jaffe" was about to deliver his next lecture.

Sometimes the humour was (possibly) unintentional. Here's a little story which Ed Nelson told me. Barry had returned from a trip to the former Soviet Union, where he had great difficulty in arranging for kosher food. It was apparently necessary for him to haul a suitcase filled with edibles. "Oh well," sighed Barry, "I guess everyone has his cross to bear."

He would often come in with a twinkle in his eye

In 1971 when I joined Barry in Princeton, we began our joint research by working on what I would call "incremental stuff using available techniques," things like coupling constant analyticity of the $P(\phi)_2$ Hamiltonian. One day Arthur Wightman called us together for a presentation by a quiet visitor from Italy, Francesco Guerra, whom I barely knew. Francesco proceeded to the chalkboard and made some extraordinary claims about the vacuum energy density in the $P(\phi)_2$ model. Barry and I gave each other a sideways look as if to say, "he's got to be kidding." He wasn't! Francesco's short proofs were stunning. The irony was not lost on us that they were based on the Euclidean approach of Ed Nelson of Princeton University. In any case, that was the moment that the Euclidean Revolution

Barry Simon in Bangkok (ca. 2003).

began for Barry and me. The three of us (GRS) entered a long and fruitful collaboration exploring and exploiting the parallels between the $P(\phi)_2$ field theory model and classical statistical mechanics. As usual, Barry snatched up the new ideas like a dog with a bone.

During the GRS period, Barry had numerous other projects on the go, such as his research in mathematical quantum mechanics and the Reed-Simon magnum opus. Thank goodness he was devoting only a fraction of his time to GRS, whereas I was on it full time. Otherwise, I would never have been able to keep up with him and contribute my fair share. Barry's joy in doing research was infectious. He would often come in with a twinkle in his eye and say something like, "While I was standing in the supermarket line, look what I discovered!" Barry was always appreciative of my efforts and extremely generous to others. I learned a tremendous amount from him both directly during our collaboration and in subsequent years from his prodigious published output.

Jürg Fröhlich

Barry Simon and Statistical Mechanics

Well, this is about my mentor and friend Barry Simon! I first met Barry at a summer school on constructive quantum field theory and statistical mechanics at Les Houches, France, in 1970. We were only twenty-four years old at the time, and I had just started my life as a PhD student of Klaus Hepp, while Barry, a former PhD student of the late Arthur S. Wightman, was already a "Herr Doktor" and—if my memory is correct—an assistant professor at Princeton University. As I wrote on the occasion of his sixtieth birthday celebrations [1], Barry would usually beat me in almost everything! To start with, he was born two and a half months before me.

At Les Houches, the late Oscar E. Lanford III lectured on general functional analysis, including measure theory

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and the theory of operator algebras, tools rightly thought to be essential to understanding quantum theory and statistical mechanics. Besides Barry and me, Alain Connes was a student at that school. Barry and Alain were soon engaged in a competition to simplify Oscar's proofs; Barry would usually win—not only in mathematics but also in the consumption of food.

Our common mentor and friend, the late Edward Nelson, wrote about Barry [1]: "In the late 1960s, Barry was a graduate student in physics at Princeton and attended some courses I taught. I soon learned that I did not need to prepare with great thoroughness; it was enough to get things approximately right and Barry from where he was sitting would tell us how to get them precisely right. I miss Barry." Well, Barry and I miss Ed!

My next encounter with Barry was in 1972 when he taught a graduate course on quantum field theory in the "3ième cycle" of French-speaking Switzerland. That course was the basis for his $P(\phi)_2$-book, which is still remembered in the community. This is perhaps because it contains those famous "Fröhlich bounds" or, more likely, because it is written in a very pedagogical manner—indeed, one of Barry's outstanding strengths was and still is to being able to write mathematical prose in a very clear, pedagogical style.

In the fall of 1974 I accepted the offer of an assistant professorship at the mathematics department of Princeton University which had been prepared by Arthur Wightman and Barry Simon. Major benefits of having had Barry as a colleague were that I never had to submit a grant proposal to the NSF; Barry's proposal not only covered his own needs but also the ones of the late Valja Bargmann and me, and should one successfully collaborate with him, he would always write the paper (except for appendices on tedious technicalities, such as cluster expansions, which he gracefully assigned to his collaborators).

Barry needed only roughly 5 percent of the time ordinary mortals need to write a paper

Barry needed only roughly 5 percent of the time ordinary mortals need to write a paper. He did his writing while listening to seminar talks. Although he appeared to be absorbed in his activity (carried out by moving his left hand along rather peculiar trajectories), he would nevertheless be able to point out errors to the lecturer or ask relevant questions at the end. Barry was simply brilliant in "multitasking."

Let me briefly describe two of our joint papers. The first one is entitled "Pure states for general $P(\phi)_2$-theories: Construction, regularity and variational equality" and was published in the *Annals of Mathematics* in 1977. In this paper, ideas and concepts from classical statistical mechanics were transferred to Euclidean field theory with the purpose of learning something new about the latter. General concepts were illustrated on simple examples of Euclidean field theory in two dimensions, which, mathematically, may be defined as generalized stochastic processes—more precisely, Markovian random fields—over $\mathbb{R}^2$ constructed as perturbations of Gaussian processes by local multiplicative functionals. From such processes interacting scalar quantum field theories on two-dimensional Minkowski space can be reconstructed, furnishing examples of what has become known as constructive quantum field theory. CQFT was first advocated by Arthur Wightman in the early 1960s with the purpose of showing that quantum theory and the special theory of relativity are compatible with each other, and was subsequently pursued by Edward Nelson, James Glimm, Arthur Jaffe, and their followers.

The modern mathematically rigorous approach to statistical mechanics was developed by, among many other people, Roland L. Dobrushin, Oscar E. Lanford, and, most importantly, David Ruelle. Our paper is unthinkable without their work and without the discoveries of K. Symanzik, E. Nelson, F. Guerra, L. Rosen, and B. Simon in Euclidean field theory, some of whose works are classics. They had shown that in the Euclidean region (time purely imaginary) of complexified Minkowski space, a quantum field theory of Bose fields looks like a model of classical statistical mechanics. In my paper with Barry this fact is exploited in an essential way.

We were to "win the jackpot" with the paper "Infrared bounds, phase transitions and continuous symmetry breaking," which was the result of joint work with our friend Tom Spencer and was published in *Communications in Mathematical Physics* in 1976.[1] Barry, Tom, and I decided to attempt to understand phase transitions accompanied by the spontaneous breaking of continuous symmetries and long-range correlations (i.e., a divergent correlation length), in models of classical lattice spin systems and lattice gases. We exploited ideas from quantum field theory; in particular, we discovered an analogue of the so-called Källen-Lehmann spectral representation of two-point correlation functions in quantum field theory. For this representation to hold true it is necessary that the model under scrutiny satisfy the *Osterwalder-Schrader positivity*, also called *reflection positivity*, a property originating in axiomatic quantum field theory (as described in well-known books by Streater and Wightman and by Jost).

Here is an example: With each site x of the lattice $\mathbb{Z}^d$ we associate a random variable $\vec{S}_x \in \mathbb{R}^N$, a classical "spin," whose a priori distribution is given by a probability measure, $d\mu(\cdot)$ on $\mathbb{R}^N$, invariant under rotations of $\mathbb{R}^N$; for instance,

$$d\mu(\vec{S}) = \text{const.}\ \delta(|\vec{S}|^2 - 1)d^N S. \tag{4}$$

Let Λ be a finite cube in $\mathbb{Z}^d$. The energy of a configuration, $\vec{S}_\Lambda := \{\vec{S}_x\}_{x\in\Lambda}$, of "spins" is given by a functional (called "Hamiltonian"),

$$H(\vec{S}_\Lambda) := -\sum_{x,y\in\Lambda} J(x-y)\vec{S}_x \cdot \vec{S}_y. \tag{5}$$

[1] *At the beginning of my career I was privileged to have several mentors, among whom Tom was undoubtedly the most important one!*

Birmingham, AL, Meeting on Differential Equations, 1983. Back row: Fröhlich, Yajima, Simon, Temam, Enss, Kato, Schechter, Brezis, Carroll, Rabinowitz. Front row: Crandall, Ekeland, Agmon, Morawetz, Smoller, Lieb, Lax.

