Mathematical Physics at Princeton in the 1970s

by Barry Simon (Pasadena, USA)

Barry Simon got his PhD. in Physics under Arthur Wightman in 1970, was on the Princeton Faculty for 12 years and, since 1981, has been at Caltech where he is currently IBM Professor of Mathematics and Theoretical Physics and Executive Officer (aka Chair) of Mathematics. He is the author of 384 research articles and 16 scientific monographs including the Reed-Simon series. He is especially proud of his many graduate students and mentees. He has been an editor of Communications in Mathematical Physics for over 30 years, served on the IAMP board in the 1980's and is again a member of that board.

The 1970s at Princeton were a unique time in the modern development of mathematical physics. I think many others who were there during that period have a feeling that it was a magical time when a multitude of resources brought together a remarkable group of talents—how remarkable is seen by the number of people there then who have remained leaders in the field.

Princeton had a long tradition in the overlap of rigorous mathematics and theoretical physics, going back at least to Sir James Jeans who was a professor there from 1906–10. There were important interactions from the earliest days of the Institute so that Wigner at Princeton and von Neumann and Einstein at IAS contributed to the Princeton tradition from the late 1920s through the early 1950s. With the appointments in the 1950s of Valya Bargmann and Arthur Wightman added to Wigner, there were, for many years, three joint appointments in mathematics and physics at the University.

There was a changing of the guard in the early 1970s with the retirements of Wigner and Bargmann. I entered grad school in physics in 1966, became junior faculty (first only in Math, then jointly) in 1969 and tenured in 1972. I met Elliott Lieb at the famous 1970 Les Houches summer school and collaborated with him when we were together at IHES in the fall of 1972. I came back convinced we should lure him away from M.I.T., and Wightman agreed enthusiastically. As part of due diligence, Arthur or I met with all the senior faculty in Physics. A not atypical reaction came from one of the more thoughtful experimentalists who remarked that it seemed to him there was a huge change of outlook in moving from Bargmann, Wightman, and Wigner to Lieb, Simon, and Wightman. But, of course, in the end Lieb's appointment sailed through and he joined us in the fall of 1974.

That experimentalist was of course correct—the group was now solidly of a theorem–proof type and there was a shift of center of focus to models of statistical mechanics and the mathematical structure of the Hamiltonians of nonrelativistic quantum mechanics as well as the axiomatic/constructive QFT program started by Wightman. There is no question that Wightman had begun this trend years before.

One reason for the increased activity was the replacement of two near-retirement faculty by younger and more active researchers. But there were two other factors. We were fortunate that Ed Nelson and Freeman Dyson were both in mathematical physics phases of their careers. Moreover, in the 1960s, Bargmann and Wigner had very few grad students and postdocs (although Wightman had Huzihiro Araki, Peter Burgoyne, John Dollard, Eduard Prugovecki, Arthur Jaffe, Oscar Lanford, Robert Powers, Lawrence Schulman, Jerrold Marsden, Christian Gruber, Eugene Speer, and Gerald Goldin as grad students in the ten years before I got my PhD). Lieb and I had both grad students and postdocs and the two departments (and some outside agencies) provided support for a remarkable number of young mathematicians and physicists.

Just a listing of these young people is likely to cause dropped jaws among the younger generation (and I apologize for anyone I left out!). People there for multiple-year post-doc/junior faculty appointments/visits in that era included Michael Aizenman, Sergio Albeverio, Yosi Avron, Jean Bricmont, Jan Brascamp, Jürg Fröhlich, Francesco Guerra, Ole Heilmann, Ira Herbst, Raphael Høegh-Krohn, Abel Klein, Larry Landau, Charles Radin, Mike Reed, Lon Rosen, Simon Ruijsenaars, Israel Sigal, and Alan Sloan. Other post-PhDs there for at least several months (some at the Institute) included René Carmona, Brian Davies, Volker Enss, Martin Klaus, John Morgan, Vincent Rivasseau, Heinz Siedentop, Erhard Seiler, and Aubry Truman. Interactions with Rutgers faculty and their visitors involved additional people. In the mid-1970s, Tom Spencer was at Rutgers. Once Joel Lebowitz moved there in 1977, we benefitted from his occasional presence as well as some of his visitors like Ingrid Daubechies and Herbert Spohn.

