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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.)

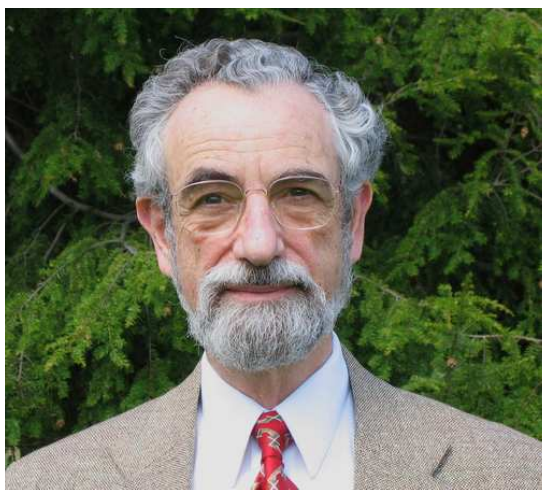
Birman, in his unfailingly polite manner, began by saying that they thought my weak trace ideals paper was very interesting. While they hadn't quite succeeded in the details of a counterexample, they were fairly certain my weak trace ideal conjecture was wrong! "That's strange," I said, "because it has 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.

Celebration of Elliott Lieb's 80th birthday in Berlin

From July 12 through 14, 2012, a conference on the "Mathematics of Many-Particle System" was held at TU Berlin in honour of Elliott Lieb on the occasion of his 80th birthday. In recognition of his scientific achievements in Mathematical Physics, conferences took place in Copenhagen, Vienna, and at Rutgers University ten and twenty years ago, but to hold this conference in Berlin to celebrate his 80th birthday appeared quite natural. Namely, after he was awarded a prestigious Humboldt Fellowship of the German Alexander-von-Humboldt Foundation, Elliott's visits to Berlin have become more and more frequent in the past decade. By now, Berlin has become an integral part of Elliott's and his wife Christiane's life.

Elliott's scientific achievements viewed from a physicist's perspective cover a broad spectrum including the quantum mechanics of atoms, molecules, condensed matter, and matter in general, statistical mechanics, and quantum field theory. Likewise, the mathematics he uses range from algebraic methods in the theory of integrable systems to analysis, partial differential equations and the calculus of variations.



Elliott Lieb

In spite of this diversity, the analysis of interacting particle systems is a thread that links Elliott's scientific results: Most of his papers are - directly or indirectly - concerned with the analysis of interacting particle systems. This is visible in his first great scientific successes in the 1960's that dealt with "exact solutions" of models from statistical mechanics, and in the deep entropy and concavity inequalities he derived in the 1970's, which are now part of the foundation of modern quantum statistical mechanics and have recently gained importance in quantum computing. Recent work of Elliot jointly with R. Frank was presented by the latter at the conference. Among his most widely known papers are certainly those on the quantum mechanics of nonrelativistic matter and its stability which linked non-relativistic matter to Thomas-Fermi theory in the late 1970's. One key ingredient is the Lieb-Thirring inequality whose recent extension to particles in two dimensions with anyon statistics was presented by J.P. Solovej at the conference. The second crucial ingredient is the Lieb-Oxford inequality which was the focus of R. Benguria's lecture. These results were, and still are, extended by Elliott and also by other scientists in various directions, e.g., to include (classical or quantized) magnetic fields or to pass to (pseudo-)relativistic systems. It should be mentioned that the mathematical methods used for these extensions had to be newly developed, and Elliott made remarkable progress in the late 1990's that unified the analysis of the stability of these seemingly completely different models to a few basic principles. Elliott's scientific activity is ongoing to this very day, and of his papers of the past decade the derivation of the energy asymptotics for the charged Bose gas is an admirable mathematical tour de force.

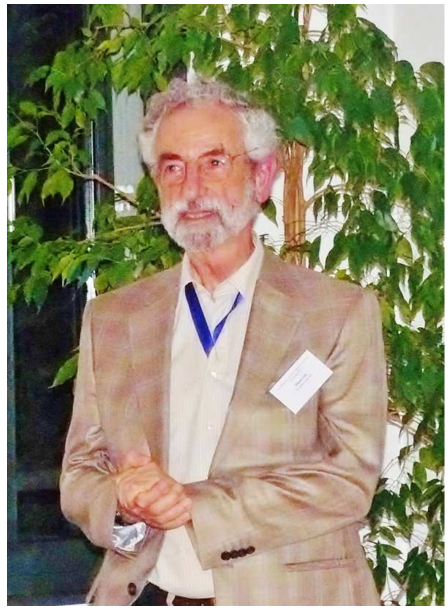
Elliott's results are deep and have a long-term impact on mathematical physics, and on mathematics and physics, in general. A good illustration of this is the fact that, while Elliott's papers do not have hundreds of citations like many papers in physics do, even his papers from fourty years ago are numerously cited every year until today.