Here $J(x)$ is a function in $\ell_1(\mathbb{Z}^d)$ assumed to be *reflection-positive* and invariant under permutations of lattice directions. By a theorem of Bochner, these properties imply that it has an integral representation,

$$(6) \qquad J(x_1, \vec{x}) = \int_{-1}^{1} \lambda^{|x_1|-1} e^{i\vec{k}\cdot\vec{x}} d\rho(\lambda, \vec{k}), \quad x_1 \neq 0,$$

with $\vec{x} = (x_2, \ldots, x_d)$, where $d\rho(\lambda, \vec{k})$ is a positive measure on $[-1,1] \times \mathbb{T}^{d-1}$, for example, $J(x) = \delta_{|x|,1}$. In (5) we

impose periodic boundary conditions at the boundary of Λ. The distribution of configurations $\vec{S}_\Lambda$ of "spins" in thermal equilibrium at inverse temperature β is given by the Gibbs measure

$$(7) \qquad dP_\beta(\vec{S}_\Lambda) := Z_{\beta,\Lambda}^{-1} exp[-\beta H(\vec{S}_\Lambda)] \prod_{x\in\Lambda} d\mu(\vec{S}_x),$$

where $Z_{\beta,\Lambda}$ is a normalization factor (called "partition function"). Let $\langle \cdot \rangle_{\beta,\Lambda}$ denote an expectation with respect to dP_β. For "wave vectors" k in the lattice dual to Λ, we define $\omega_{\beta,\Lambda}(k)$ to be the Fourier transform of the correlation function $\langle \vec{S}_0 \cdot \vec{S}_x \rangle_{\beta,\Lambda}$, $0, x \in \Lambda$.

Simon, Spencer, and I proved that

$$(8) \qquad 0 \leq \omega_{\beta,\Lambda}(k) \leq \frac{N}{2\beta(\hat{J}(0) - \hat{J}(k))} \quad \text{for } k \neq 0, \forall \beta,$$

where $\hat{J}(k)$ is the Fourier transform of $J(x)$, $x \in \Lambda$. This so-called "infrared bound" is inspired by the Källen-Lehmann representation of two-point functions in canonical relativistic quantum field theory. The realization that (6) implies the upper bound in (8) is the basic result in our work. It is then an exercise to show that if the "coupling function" $J(\cdot)$ is such that

$$(9) \qquad |\Lambda|^{-1} \sum_{k\neq 0} [\hat{J}(0) - \hat{J}(k)]^{-1} \leq \text{const.},$$

uniformly in Λ, then in the thermodynamic limit $\Lambda \nearrow \mathbb{Z}^d$, phases with broken $O(N)$-symmetry coexist and are permuted among themselves under the action of the symmetry group $O(N)$, provided β is large enough. (By (4), $\langle |\vec{S}_0|^2 \rangle_{\beta,\Lambda} = 1$; this and (8), (9) imply that the weight of the mode at $k = 0$ in $\omega_{\beta,\Lambda}(k)$ is $\propto |\Lambda|$.) For small β, however, the Gibbs state is well known to be *unique*.

It turns out that the bound (8) has many further applications. It is an important ingredient in a beautiful analysis of critical behavior in the Ising model (by Aizenman, Duminil-Copin, and Sidoravicius) and in showing that the large-distance scaling limit of the nearest-neighbor Ising and the classical XY-model is *Gaussian* in dimension $d > 4$ ("triviality of $\lambda\phi_d^4$-theory" in $d \geq 4$ dimensions); see [2], [3].

In 1980 Barry proved a correlation inequality, sometimes referred to as the "Simon-Lieb inequality," useful to establish decay of correlations in models of classical ferromagnetic lattice spin systems, e.g., the one sketched above with $N = 1$ or 2. Two years later, the basic idea expressed in his inequality became a very useful ingredient in the analysis of multiscale problems, such as Anderson localization (as in work by Tom Spencer and me).

To conclude, let me draw the reader's attention to Barry Simon's book *The Statistical Mechanics of Lattice Gases*.

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Mike Reed

On Barry Simon

When people ask, "How long did it take for you and Barry Simon to write those four volumes of *Methods of Modern Mathematical Physics*?" I usually say, "About ten years," since we started in the late 1960s when Barry was a graduate student and I was a lecturer at Princeton, and we finished in the late 1970s. Writing those books took 50 percent of my research time for ten years but only 10 percent of Barry's research time, and that wasn't because I contributed more—far from it. The reason is that no one works faster than Barry. He instantly sees the the significance of new ideas (whether in mathematics or physics), understands the technical structures necessary to bring the ideas to fruition, and immediately starts writing.

Barry's legendary speed sometimes got him into trouble. I remember going to seminars at Princeton with Barry carrying new preprints from more senior mathematicians. As the seminar proceeded, Barry would read the preprint, absorb the idea, understand the correct machinery to prove a stronger result, and begin writing. No one is more generous than Barry at giving credit to others; he always does and did. Nevertheless, when Barry's paper with a stronger result and a better proof would appear before

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Barry Simon and Michael Reed, Durham, NC, 2007.

the original result, the preprint's author would sometimes have hard feelings. These feelings would usually dissipate when he or she actually met Barry and discovered how open and generous he is.

We wrote the books because we saw that the physics of quantum mechanics and quantum field theory raised deep and interesting analysis questions. Of course our ancestors knew this too. But we saw how hard it was for mathematicians to understand the issues and fully engage because it was so difficult to read the physics literature and translate the ideas and computations into mathematical questions. Originally, we were going to write one small volume that would give the functional analysis background followed by short chapters introducing mathematicians to problems in modern physics. But we were driven by Barry's deep knowledge and intuition about physics and our shared enthusiasm to do and say more, and the result is the four volumes that we wrote.

We had a terrific time! This was long ago, so our handwritten manuscripts were typed. Then we would take the typed manuscript, usually 500–700 pages, and read it aloud. This was the only way to go slowly enough to check the English, the mathematics, and the physics. One would read, both would think, and the other would write down corrections. Typically it took three weeks full time to read a manuscript. For a couple of them, I lived with Barry. We'd get up in the morning, get to work, and give up when we were tired in the evening. We were blessed by the tolerance and good cooking of Barry's wife, Martha. It was very rare that we'd be irritated or angry at each other, because we both have strong personalities that are not easily troubled and we were completely focused on the mathematics and the science. We did all the problems in all the volumes, except the starred ones that we sure were correct but couldn't immediately see how to do.

He respects others, whatever their talent, whatever their station in life

Of course, we were pleased and proud that so many colleagues and students found our books useful. We both still teach out of them and field email questions about the problems. Since we were so young when they were written, we got lots of funny remarks at conferences from mathematicians who didn't know us, such as, "You can't be the Simon who wrote those books; you're too young," and, "Hah! I always thought that Reed was Simon's first name."

There are lots of things to celebrate about Barry Simon: his stupendous achievements, his many students, his sense of humor, his generosity to colleagues. I celebrate his deep sense of common humanity with other human beings. He respects others, whatever their talent, whatever their station in life, and this sense of common humanity makes him very special.

This article will continue next month with contributions by S. Jitomirskaya, Y. Avron, D. Damanik, J. Breuer, Y. Last, and A. Martinez-Finkelshtein.