In the 1970s, Lieb had Rafael Benguria as a student, Nelson had Charles Friedman, William Priestley, Richard Hevener, Robert Wolpert, and Gregory Lawler. My students were Tony O'Connor, Jay Rosen, Bob Israel, Evans Harrell, Percy Deift, George Hagedorn, Steven Levin, Mark Ashbaugh, Peter Perry, and Keith Miller (Antti Kupiainen was officially my student, but he really worked with Tom Spencer, then at Rutgers). Wightman's students in that era were Chuck Newman, Stephen Fulling, Alan Sokal, and Rafael de la Llave. Francis Narcowich finished in 1972 with Wigner as his advisor. Jennifer and Lincoln Chayes started in the 1970s, although they only finished (with Aizenman and Lieb as their joint advisors) in 1983.

Some anecdotes may give a flavor of what both Princeton and the field were like at the time. Given limited space, I can only touch on a few items and so miss out on saying much about the plethora of results produced during that period at Princeton in the 1970s, although the core of the excitement included work in constructive quantum field theory (Nelson's Euclidean Markov fields, Guerra's Nelson symmetry, Guerra–Rosen–Simon on stat mech methods in Euclidean field theory, Seiler's work on Yukawa, Eckmann–Epstein–Fröhlich on scattering theory), statistical mechanics (Fröhlich–Spencer–Simon and Dyson–Lieb–Simon on continuous symmetry breaking, Fröhlich–Israel–Lieb–Simon on Peierls argument, Aizenman on percolation, Sokal on Lee–Yang), lots of NRQM (Simon on quadratic forms in NRQM, Simon and Sigal on resonances, Avron–Herbst on electric fields, Avron–Herbst–Simon on magnetic fields, Lieb and Sigal on negative ions, Lieb–Thirring on stability of matter, Perry–Sigal–Simon on Mourre theory) and deep new inequalities (Lieb–Thirring bounds, Brascamp–Lieb–Luttinger rearrangement inequalities, Brascamp–Lieb inequalities, optimal constants by Beckner and Brascamp–Lieb, Lieb's work on Gaussian optimizers, Aizenman–Simon on Harnack inequality). And this is just some of what developed—I've left out a lot of great stuff in this brief listing.

Lieb and I began working on Thomas Fermi in the fall of 1972 (when, as I mentioned, we were both at IHES). He suggested that we examine its relevance to atomic Hamiltonians. Five years before, I had taken an intermediate QM course from Wightman that included a discussion of TF, including what he dubbed Teller's theorem: that molecules do not bind in TF theory. I told Elliott about this, suggesting that its inability to get this physics right probably meant the TF was just an ad hoc theory of no relevance to the real problem. He came back a day latter and announced to me, "Mr. Dalton's hooks are in the outer shell." His point was that if TF theory described the bulk as one might hope for a semiclassical theory, one shouldn't expect it get molecular binding. (Of course, in his famous work with Thirring a few years later, they realized that not only was Teller's theorem not a problem, it was a key step in a proof of stability of matter.)

We set to work and, within a few months, we had a proof of the $Z^{7/3}$ result for total binding energy, but only if the attractive Coulomb potential was cut off. We dubbed the last step of removing the cutoff "pulling the poison Coulomb tooth." I left Paris for Marseille in the New Year without our solving this technical problem. In March, I took the train to Paris, and working in Elliott's apartment, we completed the proof of the full theorem.

We wrote an announcement which we sent off to *Physical Review Letters*. In July, when we were both in Copenhagen for a conference, we got the referee's report. I paraphrase it because I certainly don't still have a copy: "This paper is one of the worst I've ever seen. It is a sequence of unproven assertions," it began. The latter was, of course, correct—the proofs were many pages—and this was only an announcement. The report continued, "many of which are obviously false. For example, the authors assert that the TF density ρ is C^∞, which would make ρ identically 0, 1 or ∞ depending on the value of C."

Elliott insisted that our letter of complaint focus on the points of physics the referee got wrong rather than his total lack of understanding of modern analysis. We demanded and got a new referee who recommended acceptance. From a comment he made to us, I think that second referee was Freeman Dyson.

