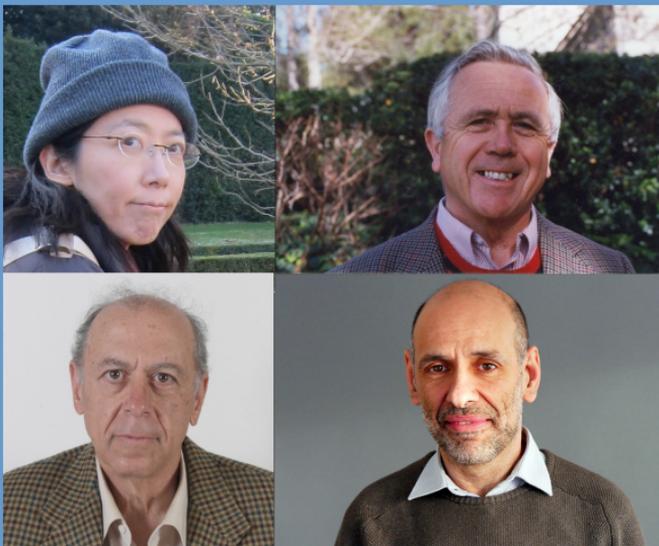


# IAMP News Bulletin October 2021



International Association of Mathematical Physics

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*Cover picture:* The 2021 Poincaré Laureates



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News Bulletin (International Association of Mathematical Physics)

# International Association of Mathematical Physics

## News Bulletin, October 2021

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# The XX International Congress on Mathematical Physics

by BENJAMIN SCHLEIN (ICMP co-convenor, Zurich)

The XX International Congress on Mathematical Physics was held in Geneva, from August 2 to August 7, 2021. As usual, the Congress was preceded by a Young Researcher Symposium (YRS), from July 29 to July 31. Additionally, two satellite meetings were held in Switzerland before the Congress, a summer school on “Current topics in mathematical physics” in Zurich, from July 19 to July 23, and a workshop on “Topological phases of matter” in Leysin from July 25 to July 28.

Because of the COVID-19 pandemic, YRS and ICMP were held in hybrid format, with some participants and speakers in Geneva and some connected online from around the world. The organization of the hybrid conference was challenging but it allowed us to reach more participants, including several researchers who could not travel to Geneva, due to current travel restrictions.

The YRS was held at the University of Geneva; there were 209 registered on-site participants and 164 registered online participants. The three main speakers were H. Duminil-Copin, M. Hairer and M. Mariño. In the afternoons, there were 48 contributed talks, divided in the four sessions *Quantum Mechanics*, *Quantum Many Body and Quantum Information* (organized by M. Porta and R. Renner), *Statistical Mechanics and Random Structures* (organized by A. Knowles and V. Tasion), *Quantum Field Theory, Integrability and Strings* (organized by N. Beisert and J. Sonner), *Partial Differential Equations, General Relativity and Dynamical Systems* (organized by C. Saffirio and P. Hintz). Additionally, the eight session organisers gave basic notion seminars to introduce participants to their fields.

The ICMP was held at the International Conference Centre in Geneva. There were 367 registered on-site participants as well as 273 registered online participants. We had 16 plenary talks, given by V. Baladi, T. Bodineau, W. De Roeck, H. Duminil-Copin, M. Gualtieri, P. Hintz, A. Kupiainen, J. Maldacena, N. Nekrasov, Y. Ogata, N. Reshetikhin, J.P. Solovej, J. Szeftel, T. Vidick, F. Xu and H.-T. Yau. Additionally, there were 70 invited and 81 contributed talks, divided in the thematic sessions *Dynamical Systems* (organized by D. Damanik, C. Liverani), *Equi-*

*librium Statistical Mechanics* (organized by A. Giuliani, Y. Velenik), *General Relativity* (organized by G. Holzhegel, G. Huisken), *Integrable Systems* (organized by G. Felder, A. Pelayo), *Many-body Quantum Systems and Condensed Matter Physics* (organized by R. Seiringer and S. Teufel), *Nonequilibrium Statistical Mechanics* (organized by T. Funaki, P. Gonçalves), *Partial Differential Equations* (organized by L. Székelyhidi, V. Vicol), *Probability and Random Structures* (organized by G. Kozma, H. Weber), *Quantum Field Theory* (organized by A. Cattaneo, J. Penedones), *Quantum Information* (organized by N. Datta, D. Pérez-García), *Quantum Mechanics and Spectral Theory* (organized by S. Jitomirskaya, G. Panati), *String Theory and Quantum Gravity* (organized by S. L. Shatashvili, E. Verlinde).

During the opening ceremony, the President of the IAMP, B. Nachtergaele, announced the winners of the 2021 Henri Poincaré Prize, sponsored by the Daniel Lagolnitzer Foundation: R. Baxter (laudatio by V. Bazhanov), D. Christodoulou (laudatio by I. Rodnianski), Y. Ogata (laudatio by H. Tasaki) and J.P. Solovej (laudatio by S. Fournais). The winner of the IAMP Early Career Award, sponsored by Springer, was then presented by S. Serfaty, the Vice President of the IAMP, to A. Aggarwal (laudatio by F. Toninelli). Finally, A. Joye, as Vice Chair of the C18 Commission of the IUPAP, announced the winners of the 2021 IUPAP Young Scientist Prize in Mathematical Physics: S. Aretakis, C. Saffirio, V. Tassion.

On Monday afternoon there was a public lecture by Nobel laureate M. Mayor. On Wednesday afternoon, recipients of the “Annales Henri Poincaré Journal Prize” (represented by M. Woods, D. Sutter, J. Bausch) and of the “Journal of Mathematical Physics Young Researcher Award” (A. Lucas) gave talks in parallel sessions. On Friday at lunch time, there was a human-rights session, devoted to the issue of gender balance and equal opportunities in mathematics and physics.

## **ICMP 2024**

The next International Congress on Mathematical Physics in 2024 will take place in Strasbourg, France. It will be organized jointly by the Mathematics and Physics Institutes of the University of Strasbourg. The chair of the organizing committee is Nalini Anantharaman, assisted by co-chairs Semyon Klevtsov, Clément Tauber, and Martin Vogel. The Congress will take place at the main conference center of the city (Palais des Congrès), near the European Parliament and well connected with the historical city center. It is planned to take place from July 1 to July 6, 2024. The Young Researchers' Symposium will take place right before the ICMP, from June 28 through June 29, 2024, at the University of Strasbourg.

Save the date!

## Laudatio for Amol Aggarwal, Winner of The IAMP Early Career Award 2021



It is a pleasure and a honor to introduce to you Amol Aggarwal, who receives today the 2021 IAMP Early Career Award. This prize was attributed to him

“for fundamental contributions to the asymptotic analysis of two-dimensional lattice models, including proving the universality of local correlations for dimer models, characterizing Gibbs measures and their current fluctuations for the stochastic six vertex model, and providing a rigorous framework for the tangent method of finding boundaries of frozen regions in planar ice models.”

Amol Aggarwal’s (born 1993) CV in a few lines:

- Undergraduate studies at MIT
- In 2016, AMS Morgan prize for Outstanding Research by an Undergraduate Student. Even before starting his PhD. thesis, Amol already had 4 articles published in leading combinatorics journals!
- Amol received his PhD in 2020 from Harvard University, where he was advised by Alexei Borodin (MIT).
- Just after that, in July, 2020, he was appointed as a Clay Research Fellow for a term of five years and he is presently Assistant Professor at Columbia.

Amol's research lies largely in the area between probability theory and combinatorics known as "integrable probability," with strong connections with mathematical physics, in particular integrable systems and out-of-equilibrium statistical mechanics (interacting particle systems and stochastic growth processes). Let me say a few words about just a few among his (numerous) ground-breaking achievements.