The Leroy P. Steele Lifetime Achievement

2016 Barry Simon
2015 Victor Kac
2014 Phillip A. Griffiths
2013 Yakov G. Sinai
2012 Ivo M. Babuška
2011 John W. Milnor
2010 William Fulton
2009 Luis Caffarelli
2008 George Lusztig
2007 Henry P. McKean
2006 Frederick W. Gehring, Dennis P. Sullivan
2005 Israel M. Gelfand
2004 Cathleen Synge Morawetz
2003 Ronald Graham, Victor Guillemin
2002 Michael Artin, Elias Stein
2001 Harry Kesten
2000 Isadore M. Singer
1999 Richard V. Kadison
1998 Nathan Jacobson
1997 Ralph S. Phillips
1996 Goro Shimura
1995 John T. Tate
1994 Louis Nirenberg
1993 Eugene B. Dynkin

Barry Simon's Students at Princeton

Anthony O'Connor, 1972
Jay Rosen, 1974
Robert Israel, 1975
Percy Deift, 1976
Evans Harrell II, 1976
George Hagedorn, 1978
Mark Ashbaugh, 1980
Antti Kupiainen, 1980
Steven Levin, 1980
Peter Perry, 1981
Keith Miller, 1982

Barry Simon's Students at the California Institute of Technology

Byron Siu, 1984
Nestor Caticha Alfonso, 1985
Barton Huxtable, 1987
Kristiana Odencrantz, 1987
Clemens Glaffig, 1988
Askell Hardarson, 1988
John Lindner, 1989
Vojkan Jaksic, 1992
Yunfeng Zhu, 1996
Alexander Kiselev, 1997
Andrei Khodakovsky, 1999
Rowan Killip, 2000
Andrej Zlatos, 2003
Irina Nenciu, 2005
Mihai Stoiciu, 2005
Manwah Wong, 2009
Rostyslav Kozhan, 2010
Anna Maltsev, 2010
Milivoje Lukic, 2011
Brian Zachary Simanek, 2012

Credits

p. 740 Childhood photo of Simon with family, courtesy of Barry Simon.

p. 741 Photo of Simon receiving his honorary doctorate, photographer Heinrich Steinlein. Used with permission.

p. 742 Photo of Simon's books, courtesy of Barry Simon.

p. 743 Photo of Sadun, Avron, Harrell, and Simon, courtesy of Evans Harrell.

p. 743. The Stark Effect, used with permission of The American Physical Society.

p. 744 Photo of Simon's coauthors, courtesy of The California Institute of Technology.

p. 745 Photo of Simon's students, courtesy of The California Institute of Technology.

p. 746 Photo of 2006 SimonFest, courtesy of The California Institute of Technology.

p. 747 Photo of Simon in boxing gloves, courtesy of Barry Simon.

p. 747 Photo of Barry and Martha Simon, photographer Heinrich Steinlein. Used with permission.

p. 748 Photo of Simon in Bangkok, courtesy of Barry Simon.

p. 750 Photo of Birmingham Meeting on Differential Equations, courtesy of Barry Simon.

p. 751 Photo of Simon and Reed, courtesy of Barry Simon.

From Mathematical Physics to Analysis: A Walk in Barry Simon's Mathematical Garden, II

Fritz Gesztesy

Editor's Note: *This is a continuation of the August feature in honor of Barry Simon on the occasion of his 2016 AMS Leroy P. Steele Prize for Lifetime Achievement and his seventieth birthday conference August 28–September 1, 2016. The authors of Part I were P. A. Deift, J. Fröhlich, E. M. Harrell, M. Reed, L. Rosen, and F. Gesztesy, who coordinated Parts I and II.*

Joseph (Yosi) Avron

Barry and Pythagoras

Admiration

I passionately admired Barry in the years that shaped me: he seemed to know everything that was worth knowing, be it math, physics, history, or literature; he could think faster than anyone else I knew; he could write mathematics so it read like beautiful poetry, and he did it effortlessly; he was a wonderful teacher who could give a perfectly organized proof of any theorem on the spur of the moment and he could multitask like a superhuman being. Barry was bigger than life. He was my idol and has since been an important part of my life.

Barry [is] bigger than life

Of Walks and Traces

Barry used to visit Israel regularly. He always set up base at the Hebrew University in Jerusalem and came for a day or two to give a seminar at the Technion. Barry made his itinerary early, which meant that I had plenty of time to get ready for his visit, which really meant that I had plenty of time to worry what worthwhile observation I had to impress Barry with. Barry's visits were like my annual driving tests: if Barry simply shrugged and lost interest, this meant that I flunked. Here is the story of a visit that eventually led to a joint paper.

Barry and Yosi, QMath7, Prague, 1998.

Sometime in 1990, following Barry's seminar at the Technion, we were strolling through campus. This time I came prepared. Ruedi Seiler and I were trying to understand Jean Bellissard's noncommutative geometry of the quantum Hall effect, where comparison of infinite-dimensional projections plays a role. I told Barry what I thought was an amusing identity about a pair of finite dimensional projections:

$$Tr\,(P-Q) = Tr\,(P-Q)^3. \qquad (1)$$

You can verify equation (1) using $P^2 = P$ and $Q^2 = Q$ and the cyclicity of the trace. But this does not really explain why the relation is true.

Memories are fragmented and treacherous. I cannot tell today if I found the trace identity on my own or if I learned it and conveniently forgot who taught it to me. Bellissard taught me how many wonderful facts about

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DOI: http://dx.doi.org/10.1090/noti1412

traces and identities similar to the trace identity play a role in his theory of the quantum Hall effect [2], so it is possible that he taught me this identity and I simply forgot.

Anticommutative Pythagoras

The following day Barry showed me two identities involving a pair of orthogonal projections that in one fell swoop explained the trace identity and put it in a much broader context. My favorite mnemonic for these identities is *anticommutative Pythagoras*

$$C^2 + S^2 = 1, \quad CS + SC = 0, \tag{2}$$

where the "cosine" and "sine" are differences of projections:

$$C = P - Q, \quad S = P_\perp - Q = 1 - P - Q. \tag{3}$$

Supersymmetry

Here is how equations (1) and (2) are related: Suppose $\lambda \neq \pm 1$ is an eigenvalue of (the self-adjoint) C:

$$C\,|\psi\rangle = \lambda\,|\psi\rangle\,.$$

Then $-\lambda$ is also an eigenvalue of C, with eigenvector $|\phi\rangle = S\,|\psi\rangle$. This follows from

$$C\,|\phi\rangle = CS\,|\psi\rangle = -SC\,|\psi\rangle = -\lambda S\,|\psi\rangle = -\lambda\,|\phi\rangle\,.$$

The proviso $\lambda \neq \pm 1$ comes about because one needs to make sure $|\phi\rangle \neq 0$. Indeed, since P, Q are orthogonal projections, $S = S^*$, and

$$\begin{aligned}\langle\phi|\phi\rangle &= \langle\psi|\,S^*S\,|\psi\rangle = \langle\psi|\,S^2\,|\psi\rangle = \langle\psi|\,1 - C^2\,|\psi\rangle \\ &= (1-\lambda^2)\,\langle\psi|\psi\rangle\,.\end{aligned}$$

It follows that if C is trace class, then the trace of all odd powers of C coincide:

$$\begin{aligned}Tr\,(P-Q) &= Tr\,(P-Q)^{2n+1} \\ &= \dim\ker(C-1) - \dim\ker(C+1) \in \mathbb{Z}.\end{aligned} \tag{4}$$

This is illustrated in Figure 1.