Boat trip on the river Spree through the centre of Berlin
(Courtesy of Jana Exnerová and Ruedi Seiler)

Many prizes are awarded to Elliott and his scientific achievements, which are appreciated in both physics, mathematics, and in mathematical physics. These prizes include the Birkhoff medal of the AMS, the Max-Planck Medal of the DPG, the Danni Heinemann Prize in mathematical physics, and the Poincaré Prize of the IAMP in 2003. Indeed, Elliott was one of the driving forces in founding IAMP in 1976, and he served as its president from 1982 through 1984. Elliott also has several honorary doctoral degrees, he is member of the US-American, Austrian, and Danish Academies of Science, and he is a fellow of several scientific associations.

All these aspects had an influence on the conference in Berlin and its scientific program. The selection of the speakers reflected Elliott's scientific interests. The speakers list included distinguished mathematical physicists such as Rafael Benguria, Rupert Frank, Sabine Jansen, Mathieu Lewin, Bruno Nachtergaele, Robert Seiringer, Heinz Siedentop, Jan Philip Solovej, Lawrence Thomas, Kenji Yajima, and Jakob Yngvason.

At the conference dinner and at the end of the conference, all agreed that it has been a wonderful meeting and great scientific success.



Conference dinner in the Herrmann von Helmholtz Building
(Courtesy of Ruedi Seiler and Jana Exnerová)

All participants cordially congratulate Elliott to his birthday, and everyone is looking forward to the next meeting in 2022!

Happy Birthday, Elliott!

Volker Bach & **Ruedi Seiler**

On ground-state phases of quantum spin systems

by Sven Bachmann (Davis, USA)

Sven Bachmann is a visiting assistant professor and postdoctoral researcher at the University of California, Davis, where his mentor is Bruno Nachtergaele. He completed his PhD in mathematical physics at the ETH in Zurich in 2009. He has worked on non-equilibrium statistical mechanics, specifically on quantum charge transport and current fluctuations. Recently, he has been interested in quantum spin systems, concentrating on the understanding of ground state phases: Their definition, their characteristics and the transitions between them.

Elucidating the ground-state properties of a complex quantum system may be the most basic task in quantum theory, but it is in general a non-trivial one and one that provides good insight in the system's behaviour, including aspects of the dynamics such as scattering theory. Among the prominent examples are the ground-state properties of N-body quantum systems, the question of the existence of a ground state for quantum field theories and its construction, and the understanding of ground-state spaces of quantum lattice systems. The renewed attention received by the latter in recent years arose in particular from the realization that non-trivial ground-state spaces could be used as robust memories in quantum information.

Rather than studying the spectral problem of a given Hamiltonian in order to obtain very precise but quite specific properties of a model, a fruitful approach is to identify general classes of systems that share similar qualitative properties. In fact, the representation of a real system by a mathematical model follows that philosophy: the model is assumed to be in the same class as the physical system. Similarly, a large part of statistical physics over the past few decades has been devoted to the study of phase transitions, namely the transitions between qualitatively different thermal state(s) of a system – the quintessential example being the melting of a solid to a liquid. Understanding qualitative transitions tuned by a parameter has found applications well beyond Gibbs states at different temperatures. Let me mention, e.g., Bose-Einstein condensation in the grand canonical ensemble, the orientational order-disorder transition of lyotropic liquid crystals tuned by the density, the conductor-insulator transition of dirty metals tuned by the disorder strength, localization-delocalization transitions in random matrices, percolation thresholds, and the Hausdorff dimension of sample paths of SLE_κ.