1. **Universality for lozenge tiling local statistics (2019).** This is perhaps my favorite among Amol's results, and it is very easy to formulate. The story starts with dimer models, or random tilings, in 2 dimensions. As was observed by Cohn, Larsen and Propp in '98, typical random tilings of large planar domains show phase separation between "frozen" and "rough" regions, separated by so-called frozen curves, or phase-separation lines. Cohn-Kenyon-Propp in 2001 formulated the natural conjecture that the local behavior of such tiling models is always governed by translation invariant Gibbs measures. Over the years, partial progress was achieved via heavy analytic tools (orthogonal polynomials, asymptotics of Kasteleyn matrix) but only for very special domains (polygons of a certain type). In a real tour de force, Amol proves the conjecture for dimers on the hexagonal lattice in arbitrary domains. The proof of this claim is extremely multifaceted. It relies on deep results in the "exactly solvable" realm (integrable probability), borrows some general intuition from Erdős-Schlein-Yau-et al.'s approach to universality in random matrices and develops new regularity results in elliptic PDEs. From a personal perspective, I was also thrilled to see that methods I had previously developed with B. Laslier to study mixing properties of Glauber dynamics of tiling models played a role in this fascinating story!
2. **Arctic Boundaries of the Ice Model on Three-Bundle Domains (2018).** For the dimer model mentioned above, phase separation can be exhibited explicitly: frozen curves/limit shapes can be computed exactly because the model is determinantal and the surface tension is explicitly computed. Things become much more challenging as soon as models are not determinantal and maybe the most well-known example is the so-called "six vertex model." For a square domain with domain wall boundary conditions, phase

separation for this model at the “ice point” was predicted long ago by numerical simulations. A few years ago, physicists Colomo and Sportiello derived an equation for the frozen curve (which they predicted to be the union of explicit algebraic curves), using a non-rigorous approach they called the “tangent method.” Amol was the first person to make the tangent method rigorous, for the square ice model in a family of domains that included the square with domain-wall boundary. Amol’s idea was to apply a formalism of Gibbs line ensembles, developed previously in the context of random matrices and random growth models, to prove certain stochastic monotonicity for lattice paths arising in square ice.

3. Another amazing work is **Current Fluctuations of the Stationary ASEP and Six-Vertex Model (2016)**. The Asymmetric Simple Exclusion Process (ASEP) in one dimension is a prototypical interacting particle system that has been studied in the last decades in hundreds of papers, both by physicists and by mathematicians. One of the key questions is its space-time fluctuation behavior at stationarity. In 1985, van Beijern, Kutner and Spohn predicted anomalous behavior of the stationary ASEP along its characteristic lines. They predicted the height function’s fluctuation exponent, and a later work of Ferrari-Spohn in 2006 on a simpler model (known as TASEP) in the same KPZ universality class predicted the asymptotic (large-time) distribution. It is that distribution that Amol proved for the ASEP. He also proved a similar result for certain translation invariant Gibbs measures of the six vertex model - again one of the very few rigorous results known about fluctuations in this fundamental lattice model of Statistical Physics.
4. I’ll also mention the work **Large Genus Asymptotics for Volumes of Strata of Abelian Differentials (2018)**. This is very far from my area of expertise, but what I gathered from experts is that Amol’s proof of a conjecture of Eskin and Zorich, describing large genus asymptotics of the Masur-Veech volumes and the Siegel-Veech constants, is a major advance in geometric topology/dynamical systems.

Summarizing: just 5 years after starting his PhD thesis, Amol Aggarwal already emerges as one of the most promising mathematical physicists of his generation. He has single-handedly (all works mentioned above are authored by him alone!) solved several important conjectures in mathematical physics, integrable probability, combinatorics, and well beyond. I am simply amazed.

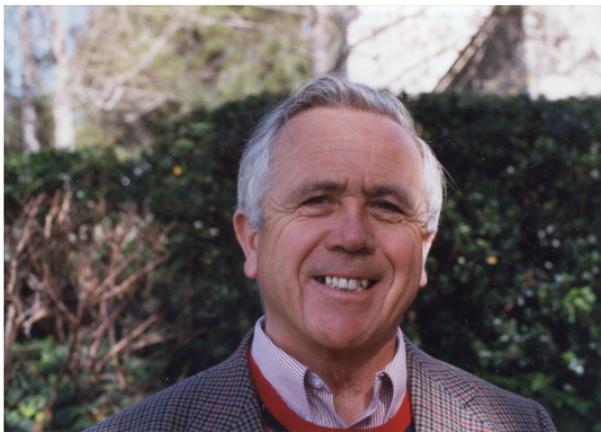
Congratulations Amol, and best wishes for a brilliant mathematical career!

Fabio Toninelli (TU Wien)

ICMP 2021 (Geneva)

2 August 2021

## Laudatio for Rodney J. Baxter, Laureate of the Henri Poincaré Prize 2021



Rodney J. Baxter

Photo credit: George M. Bergman 1999, under the Creative Commons Attribution-Share Alike 4.0 International license.

It is an honor and tremendous pleasure for me to say a few words about Rodney Baxter's works on this occasion for his richly deserved Henri Poincaré Prize in mathematical physics.

Rodney Baxter's name is firmly associated with the most elegant mathematical discoveries for at least two generations of theoretical physicists and mathematicians. Rodney graduated from Trinity College Cambridge in 1961 and received his PhD from the Australian National University (ANU) in 1964. Then after positions at the ANU and MIT he got a tenured position at ANU. Interestingly, Rodney in his recent autobiography calls himself an 'accidental academic'. Let me read the quote: "For the first 24 years of my life I had no intention of becoming an academic. Rather I expected to earn my living as an employee of some large company, such as the Iraq Petroleum Company that I joined in 1964 as a reservoir engineer. However, things panned out differently and I'm very happy

that they did. I've made a career as a mathematical physicist, working on simple models of statistical mechanical systems, asking questions akin to 'why does water boil', or 'why does it freeze' ... . I've been able to make some contributions to the subject." We are all very happy that Rodney did become an academic! I first met Rodney (in person) in 1989, more than 30 years ago, during the 'Special year in Mathematical Physics' program organised by Neil Trudinger's 'Centre for Mathematical Analysis' at the Australian National University. This was a start of my collaboration with Rodney in the area of lattice models, which has been continuing for many years. Of course, previously, during the 80's, I and many of my colleagues in Russia had studied Rodney Baxter's works. A typical state after such studies is a feeling of absolute and complete admiration of the beauty and sophistication of his mathematical results. The effect is so strong that it is not unusual that at international conferences people completely unknown to me come and ask 'You are working at the ANU, have you seen Professor Baxter?', clearly indicating that that would be a notable event in their life. Baxter's work has involved solving highly non-trivial mathematical problems in the most brilliant way. In 1971 Baxter solved the eight-vertex model on the two-dimensional lattice by inventing methods of such power and generality that the course of research in statistical mechanics was permanently altered. In 2000 we had an international conference in Canberra, which was entitled 'The Baxter Revolution in Mathematical Physics' to emphasize the broad impact of Baxter's pioneering work in many branches of physics and mathematics. Since then the scope of new application of Baxter's work is only rapidly increasing. To date it has completely revolutionized many areas of modern mathematics, including algebra, topology, geometry and mathematical analysis. In physics, there are spectacular applications in statistical mechanics and condensed matter physics (such as quantum gases), in quantum field theory, and most recently in string theory and high energy physics. This revolution originates in Baxter's brilliant inventions of what are now called the Yang-Baxter equation, the corner transfer matrix, the commuting transfer matrices and functional relations for their eigenvalues. In his pioneering paper on the hard-hexagon model Baxter has discovered the connections with the Rogers-Ramanujan identities, which besides the exact results for expectation values has led to a dual boson-fermion description for lattice models.