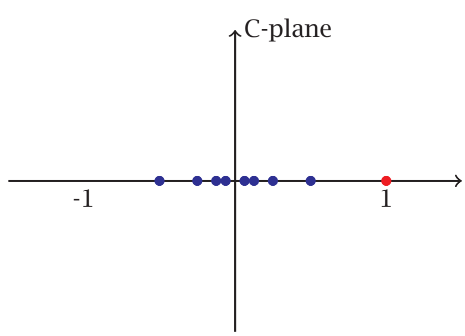

Figure 1. The spectrum of C: The paired eigenvalues $-1 < \pm\lambda_j < 1$ are marked in blue. The eigenvalue at 1 is unpaired and is marked in red.

If $P - Q$ is compact, then the right-hand side of equation (4) gives a natural "regularization" of the trace and shows that it is always an integer.

The Quantum Hall Effect

Pairs of projections play a role in the theory of the quantum Hall effect. Let me only point out how physics and math shed light on each other in the case of equation (1).

For three projections, P, Q, R, the trace identity implies that

$$Tr\,(P-Q)^3 = Tr\,(P-R)^3 + Tr\,(R-Q)^3, \tag{5}$$

which follows from

$$Tr\,(P-Q) = Tr\,(P-R) + Tr\,(R-Q).$$

This makes one wonder: Why should cubic powers of differences of projection behave linearly upon tracing?

A physical insight into the linearity comes from interpretation of $Tr\,(P-Q)^3$ as the Hall conductance. The linearity of equation (5) may then be viewed as a version of Ohm's law of the additivity of conductances.

Slow Script

Barry had the reputation of being the fastest pen in the West. So, writing these memoirs, I was actually surprised to find out that our paper [1] came out only four years later. It was written during one of Barry's subsequent visits to Israel in his tiny cramped office at the Einstein Institute at the Hebrew University.

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Martha and Barry Simon, Ludwig-Maximilians-University of Munich, 2014.

Svetlana Jitomirskaya

Quasiperiodic Schrödinger Operators

"In many years, flu sweeps the world. The actual strain varies from year to year; some years it has been Hong Kong flu, some years swine flu. In 1981, it was the almost periodic flu!" So starts Barry Simon's paper [3], also known as "the flu paper," published in 1982 and cited over four hundred times.

> *"In many years, flu sweeps the world…In 1981, it was the almost periodic flu!"*

In this paper, Barry reviewed a series of works by himself, Avron, Bellissard, Johnson, Moser, Sarnak, and others—important contributions to a newly emerging topic demonstrating a sudden burst of strong worldwide interest.

Some thirty-five years later, the "flu" is still here in full swing, and while Barry was not the one who started it nor did he have students of his own working in this field, it is fair to say that he has been largely responsible for the spread of this disease in the mathematical world.

Quasiperiodic Schrödinger operators naturally arise in solid state physics, describing the influence of a weak external magnetic field on the electrons of a crystal. In particular, for a two-dimensional crystalline layer with magnetic flux α per unit cell exerted perpendicular to the lattice plane, a certain choice of gauge reduces the model to

$$(H_{\lambda,\alpha,\theta}u)(n) = u(n+1) + u(n-1) + 2\lambda\cos 2\pi(\theta + n\alpha)u(n), \tag{6}$$

the almost Mathieu operator, with λ determined by the anisotropy of the lattice. First proposed in the work of Peierls back in the 1930s, this model did not become popular in physics until the work of Peierls's student Harper in the 1950s. The popularity further increased dramatically after the numerical study of Hofstadter in 1976. The famous Hofstadter's butterfly in Figure 2, a plot of the spectra of (6) for fifty rational values of α, was the first numerically produced fractal before the word *fractal* was even coined. That gave a significant boost to a conjecture first formulated by Azbel in the 1960s that the spectrum of (6) must be a Cantor set. Alongside the pioneering Dinaburg-Sinai work from the 1970s—the first application of KAM to show Bloch waves in a similar model—and further conjectures formulated by Aubry and Andre, it pointed to very unusual features of this model: metal-insulator transition, dense point spectrum, and Cantor spectrum. All of the above made this subject particularly appealing to mathematicians who were used to disproving rather than proving such phenomena. Meanwhile, Avron and Simon noted that earlier work of Gordon implied that this model also provides an easy example of a singular continuous spectrum. No wonder that the "flu" started spreading also in the math world, where of course it was only natural to consider the more general class of almost periodic operators.

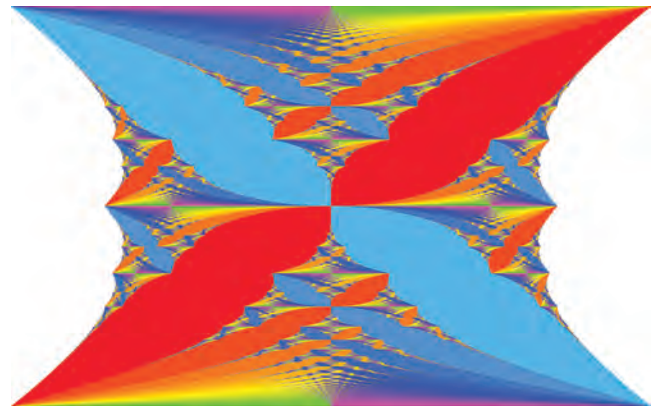

Figure 2. The colored Hofstadter butterfly.

Over the years, the field has seen a number of fundamental advances by many contributors. Sinai's and Fröhlich-Spencer-Wittwer's KAMs, Helffer-Sjóstrand's semiclassical analysis, Eliasson's reducibility/perfect analytic KAM, Bourgain's analytic revolution that made nonperturbative methods robust and allowed them to go multidimensional, deep results by Goldstein-Schlag and others all kept adding to the excitement. Then there were further physics discoveries making almost periodic models, particularly the almost Mathieu operator, relevant in new contexts. The most remarkable of those was the theory of Thouless et al. that explained the quantization of charge transport in the integer quantum Hall effect—a Nobel Prize winning discovery by von Klitzing in 1980—as connected with certain topological invariants (Chern numbers). Central to their theory is the use of the almost Mathieu operator. Moreover, predictions of Thouless et al. were verified experimentally by Albrecht, von Klitzing, et al. in 2001. Three further Nobel Prizes—quasicrystals, graphene, and topological insulators—were also linked to this field, playing a role in the unceasing spread of the "flu."

Barry's flu paper, along with his further papers from the 1980s, besides making some fundamental contributions, defined the foundations of this field in a way that made it very appealing for new students to come in. In fact, that's the way the field, despite many major advances, is seen to this day, with Chapter 9 of Barry's 1982 Thurnau Summer School lecture notes [1] still being the best quick introduction to the subject.

To give but one small illustration of how Barry's work contributed to the worldwide flu spread, one can look at Moscow in the 1980s. My advisor, Yasha Sinai, had a significant preprint problem. With no office or even table space at the Moscow State University, he kept all the preprints people had been sending him from all over the world on a big desk in his two-room apartment. The preprints were piling up, so by the late 1980s, when I was

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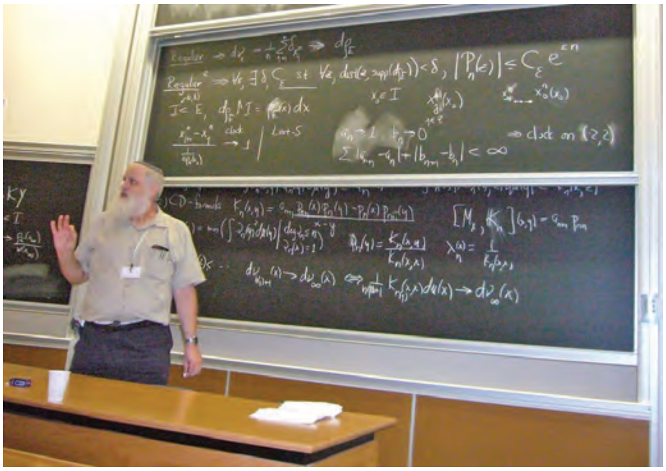

Barry Simon, Marseille, 2007.