The name 'quantum phase transition' has – somewhat unfortunately – been given to a transition happening at zero temperature within the ground state space of a family of Hamiltonians. The drosophila of the subject is the one-dimensional transverse field Ising

Hamiltonian,

$$H_N(\lambda) = -\sum_{i=1}^{N-1} \sigma_i^x \sigma_{i+1}^x - \lambda \sum_{i=1}^{N} \sigma_i^z, \qquad \lambda \geq 0,$$

defined here on a chain of length N, namely on $\mathcal{H}_N = \otimes_{i=1}^N \mathbb{C}^2$ with open boundary conditions. The matrices σ_i^x, σ_i^z are Pauli matrices acting on the ith site, i.e., tensored to the identity everywhere else. Minimizing the first term alone yields two possible product states of either eigenstates of σ^x, whereas the minimizing state of the second term has all spins in the $+1$ eigenstate of σ^z. Between these two extreme cases, λ parametrizes a phase transition, which can be studied explicitly [1]. On both sides of the critical parameter λ_c, the Hamiltonian has a spectral gap above the ground-state energy which is uniformly bounded in the length of the chain, and correlations decay exponentially. However, as indicated by the limiting cases, the ground state is non-degenerate for $\lambda > \lambda_c$, while there is a double degeneracy whenever $\lambda < \lambda_c$. The spectral gap closes at $\lambda = \lambda_c$. One of the fascinating aspects of this model is the theoretical prediction of an emergent E_8 symmetry of the excitation spectrum at the critical point [2], traces of which have recently been observed [3] (for a thorough mathematical discussion of the matter, see the recent article [4]).

With this textbook example of a quantum phase transition in mind, let us consider the first, fundamental question: What is in fact a ground-state phase? For the purpose of this note, the attention will be restricted to systems with gapped ground states. In the Ising case – as in other explicitly solvable models such as the XY-chain [5] – the transition occurs when the gap closes. Reciprocally, there cannot be a phase transition without closing the spectral gap above the ground state. Hence the following definition [6, 7]: Two gapped Hamiltonians H_0 and H_1 are in the same phase if there exists a smooth path $H(s)$ for $s \in [0, 1]$ such that $H_0 = H(0)$, $H_1 = H(1)$ and the spectral gap remains open, uniformly in the size of the system and in the parameter s. From a physical point of view, this implies that the respective ground states could be reached from one another by adiabatically following the given path, e.g., by manipulating external fields.

This Ising type of transition is quite similar to the classic phase transitions – and indeed it corresponds to the $\beta \to \infty$ limit of it. The introduction of the toric code model in [8] added a new dimension to the discussion of ground-state phases. Its nearest neighbour interaction between spins-1/2 can be defined on an arbitrary two-dimensional lattice, with deep consequences for the structure of the ground-state space. Indeed, when defined on a surface of genus g, the ground-state degeneracy is equal to 4^g, a property called 'topological order'. Moreover, any local observable – an operator that acts non-trivially only on a finite, contractible subset of the lattice – reduces to a multiple of the identity when restricted to the ground state space. In this respect, a second definition is natural [6]: Two gapped ground states Ω_0 and Ω_1 are in the same phase if there exists a local unitary map such that $\Omega_1 = U\Omega_0$.

Understanding the relation between these two definitions allows for a deep clarification of the notion of a gapped ground-state phase which takes topological order into account.

We consider a countable number of quantum systems labelled by $x \in \Gamma$, where Γ is equipped with a distance $d(\cdot, \cdot)$. Each point carries a finite dimensional Hilbert space $\mathcal{H}_x$.

The Hilbert space of a finite subsystem $\Lambda \subset \Gamma$ is the tensor product $\mathcal{H}_\Lambda = \otimes_{x\in\Lambda}\mathcal{H}_x$. The interaction between these 'spins' is local in the sense that the Hamiltonian is of the form

$$H_\Lambda(\lambda) = \sum_{X\subseteq\Lambda} \Phi(X,\lambda)\,,$$

where the self-adjoint operator $\Phi(X,\lambda)$ acts on X only and $\Phi(\cdot,\lambda)$ decays in the size of the set X. For example, a finite range interaction would have $\Phi(X,\lambda) = 0$ if $\mathrm{diam}(X) \geq R$. We consider a family of local Hamiltonians parametrized by λ, e.g., the strength of an external magnetic field. We assume that $H_\Lambda(\lambda) \geq 0$.