Baxter's work has also led to

1. The invention of quantum groups by Drinfeld and Jimbo, who have been honoured for their work by the Fields Medal for Drinfeld in 1990 and the Wigner Medal for Jimbo in 2010.
2. The discovery of a knot invariant by Jones, who was honoured by the Fields Medal in 1990.
3. The development by Sklyanin, Takhtajan and Faddeev of the powerful Quantum Inverse Scattering Methods for solving models of statistical mechanics and quantum field theory.
4. The connection of Gauge/String theory and 2D integrable system (Maldacena, Minahan-Zarembo, Costello-Witten-Yamazaki and many others).

Baxter's work has inspired many other developments by researchers around the world, including Andrews, Au-Yang, McCoy, Perk and Tracy in the USA; Belavin, Fateev, Zamolodchikov, Tselik, Wiegmann, Korepin, Kirillov, Smirnov and Reshetikhin in Russia; Date, Miwa and Jimbo in Japan; Maillet, Pasquier and Saleur in France; Pearce and Forrester in Australia; Mussardo in Italy among others of that generation. Isaac Newton said "If I have seen further, it is by standing on the shoulders of giants." He was referring to Copernicus, Galileo and Kepler. There is no doubt that Prof. Baxter is a giant who has brought the torch of mathematical physics into the 21-st century. Rephrasing Newton, I would say, "We are able to see further, because of the outstanding work of Professor Baxter."

Please join me to congratulate Professor Baxter on the award of the Henri Poincaré Prize in Mathematical Physics.

Vladimir Bazhanov (Australian National University)

ICMP 2021 (Geneva)

2 August 2021

## Laudatio for Demetri Christodoulou, Laureate of the Henri Poincaré Prize 2021



It is a great honor for me to introduce Demetri Christodoulou, a laureate of the 2021 Poincaré Prize, awarded

“for pathbreaking contributions to mathematical understanding of the Einstein equations, including fundamental results on black-hole formation and the discovery of a nonlinear memory effect in the theory of gravitational radiation, and for introducing a powerful geometric point of view for the problem of shock formation for compressible fluids.”

Demetri Christodoulou is a singular mathematician whose work has had a profound impact on the fields of general relativity, hyperbolic partial differential equations, and fluid dynamics.

Even though I described him as a mathematician, Demetri started his journey in the Physics Department. He received his PhD in physics at Princeton at the age of 19 under the direction of John Wheeler. His thesis showed existence of an irreducible mass of a black hole and played a key role in the development of black-hole thermodynamics. It has been reported that Demetri’s original thesis problem, which he was unable to solve at the time, was a bit more challenging and its solution required a 40 year detour.

This detour took Demetri first to mathematics and then to the point of view, his point of view, which sees general relativity as the arena of partial differential equations and geometry.

One of the early successes of this philosophy was his work throughout the 80’s and the early 90’s on the spherically symmetric Einstein-scalar field model.

Here, Demetri proved a slew of remarkable results, giving an almost complete description of large data dynamics and establishing a very satisfying dichotomy: for generic initial data, gravitational and scalar waves either disperse and the spacetime converges to the flat Minkowski space, or a black hole with the exterior which converges to Schwarzschild forms. The generic caveat is crucial, for he also found exotic solutions containing so called naked singularities which, mercifully, turned out to be unstable. This circle of ideas was also an inspiration behind the discovery and the study of the so called critical phenomena in numerical relativity, in which one probes the universality of behavior on the boundary in transition from 'regular' to 'singular' regime.

A truly watershed moment was his proof, in 1993, jointly with Klainerman, of stability of Minkowski space. This was not just a fundamental result but it gave birth to the synthesis of Lorentzian geometry and hyperbolic PDE's. Its lasting impact is still felt today. This work, among other things, established the laws of gravitational radiation and led Demetri to the discovery of the nonlinear gravitational memory effect – a measurable phenomenon in which a wave train causes a permanent relative displacements of test masses. This is now known as the Christodoulou memory effect.

In 2009 Demetri returned to his original thesis problem, his *pièce de résistance* – the problem of black-hole formation. Here, the story begins with the Penrose's incompleteness theorem from 1965 which guarantees geodesic incompleteness of any spacetime satisfying a null-energy condition, possessing a non-compact Cauchy hypersurface, and also, crucially, a 2-d trapped surface. For all the incredible importance and influence of this result, it fails to address if this type of geodesic incompleteness (colloquially referred to as singularity) can develop in evolution. Yet, this result lies at the foundation of all of our current understanding of the predicted theory of gravitational collapse and, in particular, its mechanism of black-hole formation. With trapped surfaces being a characteristic feature of black holes, this boils down to the question of evolutionary formation of trapped surfaces.

The problem lay dormant for 40 years until it was solved in 2009 by Christodoulou for the problem without matter (note that matter makes black-hole formation easier) and without symmetry. It was a remarkable tour de force. The basis of it was an astonishing insight identifying a whole class of initial data

which, on one hand are sufficiently large, since this phenomenon requires a strong gravitational field regime, while, on the other, still allows one to control the dynamics all the way to the eventual formation of a trapped surface. The appropriate data turned out to correspond to sharp directional bursts of curvature. It takes a painstaking analysis and a deep understanding of the algebraic structure of the Einstein equations to construct the necessary large enough portion of spacetime. It turned out to be an even more powerful idea when viewed in a more general PDE context

After that or, actually, already slightly before, Demetri turned to an even older subject – dynamics of 3-dimensional compressible fluids governed by the Euler equations. A fundamental feature of these equations is that its solutions develop shock singularities even when generated by smooth and, even more remarkably, small data. This is a classical phenomena much studied in the literature going back to Riemann and Stokes. The 20th century development of the subject was focused on creating a framework accommodating global solutions containing shocks. This became tractable only for the 1-dimensional equations and was based on analysis associated with the functions of bounded variation with the pioneering work by Glimm, Lax, Oleynik, Kruzkov and others. In higher dimensions, including the physical 3-dimensional case, conceptual and analytical difficulties were very high and progress very slow.

In 2007 Christodoulou published a monograph containing a proof of shock formation for the 3-dimensional relativistic Euler equations. There, he identified a precise class of initial data for which he constructed a maximal Cauchy development with a singular boundary and gave a complete geometric description of both the boundary and of the singular behavior of solutions. The result was a consequence of a completely novel geometric point of view on the problem. The corresponding results for the Newtonian case were given a self-contained adaptation by Christodoulou-Miao in 2012.

It turns out that part of the constructed Cauchy development becomes unphysical and has to be replaced by a physical solution containing a shock which emanates from the first singular surface. This is the problem of shock development, which, once again, was solved by Demetri in his monumental work in 2017, albeit in the restricted irrotational context.

These works signify the dawn of new era in the study of higher dimen-

sional compressible fluid dynamics, or more general classes of equations admitting shocks, whose ultimate horizon is perhaps to supersede the global theory of weak shock admitting solutions, developed in the 1-dimensional case.

Let me conclude by saying that Christodoulou's impact on general relativity, fluid dynamics and PDE's can not be overstated. It will be absorbed, internalized, and felt for many years to come. Demetri's career took him from Princeton, to Caltech, to Munich, to Syracuse, to New York, back to Princeton and then to Zurich. It was said about Feynman that the productivity of his colleagues was inversely proportionate to the distance from their offices to his. I think a bigger compliment might be the opposite statement. For Demetri, this can be attested by generations of mathematicians who have read and tried to digest Demetri's papers and books. I, personally, was fortunate enough to meet Demetri when I came to Princeton and then be influenced by his work over the years, so I was lucky to benefit from both.