Svetlana Jitomirskaya

his student, there was no space at all left to work on that desk, and Sinai was using a tiny bureau for writing. At some point he declared that he would throw away an old preprint for every new one he received. However some preprints were too precious to throw away, especially since it was almost impossible to get ahold of most articles by other means. Then Sinai had the idea to give thematic bunches of preprints to his various students. That's how I got ahold of all of Barry's quasiperiodic preprints from the 1980s. My role was to be a librarian for the bunch: I had them catalogued and was checking them out for two weeks at a time to various readers (and following up with the undisciplined ones who tried to hold onto them for a longer period). In Moscow it was still pre-Xerox time, but the flu found its way through the Iron Curtain nevertheless. The popularity of those preprints led to my knowing them very well, so that I could check out the one with the requested fact rather than the whole bunch. Also, I spent most of my time in graduate school as a stay-at-home mom, which gave me more time alone with those preprints. They quickly got me hooked, both by the subject and also the clarity, elegance, and freshness of Barry's writings. The never-boring style required a level of mental workout that seemed just right. The style seemed so "textbook classic" to me that when I came to UCI in the early 1990s and Abel Klein offered to take me along to Caltech "to meet Barry Simon," my first reaction was, literally, "Is he still alive?"

After the 1980s, aside from the Avron-van Mouche-Simon paper that came ε-close to proving one of the Aubry-Andre conjectures, our joint work on singular continuous spectra, and important results with Gesztesy and Last that came as corollaries of more general developments, Barry seemingly got cured himself and moved on to other areas, yet the damage to the world was already done.

Arguably, even more important for the spread was Barry's fifteen problems paper [4]. There he gave a list of fifteen (according to the title, but in reality thirty-five) important problems in mathematical physics, where, along with most fundamental questions such as "existence of crystals," he threw in the mix a couple of problems on the spectral theory of the almost Mathieu operator, listed as conjectures. Well, is there anything that could better entice a talented young person to enter the field than an attractive and accessible conjecture by Barry Simon appearing in a list like that? The answer is "Of course! It is a *wrong* such conjecture by Barry Simon." Indeed, that's how Yoram Last entered the area, disproving in his thesis written under the direction of Yosi Avron a wrong part of the almost Mathieu conjecture. Despite a lot of progress in the 1990s, some of the correct parts were not yet fully solved, and then Barry did something even bolder. In his list of (now only) fifteen problems in "Schrödinger operators in the twenty-first century" [5], Barry devoted three(!) to some of the more delicate remaining issues in the spectral theory of the almost Mathieu operator. This did not go unnoticed by the new young generation. A fresh PhD, J. Puig, solved an almost-everywhere version of the Ten Martini problem, with the enticing name coined, of course, by Barry. At about the same time, another fresh PhD, Artur Avila, set out to fully solve all three almost Mathieu problems of [5], which he methodically did, some with coauthors. This got him infected enough to devote his Fields Medal talk in 2014 entirely to the field of quasiperiodic operators, despite having other accomplishments.

It is particularly remarkable that two of the problems were unresolved only for zero measure sets of parameters, and including those in the list of fifteen for the twenty-first century highlighted the fact that the field was moving from measure theory/probability towards analytic number theory, with recent advances making it possible to seek very precise information for all values of parameters. This defined a significant trend in the later development: interplay of spectral theory with arithmetics, sometimes important only for the proofs,[1] but at times showing fascinating arithmetic phase transitions.

For example, one of the Aubry-Andre conjectures predicted a metal insulator transition for (6): absolutely continuous spectrum for $\lambda < 1$ and pure point for $\lambda > 1$, based on Fourier-type duality of the family (6) between these two regions, called subcritical and supercritical. Barry's corrected conjecture acknowledged the possibility of the singular continuous spectrum and dependence on the arithmetics. It turns out that as far as the subcritical regime $\lambda < 1$ goes, Aubry and Andre were right after all, with the final result obtained by Avila in 2008, and this is a reflection of a more general phenomenon better understood in the framework of Avila's global theory and almost reducibility theorem. However, in the supercritical

[1] *For example, the celebrated Ten Martini proof was dealing, after Puig's work, only with the remaining measure zero set of non-Diophantine frequencies, and while the end result is arithmetics-independent, the proof centers around delicate arithmetic issues.*

regime the dependence on the arithmetics is even more subtle than originally anticipated. Namely, let $\frac{p_n}{q_n}$ be the continued fraction approximants of $\alpha \in \mathbb{R}\backslash\mathbb{Q}$. For any α, θ we define $\beta(\alpha), \delta(\alpha, \theta) \in [0, \infty]$ as

$$\begin{aligned} \beta = \beta(\alpha) &= \limsup_{n\to\infty} \frac{\ln q_{n+1}}{q_n}, \\ \delta = \delta(\alpha,\theta) &= \limsup_{n\to\infty} \frac{-\ln |||2\theta + n\alpha|||}{|n|}. \end{aligned} \tag{7}$$

We say that α is Diophantine if $\beta(\alpha) = 0$ and that θ is α-Diophantine if $\delta(\alpha, \theta) = 0$. Lebesgue almost all α, θ are Diophantine. Then we have the following pair of transition results [2]:

(1) For Diophantine α and any θ the spectrum undergoes a transition from purely singular continuous for $1 < \lambda < e^{\delta}$ to pure point for $\lambda > e^{\delta}$.
(2) For α-Diophantine θ and any α the spectrum undergoes a transition from purely singular continuous for $1 < \lambda < e^{\beta}$ to pure point for $\lambda > e^{\beta}$.

This confirms a conjecture I made in 1994, also recently partially solved by Avila, You, and Zhou. Here the conjecture-making was definitely just an attempt to emulate Barry. In fact, this particular conjecture was partly motivated by Barry's work on the Maryland model, where he was the first to go so deep into the interplay between the spectral theory and arithmetic. That program was finally completed recently in our paper with Liu, presenting a full description of spectral transitions for *all* values of parameters. Moreover, [2] contains the description of exact asymptotics of corresponding eigenfunctions and transfermatrices, opening up a number of exciting possibilities for further analysis.

The almost periodic flu is currently in full strength, and new vistas—and new conjectures—constantly keep coming. With his fundamental contributions and the many cases in which he was responsible for the original infection, Barry deserves significant blame!

The Cantor function, or Devil's Staircase.

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David Damanik

Mathematical Physics at Caltech around the Turn of the Century; from Schrödinger Operators with Exotic Spectra to Orthogonal Polynomials on the Unit Circle

Like many outstanding mathematicians, Barry has changed his research area focus from time to time. This was on display at his sixtieth birthday conference at Caltech in 2006, where each of the five days was devoted to one major area to which he has made substantial contributions. Each day corresponded roughly to one decade of work; the talks on the fourth day presented work on Schrödinger operators with exotic spectra, which were the focus of much of Barry's research in the 1990s, while the talks on the fifth day presented work on orthogonal polynomials, an area to which Barry devoted most of his attention in the early 2000s.

…his moving from exotic spectra to OPUC was most natural and perhaps almost unavoidable

I was extremely fortunate to be a member of Barry's research group for most of the period 1996–2006. I had joined his group primarily due to my interest in exotic spectra, but seeing his transformation into an OPUC (orthogonal polynomials on the unit circle) guru gave me a front-row experience of witnessing something special. The ease and speed with which Barry absorbed an enormous amount of material and turned into one of history's foremost experts in an area which had initially been quite foreign to him was truly amazing.