Such local Hamiltonians generate a dynamics $\tau_t^\Lambda(\cdot)$ on the algebra of observables characterized by a finite 'speed of sound' as made precise by Lieb-Robinson (LR) bounds, see [9] and the references therein in a previous edition of this *Bulletin*. Let A and B be two bounded operators acting on disjoint subsets X and Y respectively. Under mild assumptions on the structure of Γ and with sufficient decay of the interaction, the following bound holds:

$$\left\|[A,\tau_t^\Lambda(B)]\right\| \leq C(A,B)\exp\left[-\mu(d(X,Y) - v_\Phi|t|)\right],$$

where v_Φ is the Lieb-Robinson velocity, and $\mu > 0$.

The locality of the dynamics exhibited by the LR bound plays a central role in the following theorem, where we come back to the question of gapped ground states. For a finite Λ, let $\mathcal{S}_\Lambda(\lambda)$ be the ground-state space of $H_\Lambda(\lambda)$, i.e., the set of states such that $\rho(H_\Lambda(\lambda)) = 0$. Let $(\Lambda_n)_{n\in\mathbb{N}}$ be an increasing and absorbing sequence of finite sets converging to Γ. The set $\mathcal{S}_\Gamma(\lambda)$ of states in the thermodynamic limit is then obtained by standard compactness arguments from $\mathcal{S}_{\Lambda_n}(\lambda)$.

Theorem. *Suppose that the family of interactions $\Phi(X,\lambda)$ is uniformly bounded, of class C^1 for $\lambda \in [0,1]$, and has finite range. If the spectrum of $H_\Lambda(\lambda)$ is characterized by a uniform lower bound $\gamma > 0$ above the ground-state energy, then there exists a cocycle of quasi-local automorphisms $\alpha^\Gamma_{\lambda,\lambda_0}$ of the algebra of observables such that*

$$\mathcal{S}_\Gamma(\lambda) = \alpha^\Gamma_{\lambda,\lambda_0}(\mathcal{S}_\Gamma(\lambda_0)), \qquad \lambda,\lambda_0 \in [0,1].$$

Note that the finite range assumption can in fact be relaxed to a sufficient decay [10]. The automorphisms $\alpha^\Gamma_{\lambda,\lambda_0}$ are quasi-local in the sense that a strictly local observable is mapped to an almost local one, namely to one which is supported on a finite number of spins, up to an exponentially small correction. We note three important facts:

1. $\alpha^\Gamma_{\lambda,\lambda_0}$ acts in the thermodynamic limit, i.e., on the algebra of quasi-local observables;
2. $\alpha^\Gamma_{\lambda,\lambda_0}$ is invariant under local symmetries of the interaction: If π is an automorphism such that $\pi(\Phi(X,\lambda)) = \Phi(X,\lambda)$ for all $X \subset \Gamma$ and $\lambda \in [0,1]$, then π is also a symmetry of $\alpha^\Gamma_{\lambda,\lambda_0}$, i.e., $\alpha^\Gamma_{\lambda,\lambda_0}\circ\pi = \alpha^\Gamma_{\lambda,\lambda_0}$;
3. $\alpha^\Gamma_{\lambda,\lambda_0}$ maps the complete set of ground states of one model, at λ_0, to the set of ground states of another model, at λ.

In other words, the automorphism preserves the general structure of the ground state space.

The spectral flow $\alpha^{\Gamma}_{\lambda,\lambda_0}$ is a generalization of the 'quasi-adiabatic continuation' of [11], and all its properties follow from its explicit form. In fact, the theorem is constructive and the automorphism is obtained as the thermodynamic limit of a unitary conjugation defined on the algebra of local observables,

$$\alpha^{\Gamma}_{\lambda,\lambda_0}(A) := \lim_{n\to\infty} V_n^*(\lambda) A V_n(\lambda), \tag{1}$$

where $V_n(\lambda_0) = 1$ and $V_n(\lambda)$ solves a Schrödinger equation $V_n'(\lambda) = \mathrm{i}D_n(\lambda)V_n(\lambda)$. The generator $D_n(\lambda)$ is given explicitly by

$$D_n(\lambda) := \int_{-\infty}^{\infty} dt\, w_\gamma(t) \int_0^t du\, \tau_u^{\Lambda_n}(H'_{\Lambda_n}(\lambda)) = \sum_{Z\subset\Lambda_n} \Psi_n(Z,\lambda), \tag{2}$$

where $w_\gamma \in L^1(\mathbb{R})$ decays almost exponentially and has a Fourier transform which is supported in $[-\gamma,\gamma]$. The first equality is a definition. The second, however, requires the use of the LR bound. Indeed, it is the locality of τ_t that ensures that $D_n(\lambda)$ can be cast as a local interaction $\Psi_n(Z,\lambda)$ with fast decay. This local structure implies in turn a LR estimate for the unitary dynamics (1) in 'time' λ. With this bound, it is a standard application to prove the existence of the thermodynamic limit $\alpha^{\Gamma}_{\lambda,\lambda_0}$.