I would like to offer Demetri again my warmest congratulations on his outstanding achievement in winning the 2021 Henri Poincaré Prize.

Igor Rodnianski (Princeton University)  
ICMP 2021 (Geneva)  
2 August 2021

## Laudatio for Yoshiko Ogata, Laureate of the Henri Poincaré Prize 2021



It is my great pleasure and honor to briefly discuss Yoshiko Ogata's research accomplishments on this occasion of her Poincaré prize. The citation of the prize reads:

“For groundbreaking work on the mathematical theory of quantum spin systems, ranging from the formulation of Onsager reciprocity relations to innovative contributions to the theory of matrix product states and of symmetry-protected topological phases of infinite quantum spin chains.”

Yoshiko received her PhD from the University of Tokyo, where she was a physics major. She was a postdoc at University of Marseille and UC Davis, and then joined Kyushu University as a faculty member. In 2009, she moved to the department of mathematics of the University of Tokyo, where she is now a full professor.

Yoshiko has been working on problems in quantum many-body systems by using the operator algebraic formulation. She has solved, and is solving, a variety of the most difficult problems in physics that involve infinite degrees of freedom by developing precise, sometimes deep, mathematical tools. Let me discuss some examples.

With Vojkan Jaksic and Claude-Alain Pillet, Yoshiko studied the general problem of non-equilibrium steady states, and justified the linear response theory, especially the Onsager reciprocal relations. The Onsager relations are still among the most essential results in non-equilibrium physics, and I would say that this is a fundamental contribution to a traditional problem in physics.

In the field of quantum spin systems, Yoshiko has made several fundamental contributions on problems that are fashionable even in the physics community.

To explain her contributions, I would like to recall Duncan Haldane's famous discovery, which brought him the 2016 Nobel prize in physics, about low-energy properties of the antiferromagnetic Heisenberg chain, whose Hamiltonian is

$$H = \sum_{j \in \mathbb{Z}} \mathbf{S}_j \cdot \mathbf{S}_{j+1},$$

where  $(\mathbf{S}_j)^2 = S(S+1)$  with the spin quantum number  $S = 1/2, 1/3/2, \dots$ . Haldane conjectured that when, and only when,  $S$  is an integer this model has a unique gapped ground state, namely, a unique ground state accompanied by a nonzero energy gap immediately above the ground state energy.

This conjecture has not yet been solved, but it was proved that a similar Hamiltonian

$$H_1 = \sum_{j \in \mathbb{Z}} \mathbf{S}_j \cdot \mathbf{S}_{j+1} + \frac{1}{3} (\mathbf{S}_j \cdot \mathbf{S}_{j+1})^2,$$

with  $S = 1$  has a unique gapped ground state which is believed to be qualitatively similar to the ground state of the original Heisenberg chain. But it is also easy to write down a model that has a unique gapped ground state for a trivial reason. For example the  $S = 1$  chain with the Hamiltonian

$$H_0 = \sum_{j \in \mathbb{Z}} (S_j^z)^2$$

clearly has a unique gapped ground state, which is the tensor product of the eigenstate  $|0\rangle_j$  of  $S_j^z$ . It is then natural to ask whether these two ground states are "smoothly connected."

To be precise we say that the models with  $H_0$  and  $H_1$  are smoothly connected if there exists a family of Hamiltonians  $H_s$ , where  $s \in [0, 1]$ , with a unique gapped ground state that smoothly interpolates between  $H_0$  and  $H_1$ . It was conjectured by Chen, Gu, and Wen in 2011 that  $H_0$  and  $H_1$  are indeed smoothly connected if one is allowed to use any short ranged Hamiltonian  $H_s$  to interpolate between them. This fact is now known rigorously. It follows, e.g., from Yoshiko's extensive classification theory of matrix product states published in 2016 and 2017 as a trilogy in *Communications in Mathematical Physics*.

But this is not the end of the story. Recall that both  $H_0$  and  $H_1$  have time-reversal symmetry, i.e., invariance under the transformation  $\mathbf{S}_j \rightarrow -\mathbf{S}_j$  for all  $j \in \mathbb{Z}$ . It was conjectured by Gu and Wen in 2009 that if we require interpolating Hamiltonians  $H_s$  to also possess time-reversal symmetry then  $H_0$  and  $H_1$  are never connected smoothly. In this case the models with  $H_0$  and  $H_1$  are said to belong to different symmetry protected topological phases. This is indeed the fact that Yoshiko proved in her ground-breaking paper appeared in 2018 and published in *CMP* last year. In this and the following paper published this year in *CMP*, Yoshiko defined indices for a unique gapped ground state of a spin chain with certain symmetry. The indices take value in the second group cohomology  $H^2(G, U(1))$  of the symmetry group, and are proved to provide classifications of symmetry protected topological phases. We should note that such indices were already defined by Pollmann, Turner, Berg, and Oshikawa back in 2010, but only for a limited class of states, namely, injective matrix product states, while Yoshiko's index theories cover an arbitrary unique gapped ground state. In this sense we can say that Yoshiko has completed the theory of symmetry protected topological phases in quantum spin chains. It is simply amazing that fully rigorous and general mathematical theory was developed only nine years after the original heuristic proposal. But this is not yet the end of the story. Yoshiko never stops. She has already completed the theory of symmetry protected topological phases of two-dimensional quantum spin systems, as we can hear from her in the next session!

I cannot help discussing one more work of Yoshiko's which is my favorite (and Yoshiko's favorite too, I hear). Suppose that there are  $n$  sequences of hermitian matrices  $H_i^{(1)}, \dots, H_i^{(n)}$  with  $i \in \mathbb{N}$  which commute with each other asymptotically, i.e.,

$$\lim_{i \uparrow \infty} \|[H_i^{(\alpha)}, H_i^{(\beta)}]\| = 0,$$

for any  $\alpha, \beta = 1, \dots, n$ . We then ask whether the sequences of matrices can be approximated by sequences of mutually commuting hermitian matrices, more precisely, whether there exist  $n$  sequences of hermitian matrices  $Y_i^{(1)}, \dots, Y_i^{(n)}$  such that

$$[Y_i^{(\alpha)}, Y_i^{(\beta)}] = 0,$$

for all  $\alpha, \beta = 1, \dots, n$  and  $i \in \mathbb{N}$ , and

$$\lim_{i \uparrow \infty} \|H_i^{(\alpha)} - Y_i^{(\alpha)}\| = 0,$$

for all  $\alpha = 1, \dots, n$ .

This is indeed a famous classical problem, and it is well known that such commuting approximations do not exist in general if  $n \geq 3$ . In her paper in 2013 published in *Journal of Functional Analysis*, Yoshiko proved that *commuting approximations always exist* if the original non-commuting matrices are taken as the densities of extensive quantities of a quantum spin system. This result is natural for physicists since thermodynamics is a classical theory where all quantities commute, and these densities are precisely thermodynamic objects.

To prove the theorem, Yoshiko studies projections onto the spaces where these extensive quantities take almost constant values, and then estimates the ranks of the projections by means of the entropy functions. This estimate, with an operator algebraic technique, enables her to construct the desired set of commuting matrices. I would say that the proof is an example of ideal combination of ideas from statistical mechanics and techniques from operator algebra.

For me, it was a truly exciting experience to witness rapid progress in mathematical physics made by Yoshiko. But I am sure that this is far from the end. I am looking forward to many more new beautiful insights from Yoshiko.