While on the subject of amazing referee reports from what I came to think of as *Physical Review Lottery* (I've had almost as many papers rejected there as accepted, and I think the importance/quality of the rejected ones is at least as high as the accepted ones), here is a favorite. I had understood that the theory of selfadjointness via hypercontractive semigroups developed in 2D QFT could be used to prove an asymmetry between the positive and negative parts of the potential. I showed that a Schrödinger operator was essentially selfadjoint on the C_0^∞ functions if the potential was positive and in L^2 with Gauss measure, even though it was known that for the negative part of V, one needed a stronger condition. I conjectured L^2_{loc} should be enough, and between my producing a preprint and the paper's publication, Kato had proven this conjecture with a totally different method depending on what I called Kato's inequality.

Kato also allowed magnetic fields and, three years later, in trying to understand what his inequality was saying, I realized it implied that the ground state energy of a nonrelativistic Hamiltonian goes up when any magnetic field is turned on. In fact, I then found a three-line proof which I submitted to *PRL* as shorter than a one-page paper entitled "Universal diamagnetism of spinless Bose systems." The report said (and again, I paraphrase): "Since there are no stable spinless bosons in nature, the result of this paper is of limited physical applicability. But it is nice to see something nontrivial proven in just a few lines, so this paper should be accepted as an example to others."

There is a postscript about this paper that illustrates the atmosphere at Princeton in those days. There was a weekly "brown bag lunch." The three joint senior faculty and often also Dyson and Nelson attended together with 10–20 postdocs and graduate students. After lunch, there were brief presentations/discussions. At one, I described this result and mentioned that I conjectured that this was a zero temperature result and that there should be a finite temperature result that was an inequality between integral kernels of semigroups, and I was working on it. Almost immediately, Ed Nelson interjected: "You know that follows from the stochastic integral magnetic field version of the Feynman–Kac formula." Stirred by this, I found a direct proof from Kato's inequality which, in typical fashion, Ed refused to be a coauthor of. These inequalities I dubbed "diamagnetic inequalities," now used often in the study of quantum mechanics in magnetic fields.

The next story is a commentary on changes in technology and an illustration that the academic pecking order may not have changed much. In the early spring of 1970, Høegh-Krohn and I finished our paper on hypercontractive semigroups (in which we introduced the name—Nelson had invented the concept but objected when I told him our name since one of the conditions involved being bounded—so he suggested "hyperbounded." I told him hypercontractive sounded better, so we used that) and their use in discussing cutoff quantum field Hamiltonians in two space time dimensions. It was a competitive field and we were anxious to get the preprint out.

This was before electronic typesetting; instead, typewriters were used. Symbols were often put in by a secretary putting an overlay on a typewriter key or if she (and in those days they were all she's) was fortunate enough to have an electronic typewriter, replacing the standard type ball by a symbol one. Moreover, xeroxing was still way too expensive to xerox 150 copies of a paper. Instead, one used mimeograph machines where there were green cloth stencils. The typewriter made holes in the stencils and the mimeograph machine forced ink through those holes onto paper. The first proofreading wasn't so bad—the secretary put a sheet of paper between the cloth and its backing, so the green cloth removed by the typewriter made marks on the paper that you could proofread them. The secretary then could make small changes by using a technique to fill in the holes forming a word and retyping. Rearranging paragraphs or adding paragraphs was difficult, involving literal cutting and pasting! And if you wanted to proofread after a first round of corrections, you had to hold the stencils up to the light and go crazy trying to read what was there!

The math department support staff at Princeton was run by a tough lady named Agnes Henry, who was what would now be called a departmental administrator but was then the senior secretary. When we finished our first chapter, I gave it to Ms. Henry who assigned it to a secretary who returned parts as she typed. When we got the second chapter written, I gave it directly to the secretary who had been typing the first part. About a day later, Agnes left a note in my mailbox (she couldn't email me because, in 1970, that didn't exist) asking me to come and see her. She made it clear that it was a terrible breach for me, then a first-year instructor (although appointed to be an assistant professor the next fall) to directly hand work to a secretary and that not only would drafts have to go through her, but it appeared that things had gotten so busy she didn't possibly see how the rest of the paper could get typed before the summer break. In case I hadn't gotten the message, she added that if I'd come to her directly, she would have tried harder to accommodate my schedule.