On a side note, it is worth mentioning that the spectral flow is not restricted to the mapping of ground state spaces. In fact, for any family of operators with bounded derivative and a uniformly isolated spectral patch with associated spectral projection $P(\lambda)$, we have $P'(\lambda) = \mathrm{i}[D(\lambda), P(\lambda)]$, where $D(\lambda)$ is defined by the integral in (2).

Now, what is the significance of this theorem for the question of gapped ground-state phases of quantum spin systems? First of all, it clarifies and relates the two definitions of a phase, namely the one involving a gapped path of Hamiltonians and the one using a local unitary map: Paraphrasing, the ground-state spaces of a smooth family of gapped Hamiltonians are related by a quasi-local automorphism of the algebra of observables. Furthermore, the locality of the map is made explicit and allows for its extension to infinite systems, a non-trivial but crucial step for the study of phase transitions. Also, including symmetries and symmetry breaking in the discussion is immediate.

Last but not least, the notion of local automorphic equivalence offers a natural way to describe topological phases. Topological order may have received various definitions, but the common feature is – paradoxically – that topologically ordered systems are locally disordered: No local order parameter can distinguish between the various ground states. Topological order is revealed by considering the same interaction on different lattices. Hence the following proposition. First fix a set of lattices $\mathcal{L}$. In one dimension, that would be $\mathbb{Z}$ and the two possible half-infinite chains with one boundary; in two dimensions, e.g., $\mathbb{Z}^2$, various half-planes, and closed surface of different genera. Consider two interactions Φ_0 and Φ_1 with corresponding Hamiltonians $H_{0,\Gamma}$ and $H_{1,\Gamma}$ that are gapped for all $\Gamma\in\mathcal{L}$, with ground-state spaces $\mathcal{S}_{0,\Gamma}$ and $\mathcal{S}_{1,\Gamma}$. Then, the two models are in the same ground state phase if for all $\Gamma\in\mathcal{L}$ there exists a quasi-local automorphism α^{Γ} mapping $\mathcal{S}_{0,\Gamma}$

to $\mathcal{S}_{1,\Gamma}$. Hence, not only does the structure of the two ground-state spaces in the bulk have to be similar – for example of equal dimension – but they must also depend on the underlying geometry and topology in the same way.

Let us briefly discuss the one-dimensional case. In [6, 12] it is concluded that all gapped, translation invariant, one-dimensional quantum spin systems without symmetry breaking belong to the same phase, and that they are in particular equivalent to a product state: There is no trace of topological order in one dimension. This may be true for the bulk phase, but fails to take into account the behaviour at the boundary. In [13] we introduced a family of models that illustrate the role of edge states. These Hamiltonians all have a unique gapped ground state in the limit of an infinite chain, which is a product state indeed. However, when they are defined on the right half-infinite chain with a left boundary, there are 2^{n_L} ground states which can be interpreted as a bulk product vacuum upon which n_L distinguishable particles can be added. At most one of each can bind to the edge without raising the energy, but a second one, in the bulk, represents an excitation above the spectral gap. A similar situation happens on the half-infinite chain with a right boundary, and n_R particles. The exact 'masses' of these particles are irrelevant: two such models are in the same gapped phase if and only if they have the same numbers n_L and n_R. They are very simple representatives of the equivalence classes defining ground-state phases and in fact, it is possible to prove that the famous AKLT model [14] belongs to the class indexed by $n_L = n_R = 1$.

Before concluding, it should be noted that this leaves the question of the actual nature of topological order unanswered. According to the physics literature, it is a manifestation of long range entanglement as detected by the scaling behaviour of the entanglement entropy,

$$S(\rho_X) := -\mathrm{Tr}\left(\rho_X \ln \rho_X\right),$$

where ρ_X is the density matrix describing the restriction of the bulk state to the finite set $X \subset \Gamma$. The area-law conjecture, proved only in one dimension [15], states that the entropy scales as the area of X, $S(\rho_X) \leq C|\partial X|$, for general gapped quantum spin systems with finite-range interactions. Topological order is expected to manifest itself through universal subleading terms to that law, e.g., a constant in two dimensions, see [16] and references therein. A further promising generalization is the concept of entanglement spectrum [17].