I would like to end by congratulating Yoshiko on this occasion of her winning the Henri Poincaré Prize.

緒方さん、おめでとうございます。

Hal Tasaki (Gakushuin University)

ICMP 2021 (Geneva)

2 August 2021

## Laudatio for Jan Philip Solovej, Laureate of the Henri Poincaré Prize 2021



It is an honor and a privilege to present to you Jan Philip Solovej, who receives the Henri Poincaré prize 2021

“for outstanding contributions to the analysis of quantum many-body problems ranging from the electronic structure of large atoms to the Lee-Huang-Yang asymptotics of the ground state energy of dilute Bose gases.”

Jan Philip received his Ph.D. from Princeton University in 1989 with Elliott Lieb as advisor and with a thesis entitled “Universality in the Thomas-Fermi-von Weizsäcker model of atoms and molecules.” At that time, the study of large atoms was one of the central questions in mathematical physics. To set the stage, the famous “ $2Z + 1$ ”-bound of Lieb had just appeared in 1984, and the papers by Hughes and by Siedentop and Weikard on the Scott correction must have been written up during Jan Philip’s time as a graduate student— just to mention some of the important developments of the time. It appears clear that Jan Philip got strongly motivated for settling the important question of the maximum possible ionization of an isolated, non-relativistic atom while in Princeton. The ionization conjecture states that this ionization, i.e. the maximum number of electrons that can be bound to the nucleus minus the nuclear charge, is bounded by a universal constant independent of the nuclear charge. It is one the main achievements of Jan Philip to have proved this conjecture in the Hartree-Fock model; first in 1991 in a reduced Hartree-Fock model and later in 2003 in the full Hartree-Fock theory of atoms. The conjecture for the full quantum mechanical many-body problem

remains open, but I am convinced that Jan Philip has not yet given up on proving it!

The subjects of semiclassical analysis, electronic structure, and stability of matter, are strongly intertwined. Jan Philip has made important contributions to them all. Let me here only briefly mention some. One highlight is the influential works, with Lieb and Yngvason, on semi-classical analysis and “Magnetic Thomas-Fermi Theory” in the presence of strong magnetic fields. Of fundamental importance is the beautiful proof with Lieb and Loss of stability of matter with magnetic fields. Together with Erdős he proved strong Lieb-Thirring type inequalities with variable magnetic fields and studied the structure of magnetic zero- modes. Also the proof, with Spitzer and Sørensen, of the Scott correction for a model of an atom including (some) relativistic effects deserves mentioning. Together with Erdős and myself, he proved semi-classical results for large atoms, including the Scott correction, in a model with self-generated magnetic fields.

It is also important here to mention the rigorous derivation in 2012 with Frank, Hainzl, and Seiringer of the Ginzburg-Landau theory of superconductivity from the underlying BCS- theory.

Another highlight in the list of scientific achievements of Jan Philip is the proof of the Lee-Huang-Yang term in the ground state energy of dilute Bose gases. The road to this proof is also long, and shows a determination and a willingness to work hard and develop the necessary tools along the way. An early milestone is the proof, joint with Elliott Lieb, of the formula for the ground state energy of the 1- and 2-component Bose fluid (jellium) in the large density limit in 2001 and 2004. In these works it is used that a simple “completion of the square”-version of Bogoliubov’s diagonalization of quadratic Hamiltonians is a sufficient and robust tool for lower bounds, and localization techniques that do not disturb the condensate are developed. These tools were then sharpened over the years. Let me in passing mention the strongly influential lecture notes from Jan Philip’s course on Many-Body Quantum Mechanics during his semester as Mercator Guest Professor at the LMU, Munich in 2007. Our final proof in 2020 of the Lee-Huang-Yang correction term to the ground state energy of dilute Bose gases in the thermodynamic limit, combines versions of these techniques, with another “completion of the square” argument to take care of the correlation terms between excited pairs in the gas—an argument that has its roots in a paper he

wrote with Gian Michele Graf in 1994—as well as the understanding that the remaining terms not present in Bogoliubov’s calculation effectively cancel each other out.

In 1995, I started as a graduate student with Jan Philip, who had just returned to Denmark from the US, as advisor. My choice of advisor was only based on the suggestion of a trusted professor. This leap of faith has turned out to be one of the best decisions of my life! As the many postdocs and graduate students who have had the luck to work with Jan Philip can testify, he is a wonderful mentor and a generous and insightful scientist, who is never satisfied with the easy, partial solution, but aims for real progress and understanding. This generosity and insight has benefitted a large part of a generation of mathematical physicists in Europe, starting in the 1990’s with European Research Networks with Jan Philip as an important participant and more recently with members and visitors of his group supported by the ERC and later the QMATH-centre funded by the Villum Foundation. I am convinced that I speak on behalf of all these mathematical physicists when I take this occasion to thank him for his wonderful gift of scientific inspiration.

Jan Philip Solovej has solved major, long-standing open problems in the field of mathematical physics and in the process developed the necessary novel tools without ever losing the balance between mathematical beauty and relevance for physics. This has enriched our field. He is a most worthy recipient of the Poincaré prize 2021, and I look forward to many more breakthroughs and inspiring discussions in his office at the H. C. Ørsted Institute in Copenhagen.

Congratulations to Jan Philip and a deeply felt “Thank you!”

Søren Fournais (Aarhus University)

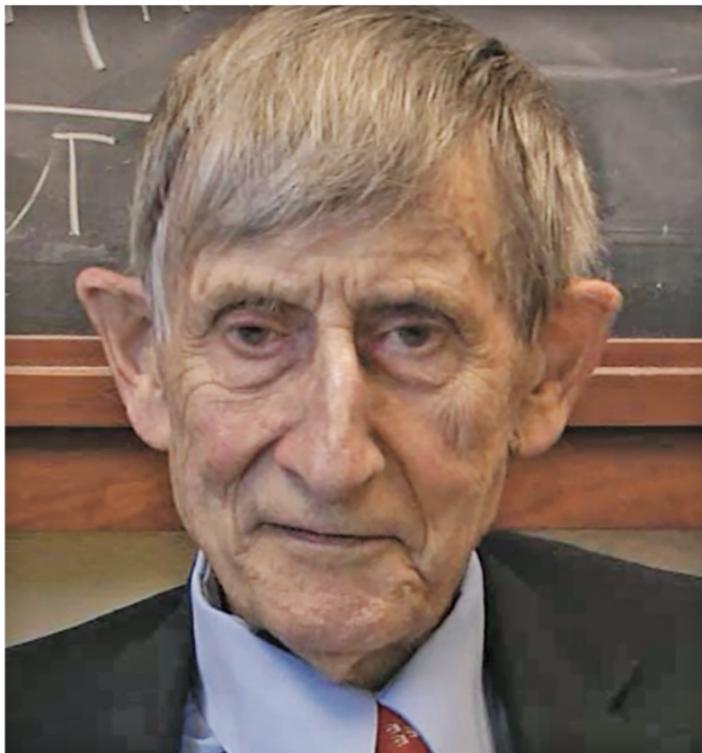
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Photos of Jan Philip Solovej courtesy of : Jim Hoyer, UCPH (front page); Anders Fjeldberg (preceding page)

## In Memoriam: Freeman Dyson (1923–2020)

by GEORGE E. ANDREWS, JÜRIG FRÖHLICH, and ANDREW V. SILLS



Freeman Dyson, March 13, 2011.