However, in hindsight his moving from exotic spectra to OPUC was most natural and perhaps almost unavoidable. Let me explain…

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Barry had for a long time been interested in the mathematics of quantum mechanics and, in particular, the spectral analysis of Schrödinger operators $H = -\Delta + V$. The most basic questions here concern the spectrum of H, or the allowed energies of the system, and the spectral measures of H, from which one may glean information about the long-time behavior of the solutions of the Schrödinger equation $i\partial_t \psi = H\psi$. In the good old days, researchers in this field analyzed atomic models, where V vanishes reasonably rapidly at infinity, or crystalline models, where V has translation symmetries forming a full-rank lattice. In both cases the spectrum, which is a subset of the real line, will consist of nondegenerate intervals plus possibly some isolated points outside these intervals. The spectral measures, which are supported by the spectrum, will in these cases have an absolutely continuous component, plus possibly some point masses. The latter will sit at the isolated points of the spectrum, but they may also sit inside the nondegenerate intervals. While the standard decomposition of a measure on the real line will also allow for a singular continuous component, in the early days no nontrivial singular continuous components were known to occur for spectral measures of Schrödinger operators H with "reasonable" potentials V, and quite a bit of effort was devoted to actually proving that they indeed do not occur under suitable assumptions on V.

Both of these paradigms were severely challenged due to discoveries in the 1970s and 1980s. Spectra containing neither nondegenerate intervals nor isolated points (Cantor sets) were discovered in the context of almost periodic V, and examples of potentials V were found for which there actually did occur singular continuous spectral measures. The early results in these directions were obtained for suitable examples. However, both phenomena were understood at a much deeper level in the 1990s, and Barry was at the center of many of these developments. In fact, both phenomena turned out to be generic in a suitable sense, and Barry contributed key results. For example, in a series of seven papers in the 1990s with a variety of coauthors, Barry studied the occurrence of singular continuous measures in spectral theory, discussing mechanisms leading to them, as well as genericity questions about the applicability of these mechanisms. It was this series of papers that drew me to Barry's work and caused me to move from Germany to sunny Southern California.

One setting in which a strong effort was made in the 1990s to clarify precisely which assumptions preclude or allow certain spectral phenomena was the case of decaying potentials. To be specific, consider Schrödinger operators H on the half-line, that is, in the Hilbert space $L^2(0,\infty)$, for which the potential V is small at infinity. For example, one may assume a power-law decay condition $|V(x)| \le C(1+|x|)^{-\gamma}$, $\gamma > 0$, or an integrability condition $V \in L^p(0,\infty)$, $p \ge 1$. Under these assumptions the spectrum of H will consist of the half-line $[0,\infty)$, plus possibly some isolated points below zero that can accumulate only at zero. Thus the shape of the spectrum is classical in the sense described above. It is not clear, however, whether the spectral measures are classical as well, that is, whether they are absolutely continuous on $[0,\infty)$ and have only some additional point masses. Since the isolated points below zero will always correspond to point masses, the interesting question is about the nature of the spectral measures on $[0,\infty)$. It was already well known at the time that power decay with $\gamma > 1$ implies pure absolute continuity on $(0,\infty)$, that power decay with $\gamma = 1$ allows for eigenvalues inside $(0,\infty)$, and that power decay with $\gamma \le \frac{1}{2}$ allows for disappearance of the absolutely continuous components of the spectral measures. Thus the central questions concerned the γ-interval $(\frac{1}{2}, 1)$ and specifically whether the absolutely continuous part survives in all of $(0,\infty)$ and what type of singular components can occur in this energy region.

David Damanik, Barry Simon, and Shinichi Kotani, Kyoto, Japan, 2006.

When I arrived at Caltech, these questions were the most pressing ones in Barry's group. One of his many superb students, Alexander Kiselev, had written his 1997 PhD thesis on this problem and was successively able to weaken the assumption on γ that ensured the survival of the absolutely continuous spectrum on $(0,\infty)$. At the time, the best result used the assumption $\gamma > \frac{2}{3}$, but everyone was betting on $\gamma > \frac{1}{2}$ being sufficient, so that on a power scale there is indeed a sharp transition from the presence of an absolutely continuous spectrum to the possibility of its disappearance at $\gamma = \frac{1}{2}$. This was the so-called "$\frac{1}{2}$-conjecture." For a class of random decaying potentials, this spectral transition phenomenon was elucidated from a new angle, via modified Prüfer and EFGP transforms, in a 1998 paper Barry wrote together with Alexander Kiselev and Yoram Last.

In one of the major events in spectral theory in the 1990s, the $\frac{1}{2}$-conjecture was proved in 1997 simultaneously, using different methods, by Alexander Kiselev together with Michael Christ, and by Christian Remling, who was another German in Barry's group in 1996–97. In fact, a stronger result was shown that is interesting in its own right: for Lebesgue almost all $E \in (0,\infty)$, all solutions u of $-u''(x) + V(x)u(x) = Eu(x)$ are bounded.

Recall that the smallness of V near infinity can be expressed through power decay bounds, as well as L^p integrability statements. Given the results from the power-decaying case, one could reasonably conjecture that a similar transition in spectral behavior takes place on the L^p-scale at $p = 2$. So Barry, with his never-ending supply of hypertalented students, suggested to Rowan Killip (who had started his graduate studies at Caltech in 1996) that he look at this question. It took only a very short visit of Percy Deift to Caltech for this problem to fall. In the 1999 paper by Deift and Killip, a very slick proof of $V \in L^2$ implying absolutely continuous spectrum on $(0, \infty)$ was published. Killip would then go on to extend this result to periodic background and produce another outstanding thesis coming out of Barry's group on the case of decaying potentials, closing out the previous century in style.

With this spectral transition clarified, the other interesting question concerned the nature of the singular spectrum that may be embedded in $[0, \infty)$. For example, can there ever be an embedded singular continuous spectrum, and, if so, under which assumptions on V can it occur? It turned out that the answer to these questions was already implicitly contained in the approach to the first question used by Deift and Killip in terms of a more sophisticated use of sum rules. In two major events at the start of this century this was uncovered. In the process of uncovering what was really going on, Barry was naturally led to learning the history of OPUC and examining in detail the very close connections between OPUC and the theory of Schrödinger operators, which would then keep him busy for a number of years.

First was a 2001 preprint of Serguei Denissov, who used Krein systems to construct embedded singular continuous spectra for some L^2 potentials. Krein systems are continuum analogs of OPUC, and, in understanding Denissov's preprint, Killip and Simon were prompted to learn new material to understand his proof, which in turn exposed them to the world of OPUC and the realization that the heart of the matter lay in an OPUC result from the early part of the previous century that puts L^2 decay in 1-1 correspondence with a class of spectral measures.

Second, in the seminal 2003 Killip-Simon paper, quite possibly one of Barry's most influential papers ever, the

Serguei Denisov, Alexander Kiselev, Rowan Killip, David Damanik, Yoram Last, 2002.

Barry Simon, Andrei Martínez-Finkelshtein, and Jonathan Breuer at Aarhus University, Denmark, 2014.

Jacobi matrix analog of this result was worked out. Semi-infinite Jacobi matrices are in many ways the discrete analog of Schrödinger operators on the half-line, and as a consequence of this result, one could clearly see that L^2 decay not only allows the occurrence of embedded singular continuous spectra but also puts hardly any restrictions on the kind of embedded singular continuous spectra that can occur. The Schrödinger operator analog appeared later in a 2009 Killip-Simon paper, but the main thrust of the activity in Barry's group following the 2003 Killip-Simon paper was focused on digging into the existing OPUC literature, clarifying what else it may teach us about Schrödinger operators and Jacobi matrices, and, more importantly, revolutionizing the OPUC theory by introducing tools and ideas from the spectral analysis of the latter two classes of operators—and in essence paying back the favor.