True to her word, the paper was typed when I returned after a summer in Cargèse and Les Houches. But since she thought I might not have learned my lesson, she said that they could run the mimeo machine but it would take several weeks before they could find the time to collate the roughly 150 copies I wanted to mail out (snail mail, of course!). So I set up the highest class collating party you've ever heard of. There were about six of us, including Mike Reed and me and David Ruelle and Oscar Lanford who were visiting. It took about an hour as we circled several tables collating, but I finally got the paper out.

In the mid-1970s, I spent a lot of time thinking about semiclassical estimates. I was drawn to the fact that classical phase space gave leading asymptotics for $N(V)$, the total number of bound states of a one-body Schrödinger operator in potential V. I learned of this result from work of Martin and of Tamura (although I didn't know it at the time, Birman–Borzov and Robinson had similar results). All these proofs assumed regularity on V, at least continuity. Such a restriction seemed unnatural to me, and in mulling over what was needed to remove it, I realized that the key to proving this for the most natural class (namely, those V for which the classical phase space for $|V|$ was finite, that is, V in $L^{n/2}$ in n space dimensions) was a bound that a multiple of classical phase space was an upper bound for $N(V)$. Since I knew such classical bounds were true in other cases (e.g., the partition function, by results of Golden and Thompson), I conjectured that there was such a bound, that is, that in dimension 3 or more, $N(V)$ was bounded by a multiple (only depending on n) of the $n/2$ power of the $L^{n/2}$ norm of V.

In the spring of 1975, I realized that trace ideals were the natural language for the problem, and that by using some interpolation theory for such operators, I could prove the weaker result where the norm was replaced by the sum of a pair of L^p norms with p's slightly above and slightly below the correct value. I couldn't do any better than this, so I wrote the results up and shopped two conjectures around among colleagues: the semiclassical bound and a conjecture about certain integral operators lying in weak trace ideals that would imply the semiclassical conjecture.

Among those I asked about this were Elliott Lieb and Charlie Fefferman. Elliott was already thinking hard about semiclassical bounds—in the summer of 1975, he and Walter Thirring submitted their great paper on the stability of matter where a central ingredient was a semiclassical bound on the sum of eigenvalues. This is a special case of the Lieb–Thirring bound which appeared in a paper written in the 1975–76 academic year.

At tea one day in the fall of 1975, Charlie introduced me to a visitor to the Institute named Michael Cwikel who was an expert on interpolation theory. Charlie suggested I explain my conjecture and its interest to Michael, and I did.

As someone with a joint appointment, I had offices and mailboxes in both the Physics and Math departments. I mainly used the physics office, only using math to "hide out." I made a point though to check my math mailbox at least once a week, usually on my way home. Several months later, as I was walking along the corridor in Jadwin Hall from my physics office to head across to check my Fine Hall math mailbox, I passed by Elliott's office and he stopped me to say: "I think I can prove your conjecture" and he proceeded to sketch for me his elegant path integral approach to getting the semiclassical bound. After that, I decided to stop at my math mailbox even though I was late getting home. What did I find there but a note from Cwikel explaining that he had proven my trace ideal conjecture and thereby also proven the semiclassical bound. All within an hour!

In the summer of 1976, I visited the Soviet Union to attend a conference in a small resort town outside what was then Leningrad. It was an ideal location/conference for me. The Russians whose work interested me couldn't get out of the Soviet Union. The conference was on Stat Mech and QFT, so the Sinai–Dobrushin group from Moscow attended (indeed, I gave them a series of informal lectures on my then recent work with Fröhlich–Spencer and with Dyson–Lieb on phase transitions with continuous symmetry breaking). While they were not attending the conference, the venue was close enough to Leningrad that Birman and Solomjak and some younger people from their group could come out to talk with me. (I didn't know it at the time but was later told it took some considerable effort for them to get permission to do that.)

Birman, in his unfailingly polite manner, began by saying that they thought my weak trace ideals paper was very interesting. While they hadn't quite succeeded in the details of a counterexample, they were fairly certain my weak trace ideal conjecture was wrong! "That's strange," I said, "because it has been proven by Cwikel" and I proceeded to show them the proof. In further discussion, they said that a younger worker in their group, Gregory Rozenbljum (as his name was then transliterated by the AMS!), had announced a result that implied the semiclassical bound I conjectured. Given Cwikel's work (and, of course, I also told them of Lieb's work), they decided Gregory had better write his stuff up awfully fast. And so were born the CLR bounds.

I hope I've conveyed some of the excitement of Princeton during the 1970s.