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# 1 A Brief Biography

## 1.1 Personal background

Freeman John Dyson was born in Crowthorne, Berkshire, in the United Kingdom, on December 15, 1923. His father was the musician and composer Sir George Dyson; his mother, Mildred Lucy, née Atkey, was a lawyer who later became a social worker. Freeman had an older sister, Alice, who said that, as a boy, he was constantly calculating and was always surrounded by encyclopedias. According to his own testimony, Freeman became interested in mathematics and astronomy around age six.

At the age of twelve, he won the first place in a scholarship examination to Winchester College, where his father was the Director of Music; one of the early manifestations of Freeman's extraordinary talent. Dyson described his education at Winchester as follows: the official curriculum at the College was more or less limited to imparting basic skills in languages and in mathematics; everything else was in the responsibility of the students. In the company of some of his fellow students, he thus tried to absorb whatever he found interesting, wherever he could find it. That included, for example, basic Russian that he needed in order to be able to understand Vinogradov's *Introduction to the Theory of Numbers*.

In 1941, Dyson won a scholarship to Trinity College in Cambridge. He studied physics with Paul Dirac and Sir Arthur Eddington and mathematics with G. H. Hardy, J. E. Littlewood, and Abram Besicovich, the latter apparently having the strongest influence on his early development and scientific style. Dyson's knowledge of Russian came in handy, as Besicovich preferred to converse with Dyson in Russian. Dyson published several excellent papers on problems in number theory, analysis, and algebraic topology. Politically, he considered himself to be a socialist.

During the war, at the age of nineteen, Dyson was assigned to the Royal Air Force's Bomber Command, where he developed methods for calculating the optimal density of bombers in formations to hit German targets. In 1945, he was awarded a BA in mathematics. He became a fellow of Trinity College (1946–1949), where he occupied a room just below the philosopher Ludwig Wittgenstein. After having read Heitler's *Quantum Theory of Radiation* and the *Smyth Report* on the Manhattan Project, Dyson concluded that "Physics would be a major stream of

scientific progress, during the next twenty five years,” and he decided to trade mathematics for theoretical physics.

In 1947, Dyson won a Commonwealth Fund Fellowship and applied to become a graduate student of Hans Bethe at Cornell University. It may be surprising that he decided to leave Cambridge, where Eddington, Kemmer, and Dirac taught, and move to America. Dyson wrote [14, Chapter 1]:

Scientists come in two varieties, hedgehogs and foxes. I borrow this terminology from Isaiah Berlin, who borrowed it from the ancient Greek poet Archilochus. ... [Foxes] know many tricks, hedgehogs only one. Foxes are broad, while hedgehogs are deep. Foxes are interested in everything and move easily from one problem to another. Hedgehogs are only interested in a few problems that they consider fundamental, and stick with the same problems for years or decades. [...] Some periods in the history of science are good times for hedgehogs, while other periods are good times for foxes. The beginning of the twentieth century was good for hedgehogs. [...] [I]n the middle of the century, the foundations were firm and the universe was wide open for foxes to explore.

Obviously, Freeman Dyson was the archetypal “fox,” and the period in physics when he started to do research and scored his first great successes was exactly right for foxes. He was so much a fox that he never got around to getting his doctoral degree. Of course, he did not need to. He was offered a professorship at Cornell University in 1951, to work with Hans Bethe essentially as a replacement for Richard Feynman, who had left for Caltech a year earlier. Dyson wrote about the time he spent at Cornell [8, p. 18]:

I enjoyed teaching students in class-room courses, and I enjoyed talking to them individually about science, but I did not enjoy being responsible for dragging them through the three-year treadmill of Ph.D. thesis research. From this unhappy situation, I was rescued by the offer of a Professorship at the Institute for Advanced Study. I loved Cornell and I loved Hans Bethe, but I hated the Ph.D. system to which my students were tied. The Institute suited my style of work

much better. The life-cycle of the Institute is one year long, with a fresh crowd of visiting members arriving each September. The annual cycle is well matched to my short attention-span. ... With some regret but more relief, I left Cornell in June 1953 and took up my new position in Princeton in September. I was delighted to have a position in which I would never again be responsible for a Ph.D. For the rest of my life I have been fighting ineffectually against the ever-tightening grip of the Ph.D. system on young people wishing to pursue careers in science. I am eternally grateful to Cornell for accepting me as a professor in 1951 without a Ph.D. Unfortunately, the liberality with which Cornell treated me did not extend to my students.

Dyson had married Verena Huber-Dyson in 1950. They had two children (Esther and George), but the marriage ended in divorce in 1958. In 1957, Dyson became a citizen of the United States. In 1958, he married Imme Jung, and together they had four children (Dorothy, Mia, Rebecca, and Emily). Dyson was a family man, and he seemed to greatly enjoy the company of his own six as well as other children.

## 1.2 Some of Dyson's key contributions to theoretical physics

At the time Dyson started his career in physics, quantum field theory was in a messy state. Dirac and Werner Heisenberg thought that, in a revolution similar to the one that gave birth to quantum mechanics, relativistic quantum field theory (RQFT<sup>1</sup>) would eventually be superseded by a mathematically meaningful theory unifying quantum theory with relativity theory. Dyson concluded that what was necessary was to clarify the intricacies of the existing formalism of RQFT and to then use it to do concrete calculations explaining new experimental data. In Cambridge, Dyson had learned some quantum field theory from his friend Nicholas Kemmer and from a book by Gregor Wentzel.

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<sup>1</sup>One referee pointed out that many would prefer the abbreviation "QFT" over "RQFT" since although nonrelativistic quantum field theories do exist, the relativistic variety is the standard, and certainly the kind on which Dirac, Feynman, and Dyson focused.

Dyson wrote [8, p. 12]:

It was my luck that I arrived with this gift from Europe just at the moment when the new precise experiments of Lamb and others [...] required quantum field theory for their correct interpretation. When I used quantum field theory to calculate an experimental number, the Lamb shift [...], Bethe was impressed.

Dyson's principal contribution to quantum field theory was, however, to unify the approaches to quantum electrodynamics (QED), the quantum theory of electrons, positrons, and photons, that had been proposed by Feynman, Julian Schwinger, and Shin'ichirō Tomonaga, a little earlier. This unification work was facilitated by a cross-country road trip, from Ithaca, New York, to Albuquerque, New Mexico, that Dyson took with Feynman in June 1948, joyfully recounted by Dyson in Chapter 6 of [6]. After parting company with Feynman in Albuquerque, Dyson rode a sequence of Greyhound buses to Ann Arbor, Michigan, where he attended a series of lectures by Schwinger, and had many conversations with him over a five-week period. From Ann Arbor, he took a Greyhound bus to California, where he spent ten days. As the summer was winding down, he headed back east to Cornell. Dyson described the crucial insights he gained on this leg of the journey as follows [6, p. 67]:

Feynman's pictures and Schwinger's equations began sorting themselves out in my head with a clarity they never had before. For the first time I was able to put them all together. For an hour or two I arranged and rearranged the pieces. Then I knew that they all fitted. I had no pencil or paper, but everything was so clear I did not need to write it down. Feynman and Schwinger were just looking at the same set of ideas from two different sides. Putting their methods together, you would have a theory of quantum electrodynamics that combined the mathematical precision of Schwinger with the practical flexibility of Feynman. Finally, there would be a straightforward theory of the middle ground. It was my tremendous luck and I was the only person who really had the chance to talk at length to both Schwinger and Feynman and really understand what both of them were doing.