In retrospect, the Killip-Simon papers laid bare what the Deift-Killip paper had only hinted at, namely, that the use of sum rules may connect coefficient/potential information to spectral information and that this is in fact a two-way street. This realization is what ushered in the new century in the mathematical physics group at Caltech and prompted Barry to move from exotic spectra to orthogonal polynomials.

Jonathan Breuer and Yoram Last

Barry between Caltech and Jerusalem

We both consider ourselves (with pride) to be students of Barry Simon, although formally this is true of neither of us. Aside from his books and papers, which were the basic texts in our graduate education, he mentored us both as postdocs, and we have collaborated, both jointly and separately, with Barry. Each paper we have written with him has been a significant learning experience, and

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Madrid, 2008.

our two joint collaborations with Barry, both dealing with asymptotics of Christoffel-Darboux (CD) kernels, are no exception.

The first project was started as one of us (JB) was just starting out as a postdoctoral scholar at Caltech with Barry as host. Barry was then writing his book on Szegő's theorem [3] and during a visit to Israel told us about Nevai's delta convergence theorem and its connection to subexponential growth of generalized eigenfunctions of Jacobi matrices. A discussion with one of us (YL) over lunch made it clear that examples could be constructed of regular measures (i.e., models with subexponential growth) for which the delta convergence fails. The job of filling in the details for the construction naturally fell to the most junior member (JB).

JB: "As I arrived at Caltech I was concentrating on filling in the details for this example. However, my wife and I had made a promise to our son that when we got to California we'd go visit Mickey Mouse in Disneyland as soon as we could. Cherie Galvez, Barry's late (and great!) secretary, suggested that since it was September and the academic year had not yet started, we go there on a weekday and not over the weekend when it's crowded, a suggestion we gladly followed. As I walked into Barry's office the following week, however, Barry looked sternly at me and said he was given to understand I had skipped a work day to go to Disneyland. As I was stuttering my response, his stern look became a devilish smile and he told me not to worry. Whatever Cherie approves is fine with him. This was my first, but not last, encounter with Barry's mischievous side and my introduction to the positive work atmosphere he creates."

This was my first, but not last, encounter with Barry's mischievous side…

Eventually the paper [1] grew to be much more than a counterexample. We realized that the eigenfunction growth condition was in fact equivalent to the delta-convergence, extended Nevai's theorem, and made several conjectures. Throughout this project Barry was the clear leader. He formulated the problems, had the best understanding of the context, realized the possible extension of the theorem, and eventually wrote up the results. This is not uncommon with projects where he is involved. However, there are exceptions, one of which is our second joint paper with him.

This paper [2] deals with stability of the convergence of the CD kernel to the sine kernel, and in this case the motivation came more from our side. The general motivating problem behind this paper, which we consider important and largely unsolved, is that of stability of asymptotic level spacing for Schrödinger operators under decaying perturbations. There is an extensive body of literature on the stability of spectral properties. However, almost none of it deals with this type of "fine" spectral property.

One of the theorems in this paper is an illustration of why it is beneficial to get Barry interested in a problem. This theorem says that universality at a point implies that this point is not an eigenvalue of the Jacobi matrix. After thinking about this problem for a little while, we presented it to Barry at lunch on the first day of one of his visits to Jerusalem. By the end of lunch he had a clear and elegant proof and even thanked us for asking the question.

Barry's mathematical prowess, his speed, his depth of insight, his unique ability to see directly to the heart of a problem or a proof are well known to his collaborators and have become legendary through their stories. Slightly less discussed, perhaps, is Barry's leadership and, in particular, his dedication to the advancement of the mathematical fields with which he is associated. The following story is an example.

The ninth OPSFA (orthogonal polynomials, special functions, and applications) international conference took place in Marseille in July 2007. Two remarkable results that were obtained just prior to the start of the conference were Lubinsky's theorem on universal limits of Christoffel–Darboux kernels and Remling's theorem on right limits of Jacobi matrices with absolutely continuous spectrum. Neither Lubinsky nor Remling was speaking at this conference. Nevertheless, Barry, who was a plenary speaker there, felt these two works had to be made known to the community. He thus asked the organizers for two extra slots to discuss these results. Even though the allotted slots were after the end of the daily schedule, both talks were very well attended and were clear and fascinating. There aren't many mathematicians who will volunteer to give two extra talks in a conference on results that aren't even theirs. Barry is not only a scholar but a true leader in his field.

Congratulations Barry on this well-deserved honor. We wish you (and ourselves) many more years of fruitful collaboration.

Andrei Martínez-Finkelshtein, Barry Simon, and Maria Jose Cantero, Madrid, 2005.

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Andrei Martínez-Finkelshtein

Orthogonal Polynomials and Spectral Theory: Barry's Revolution

Qualifying exams were tough at Moscow State University, at least at the end of the 1980s. Those in analysis consisted of real and complex analysis, harmonic analysis, and operator and spectral theory. In other words, basically the content of [2]. Looking for good textbooks, I was advised to read the first two volumes of the Russian translation of [1]. Books, especially scientific books, were cheap in the Soviet Union, affordable even by a graduate student, so I went to a bookstore to get my own copy. All volumes were out of print. Fortunately, there was a well-developed network of "Bukinists," used bookstores where I found all volumes except the most important one for me, Volume I, *Functional Analysis*. I checked unsuccessfully in several places, leaving the n-th Bukinist disappointed, when I was called by a mysterious guy who in a low voice offered me the desired Volume I for several times its official price! Indeed, Moscow at that time was a curious place, where smugglers made profit from Reed & Simon. Thus, my first and indirect encounter with Barry was not deprived of a certain excitement. When much later I heard that Barry had written a paper on orthogonal polynomials, I could not believe that it was the very same Barry Simon! I learned later how young Barry was when he wrote [1].

The role of Barry in the rapid development of the theory of orthogonal polynomials in the last twenty years, especially in the use of spectral theory techniques, is well known and documented. This exemplifies the often described and admired feature of Barry: how fast he can work. I think it was very early in 2004 when I received a message from Barry asking me to take a look at a paper. Before I had time to read it carefully, the small paper grew into a much bigger one, and I got an updated version, which had the same fate. Days (I mean DAYS) later the paper became a short book, then a longer book, then in April 2004 he sent the message:

> It's done!! It's done!! Well sort of. I have "essentially" completed my book on Orthogonal Polynomials on the Unit Circle.

Obviously, the book didn't stop growing, with about biweekly updates, until it was published in two volumes and more than one thousand pages!

It contained both classical and new results in orthogonal polynomials, spectral theory, and complex analysis. For instance, it showed the central role played by the matrices, related to OPUC in the same way as Jacobi matrices are related to orthogonal polynomials on the real line.

There were also higher-order analogues of Szegő's theorem, that is, conditions on integrability of expressions containing the logarithm of the orthogonality weight in terms of the recurrence (or Verblunsky) coefficients of the corresponding orthogonal polynomials.

About ten years after its publication, *Orthogonal Polynomials on the Unit Circle* is one of the most outstanding contributions to the field, both in terms of scientific impact and popularity, an indispensable reference for researchers, comparable to the influence that the classical monograph of Szegő [3] had in its time. It also stimulated a burst of activity in the area: "If Barry Simon is interested in orthogonal polynomials, there should be something in it!"

Barry and his collaborators also made numerous contributions to the theory of orthogonal polynomials on the real line, especially at the boundary with spectral theory.

Aarhus University, Denmark, 2014.