In this short note I have concentrated on the very definition of (gapped) ground-state phases, and on the closely related problem of classifying them. Far reaching proposals including symmetries already exist in the literature, e.g. [18] for one-dimensional systems. The fundamental relation of topological order with non-local entanglement requires a rigorous analysis. The mathematical study of quantum phases and of the transitions between them is still at embryonic stages, and many physically relevant problems remain open.

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News from the IAMP Executive Committee

New individual members

IAMP welcomes the following new members

1. Dr. Andrew Bruce, International College, University of Brighton, United Kingdom
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Recent conference announcements

Stochastic and Analytic Methods in Mathematical Physics
September 2 – 9, 2012, Yerevan, Armenia
http://www.mathphys.org/Armenia2012
This conference is partly funded by the IAMP.

Mathematical Aspects of Quantum Field Theory and Quantum Statistical Mechanics
July 30 – August 1, 2012, DESY, Hamburg, Germany
http://www.qq12.org/
This conference is partly funded by the IAMP.

Spectral Theory and Differential Equations
International Conference dedicated to the 90th birthday of Vladimir A. Marchenko
August 20 – 24, 2012, Kharkov, Ukraine
http://www.ilt.kharkov.ua/marchenko2012

Complex patterns in wave functions – drums, graphs, and disorder
September 5 – 7. 2012
http://royalsociety.org/events/Complex-patterns-in-wave-functions/

Asymptotic analysis and spectral theory on non-compact structures
September 10 – 12, 2012, Johannes-Gutenberg-Universität, Mainz, Germany
http://aspect12.blogspot.com

A symposium to the memory of Hans-Juergen Borchers
October 22, 2012, Universität Göttingen, Germany.
http://www.lqp.uni-goettingen.de/events/borchers.info.html
There will be three talks by Christoph Kopper, Gandalf Lechner, and Jakob Yngvason

Adventures in Mathematical Physics (25 èmes Entretiens Jacques Cartier)
November 19 – 21, 2012, Université Lyon 1
http://math.univ-lyon1.fr/~roger/

Open positions

Postdoctoral positions in Helsinki

The Mathematical physics group at the University of Helsinki (http://mathstat.helsinki.fi/mathphys/) is looking for several *postdoctoral researchers* in the fields of nonequilibrium statistical mechanics, transport equations, disordered systems and random geometry.

The starting date for the positions is negotiable between fall 2012 and spring 2013. The positions are funded through a European Research Council (ERC) Advanced Grant (http://wiki.helsinki.fi/display/mathphys/ERC-MathPhys) and through Academy of Finland projects of Antti Kupiainen and Jani Lukkarinen.

The Mathematical physics group in Helsinki consists currently of four senior members, five postdocs and six PhD students. The group is nationally and internationally well connected, and it is part of the Finnish Centre of Excellence in Analysis and Dynamics Research (http://wiki.helsinki.fi/display/huippu/Home).

Please send your application and any possible queries about the above positions to Jani Lukkarinen (e-mail: jani.lukkarinen@helsinki.fi). The application should include the following information, preferably as one or more PDF files attached to the application e-mail:

1. Standard curriculum vitae
2. List of publications of the applicant

3. Description of previous research (max. 1 page)

4. Current research interests (one paragraph)

5. Three letters of recommendation, including contact details of the person providing the reference

The deadline for the applications is August 27, 2012.

Faculty position at KU Leuven

KU Leuven invites applications for a mathematical physicist in the Department of Physics and Astronomy, working in the area of mesoscopic phenomena and condensed matter physics, to start in October 2013. Please contact Christian.Maes@fys.kuleuven.be for more information. The electronic application can start from http://www.kuleuven.be/personeel/jobsite/vacatures/science.html#2013_2014 under number 22/2012 where the official announcement and further information are posted. The candidates have an excellent track record in the research of complex systems as from problems in statistical mechanics and have interest in collaborating with other theoretical and/or experimental scientists in their field of research. The candidates are also dedicated to education in mathematics or physics at university level.

The deadline for application is September 30, 2012.

The job announcement page of the IAMP is

http://www.iamp.org/page.php?page=page_positions

Manfred Salmhofer (IAMP Secretary)