Feynman, Schwinger, and Tomonaga shared the 1965 Nobel Prize for their contributions to quantum electrodynamics. Dyson discovered the right general concepts and methods, in particular a Lorentz-covariant form of perturbation theory for the scattering matrix, involving the systematic use of what are now universally called *Feynman diagrams*, and renormalization theory [5], to convert RQFT into something considerably more compelling than a machine spitting out numbers that miraculously fit experimental data. In developing renormalization theory he understood the importance of “scale separation” in RQFT, an idea that later gave rise to the so-called *renormalization group*, an important paradigm developed primarily by Wilson, who greatly generalized ideas of Stückelberg and Petermann and of Gell-Mann and Low. Dyson generously shared his understanding of quantum field theory with Bethe and Feynman, and explained the latter’s ideas to people preferring Schwinger’s over Feynman’s approach to RQFT, such as J. Robert Oppenheimer, who had become the director of the Institute for Advanced Study.

Since Dyson was a “fox,” it is unimaginable that he would work in the same field for more than a year or so at a time. Indeed, right after his initial successes with QED (and with meson theory), he moved on to work on problems in statistical mechanics and solid-state physics.

Dyson’s ideas and results in statistical mechanics and condensed matter physics, among them his proof, with Andrew Lenard, of “Stability of Matter,” inspired a tremendous amount of important work by younger colleagues, among whom one should mention Elliott H. Lieb, who pursued many of the themes Dyson had set with admirable success.

Dyson has made many further seminal contributions to mathematical physics, applied mathematics, and engineering. Of particular note is his deep work on *random matrix theory* (RMT), originally initiated by Eugene P. Wigner in work on the energy spectra of heavy nuclei. Dyson’s insights have inspired numerous applications of RMT; for example, to number theory (work of Hugh L. Montgomery on the zeros of the Riemann zeta function). More recently, “Dyson’s Brownian motion” has become a very powerful tool to prove new results in RMT.

### 1.3 Some of Dyson's key contributions to mathematics

1.3.1. *The rank and crank of a partition.* In one of his earliest papers [4], published when he was a 20-year-old undergraduate at Cambridge, Dyson wrote:

Professor Littlewood, when he makes use of an algebraic identity, always saves himself the trouble of proving it; he maintains that an identity, if true, can be verified in a few lines by anybody obtuse enough to feel the need of verification. My object in the following pages is to confute this assertion.... The plan of my argument is as follows. After a few preliminaries I state certain properties of partitions which I am unable to prove: these guesses are then transformed into algebraic identities which are also unproved,... finally, I indulge in some even vaguer guesses concerning the existence of identities which I am not only unable to prove but also unable to state. ... Needless to say, I strongly recommend my readers to supply the missing proofs, or, even better, the missing identities.

In this paper, Dyson defines the *rank* of a partition (largest part minus the number of parts) and conjectures that this provides a combinatorial accounting for the congruences of Srinivasa Ramanujan [17]:

$$p(5n + 4) \equiv 0 \pmod{5}, \quad (1.1)$$

$$p(7n + 5) \equiv 0 \pmod{7}, \quad (1.2)$$

where  $p(n)$  denotes the number of partitions of the integer  $n$ . The rank conjectures were proved a decade later by Oliver Atkin and Peter Swinnerton-Dyer [2].

The rank does not, however, explain the third Ramanujan congruence

$$p(11n + 6) \equiv 0 \pmod{11}. \quad (1.3)$$

Dyson therefore goes on to conjecture the existence of a partition statistic "similar to, but more recondite than, the rank of a partition; I shall call this hypothetical coefficient the 'crank' of the partition... I believe the 'crank' is unique among arithmetical functions in having been named before it was discovered." More than four decades later, George Andrews and Frank Garvan found the requested

crank [1]. Ranks and cranks and their generalizations remain an active area of research to this day.

**1.3.2. Identities of Rogers–Ramanujan type.** The Rogers–Ramanujan identities are a pair of  $q$ -series–infinite product identities that were discovered by L. J. Rogers in 1894 [18], yet were ignored by the mathematical community until Ramanujan independently rediscovered them (at first without a proof) and brought them to the attention of G. H. Hardy. They are as follows: for  $|q| < 1$ ,

$$\sum_{n=0}^{\infty} \frac{q^{n^2}}{(1-q)(1-q^2)\cdots(1-q^n)} = \prod_{\substack{j \geq 1 \\ j \equiv \pm 1 \pmod{5}}} \frac{1}{1-q^j} \quad (1.4)$$

and

$$\sum_{n=0}^{\infty} \frac{q^{n(n+1)}}{(1-q)(1-q^2)\cdots(1-q^n)} = \prod_{\substack{j \geq 1 \\ j \equiv \pm 2 \pmod{5}}} \frac{1}{1-q^j}. \quad (1.5)$$

Dyson delighted in identities of the “Rogers–Ramanujan type” and in fact Dyson’s very first published paper [3] was on three modulus 7 analogs of (1.4) and (1.5) that were also originally due to Rogers [18].

During the war, while working at Bomber Command, Dyson corresponded with W. N. Bailey, who was at the time working out a deeper understanding of identities of the Rogers–Ramanujan type. In the course of the correspondence, Dyson contributed many identities to Bailey’s two papers on Rogers–Ramanujan type identities. Dyson reported in [13] that his personal favorite of these identities was

$$\sum_{n=0}^{\infty} q^{n(n+1)} \frac{\prod_{k=1}^n (1+q^k+q^{2k})}{\prod_{h=1}^{2n+1} (1-q^h)} = \prod_{j=1}^{\infty} \frac{1-q^{9j}}{1-q^j}. \quad (1.6)$$

Even after Dyson’s ascendance as one of the world’s leading physicists, he returned from time to time to some research in the theory of partitions and  $q$ -series, such as in [5, 9].

**1.3.3. The Dyson conjecture.** In the first of a series of papers on the statistical theory of energy levels of complex systems [10], Dyson introduced what came

to be known as the *Dyson conjecture*: the constant term in the expansion of the product

$$\prod_{1 \leq i \neq j \leq n} \left( 1 - \frac{z_i}{z_j} \right)^{a_i} \quad (1.7)$$

is the multinomial coefficient

$$\frac{(a_1 + a_2 + \cdots + a_n)!}{a_1! a_2! \cdots a_n!}.$$

In 1975, George Andrews stated a  $q$ -analog of the Dyson conjecture, which was proved ten years later by Doron Zeilberger and David Bressoud [20]. The Dyson conjecture has inspired numerous generalizations and extensions by many authors over the years, including the extension of the conjecture to root systems by Ian Macdonald.

1.3.4. “*Missed opportunities*” lead to *found opportunities*. On January 17, 1972, Dyson delivered the Gibbs Lecture at the annual AMS Meeting, which he entitled “Missed opportunities” [12]. In it, he famously declared that “the marriage between mathematics and physics, which was so enormously fruitful in past centuries, has recently ended in divorce.” He went on to recount a tale of how he “missed the opportunity of discovering a deeper connection between modular forms and Lie algebras, just because the number theorist Dyson and the physicist Dyson were not speaking to each other.” Happily, and in no small part spurred on by Dyson’s lecture, it appears that mathematics and physics have reconciled in the ensuing decades and are once again collaborating harmoniously.

## 1.4 Dyson’s boundless curiosity

Dyson described what he regarded as *his job* in mathematics and physics as follows: “I define a pure mathematician to be somebody who creates mathematical ideas, and I define an applied mathematician to be somebody who uses existing mathematical ideas to solve problems. According to this definition, I was always an applied mathematician, whether I was solving problems in number-theory or in physics.” Dyson was a “fox.” He did not discover new physical theories, but, with an unflinching instinct for the most interesting open questions, went ahead

to elucidate the mathematical structure of physical theories and solve difficult concrete problems.