Orthogonal polynomials on the real line are characterized by their three-term recurrence relations, whose coefficients can be assembled into a three-diagonal (Jacobi) matrix J. The spectral measure of this semi-infinite matrix is precisely the orthogonality measure for the polynomials. Simon and his collaborators made very significant contributions to both direct (which try to read the properties of this measure from the behavior of the entries of the Jacobi matrix) and inverse spectral problems. For instance, they characterized when

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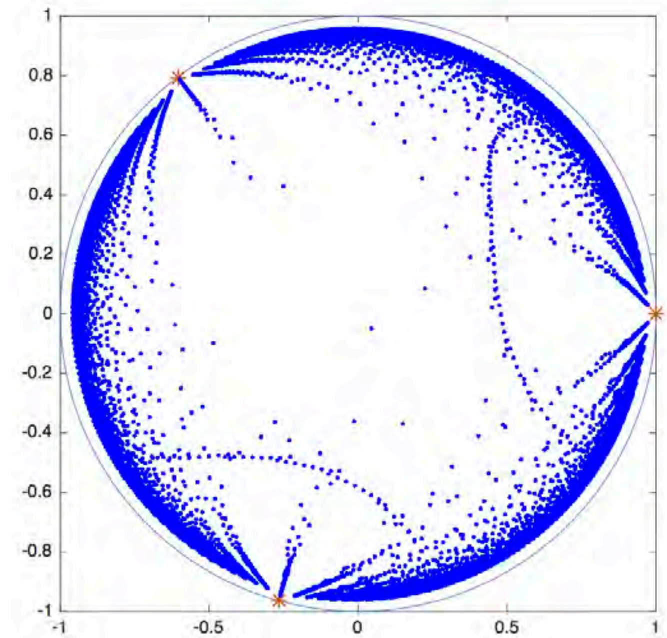
J is an ℓ^2 perturbation of either a "free" or a periodic Jacobi matrix, or they found higher-order analogues of the Szegő condition, developing for that purpose several useful technical tools.

Zeros of orthogonal polynomials have independent interest and applications, and their behavior is being actively studied. Their fine structure is strongly connected to the so-called universality behavior of the Christoffel-Darboux kernels of the associated polynomials, relevant to statistics of eigenvalues of random matrices, a subject on which there is an enormous amount of discussion in both the mathematics and the physics literature. Avila, Last, and Simon showed in 2010 that universality and the so-called "clock behavior" of zeros on the real line in the absolutely continuous spectral region is implied by convergence for the diagonal Christoffel-Darboux kernel and by boundedness of its analogue associated with second kind polynomials. They also showed that these hypotheses are always valid for ergodic Jacobi matrices with absolutely continuous spectra.

I am also interested in asymptotic problems for orthogonal polynomials, and during 2004–05 some of my work overlapped with Barry's research; together with Ken McLaughlin and Ed Saff we were focusing on the asymptotics of orthogonal polynomials on the unit circle with respect to analytic weights. The zeros of such polynomials were also, and almost simultaneously, studied by Simon. But our techniques were very different: While we were using the newly created tool of Riemann-Hilbert analysis, Simon's approach was more classical, obviously borrowing ideas from spectral theory. At a conference in honor of Percy Deift's birthday, Barry referred to the Riemann-Hilbert method (which yields impressive results but invariably requires lengthy calculations) as "driving a Caterpillar truck," as opposed to his "using an ax" in order to open a path through the jungle of the unknown towards the desired goal. Later, looking at the exhaustive results that Barry was able to obtain, I compared his method to using not an ax, but napalm.[2]



Zeros of orthogonal polynomials on the unit circle of degrees $n = 1, 2, \ldots, 150$, with respect to the weight $W(z) = |z-1|^{1/5}|z-a|^{-2/5}|z-a^2|^{-1/5}$, for $|z| = 1$, with $a = \exp(\pi i\sqrt{2}/2)$.

Madrid, 2008.

Due to our mutual interest in these topics, I got an invitation to visit Barry at Caltech in 2008 and was able to see him in action. I actually visited Caltech several times, with almost a year's spacing. These visits were highly enjoyable and stressful at the same time. I felt like a graduate student, and although it was a test for my self-esteem, it was a fantastic experience. We started to work, but it progressed slowly, partially due to my distraction with so many other things I had to learn from Barry. Our typical interaction, on the rare occasions when it was I who came up with a new idea, was like this: After spending the whole weekend immersed in lengthy computations, I would ask Cherie,[3] Barry's long-time secretary, for an appointment to see him, and I would proudly scribble my formulas on the blackboard. In the event they were right, Barry would look at them for a while in silence, slightly squinting and playing with his beard, then murmur that it was a bit late, that he needed to drive

"Barry writes books in the time others write papers"

[2] *This controversy between Caterpillar truck and ax went on for a while. Barry has a notorious sense of humor, often reflected in his writing.*

[3] *Sadly, Cherie passed away in July of 2013.*

Olga Holtz, Herbert Stahl, Guillermo López-Lagomasino, Vilmos Totik, and Kathy Driver, San Antonio, 2010.

home, but that he thought he could prove it in a few lines. An hour later (about the time it would take him to drive home from Caltech!) I would receive a scan of Barry's "doctor handwriting" containing a proof…in a few lines!

Here are a few more observations about Barry from that time:

- Barry has a vast culture. Not only does his personal toolbox contain so many mathematical results, theories, formulas, and ideas, but he masterfully applies them elsewhere. He has quite wide interests: computers and politics, just to mention two of them. He knows a lot about these topics and discusses them with passion. A preferred place for such discussions was the so-called "brown bag meetings" at Caltech, right after his seminars. One day Barry was regretting that he was spending too much time following political news, and I wondered what more he could have done without "wasting" this time.
- Barry is so fast it sometimes looks unreal. I already told how he would re-prove my laboriously obtained results when driving home. But I witnessed how he would "spoil" somebody's punchline at a seminar talk, exclaiming a few minutes into the talk, "Ah, you are going to do this and this, claiming that…!"
- On top of this, Barry is extraordinarily well organized. I mentioned that everybody visiting Barry needed an appointment to meet him, and his schedule was strictly respected.

All these factors sum up to Barry's legendary productivity: his five-volume *Comprehensive Course in Analysis* has 3,259 pages! Quoting Vilmos Totik, "Barry writes books in the time others write papers."

I will finish by mentioning Barry Simon's teaching, which has had a tremendous impact on the community. His lectures and review papers have had a great influence on numerous people in a wide range of fields in physics and mathematics and have served as an enormous source of inspiration. Barry is a passionate lecturer who masters the blackboard, something not so common in these days of multimedia presentations. I remember that in June of 2005 Barry gave a two-day seminar at the University Carlos III de Madrid. It was bad timing: the main lecture halls were closed for some reason, and we had to squeeze into a small room with a tiny board, about 5×7 feet! We were all rather concerned about Barry's reaction, but he masterfully gave the whole course, using every single one of those 35 square feet.

In contrast with that, the 9th International Symposium on Orthogonal Polynomials, Special Functions, and Applications took place in Marseille two years later, and Barry volunteered to give an extra late-evening session on some hot topics on orthogonal polynomials. The main lecture room in the International Center for Mathematical Meetings of the French Mathematical Society in Luminy was spectacular, the blackboard made of nine large moving panels. The use of this surface by Barry was masterful again; all blackboards were filled with formulas and theorems, going up and down in front of the audience in an impressive exhibition of his communication skills.

Throughout his scientific career Barry Simon has had a special concern for young (and not so young) scientists. The long list of people who have worked with Barry Simon is remarkable and includes many PhD students, postdoctoral fellows, and collaborators from many fields. It is a privilege and an honor for me to be part of this list. There are still several open questions and unfinished projects with Barry, and I hope to be able to ask him for an appointment again in the near future and to scribble my formulas on a blackboard, even risking to hear from him, "I think I can prove it in a few lines!"

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Credits

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