It is well known that Dyson also got involved in engineering projects with *General Atomic*, such as the design of the TRIGA reactor, the design of small nuclear bombs with an intended application to the propulsion of spaceships by nuclear explosions (Project Orion), etc. He was also involved in political initiatives, such as the 1963 Partial Test Ban Treaty, which he supported in spite of the fact that it rendered Project Orion obsolete. In his later years, he wrote many very successful books for a general audience, such as *Disturbing the Universe*, *Infinite in All Directions*, *The Scientist as Rebel*, and *Maker of Patterns*. Dyson was a remarkably talented writer, and rumor has it that he never had to write any page twice. In recent years, he wrote numerous stimulating reviews for the *New York Review of Books* and corresponded with his readers.

## 2 Personal Recollections

### 2.1 Stephen L. Adler

My first very indirect contact with Freeman probably occurred while I was a graduate student at Princeton (1961–1964), when I occasionally came with classmates to attend the Tuesday theoretical seminar at the Institute for Advanced Study (IAS). Oppenheimer and other members of the IAS physics faculty sat in the front row, and peppered the speaker with questions. My friends and I sat in the rows behind the first row. Freeman was almost certainly at one or more of these sessions, but I didn't speak with him.

My real association with the IAS, and with Freeman, began in the fall of 1965, after I had done work on consequences of the partially conserved axial-vector current hypothesis, current algebras, sum rules, and neutrino physics that had attracted much attention in the high energy physics community. Freeman's interests at that point were shifting to astrophysics, Tullio Regge's interests were basically mathematical, and C. N. (Frank) Yang had left the IAS to go to Stony Brook, leaving no faculty with interests focusing on current issues in elementary particle physics. This led to a decision to bring in younger people on a temporary basis. One consequence was that out of the blue (at least so it seemed to me at the time, but undoubtedly Freeman, Tullio, and Princeton University faculty

were involved) I received a phone call from Oppenheimer when I was at Harvard, offering me a five-year membership at the IAS at a generous salary, much more than I was getting as Junior Fellow. Oppenheimer also told me that a similar offer was being made to Roger Dashen (whom I had met briefly in the spring of 1965) so that neither of us would feel too lonely. Researching in the School of Natural Sciences (SNS) minutes much later, I learned that this initiative was part of a decision to divide the School of Mathematics at the IAS into separate Schools of Mathematics and Natural Sciences. This division was completed during the 1965–1966 academic year.

It didn't take me long to decide to accept the IAS offer. In the spring of 1966 I visited Murray Gell-Mann's group at Cal Tech, and got to know Roger Dashen much better before we both moved to Princeton. When Dashen and I arrived at the IAS for the fall semester of 1966, we had the job of restarting the high energy physics program. Freeman and Tullio both gave us remarkably free rein in doing this, a model behavior that I have tried to emulate as younger faculty (Witten, Wilczek, Seiberg, Maldacena, and Arkani-Hamed) were much later on brought into the School. Although our status was that of long-term member, Roger and I participated in SNS meetings in Oppenheimer's office, along with Freeman and Tullio. After Oppenheimer died in February 1967, the meetings moved to Tullio Regge's office.

Instead of restarting the Tuesday theoretical seminar, Roger and I started two seminars, one on Mondays for local speakers and one on Fridays for out-of-town speakers, alternating biweekly with the high energy physics group at Princeton University. A long-term project, which had strong support from Freeman and Tullio, was to separate the physics books from the mathematics books. These were all shelved together in the library room on the second floor of Fuld Hall, and were arranged in alphabetical order by author with no subject index. I undertook to organize this separation, with the acquiescence of Carl Kaysen, the new IAS director. Armand Borel, the Mathematics professor responsible for the library, grumbled that I was only a long-term member, but he was as always pragmatic and in the end did not block this reform from going ahead. I spent weeks after lunch going through the card catalog to make the separation, and a new librarian was hired to catalog the physics books by subject using the Library of Congress system. Freeman's support was vital in making sure that all of this went through.

Much later on, when a younger generation populated the IAS Mathematics faculty, the mathematics books were also rearranged by subject.

Although Roger and I were in the same office building as Freeman for a few years after we came, I had only a few physics interactions with Freeman; our interests had diverged substantially. Freeman's main impact on me and the high energy physics group was his hands-off attitude, in letting us run things unimpeded, and his support when interactions with the rest of the IAS were involved. Freeman was a passionate supporter of Kaysen's initiative to broaden the IAS by creating a School of Social Science, which was opposed at that time by some of the Mathematics and Historical Studies faculty. Additionally, he gave strong support to bringing biology to the IAS, now taking the form of the Systems Biology group within the SNS. Freeman's broad-mindedness, his openness to new ventures, has been a very significant legacy to the IAS. Much later on, when Congress eliminated the university exemption from the ban on mandatory retirement, Freeman set a good example by retiring at 70 even though he did not have to. Most of us in Mathematics and Natural Sciences have followed his example, by retiring at or before 70, and this has allowed both Schools to bring in new, younger faculty, keeping the IAS vital as it moves forward into the future.

## 2.2 Krishnaswami Alladi

Freeman Dyson (1923–2020) was a brilliant physicist and mathematician who was influential not only due to his fundamental research contributions, but also because his views on various important scientific issues always attracted worldwide attention. He is known the world over as an outstanding theoretical physicist, but relatively few know that he began his research career as an undergraduate by providing a simple and elegant explanation of a remarkable theorem on partitions discovered by the Indian mathematical genius Srinivasa Ramanujan. Both my late father Prof. Alladi Ramakrishnan and I had the privilege of having known Dyson, and here I will provide some personal remembrances. But I begin by giving a brief account of Dyson's work relating to Ramanujan's congruences for the partition function, because this was Dyson's first important discovery.

*2.2.1. Dyson's rank for partitions.* In the early part of the 20th century, Srinivasa Ramanujan revolutionized the theory of partitions by discovering some spectac-

ular results. One of his startling discoveries was that  $p(5n + 4) \equiv 0 \pmod{5}$ ,  $p(7n + 5) \equiv 0 \pmod{7}$ , and  $p(11n + 6) \equiv 0 \pmod{11}$ , where  $p(n)$  denotes the number of partitions of a positive integer  $n$ . Ramanujan's mentor G. H. Hardy of Cambridge University was stunned to see these congruences because partitions represent an additive process, and so no one would expect that partitions would satisfy such lovely divisibility relations. Ramanujan gave proofs of these congruences, but these proofs were analytic in nature. Since partitions are combinatorial objects, it was desirable to understand these congruences combinatorially. Such an explanation was found in 1944 by Freeman Dyson who was an undergraduate mathematics major at Cambridge University at the time.

Dyson defined the *rank of a partition* as the largest part minus the number of parts. He observed that the rank can be used to split the set of partitions of  $5n + 4$  into five subsets of equal size, and the set of partitions of  $7n + 5$  into seven subsets of equal size. Thus the rank explains Ramanujan's partition congruences mod 5 and 7.

But then he noted that the rank would not explain the third congruence pertaining to 11. He went on to conjecture the existence of a partition statistic that he dubbed the *crank* which would explain why 11 would divide  $p(11n + 6)$ . Dyson published his findings in a charming paper [4] in 1944 entitled "Some guesses in the theory of partitions" in the Cambridge University undergraduate mathematics journal *Eureka*. There he humorously remarked that it was probably the first instance in mathematics when an object (the crank) was named before it was found! Interestingly, 43 years later, the crank was found by George Andrews and Frank Garvan during the Ramanujan Centennial Conference at the University of Illinois, Urbana, in the summer of 1987, and thus Dyson's crank conjecture was solved.

Since then the study of cranks for general partition functions and their relatives has become an active area of research in number theory.



L to R: Krishnaswami Alladi, Frank Garvan, George Andrews, and Freeman Dyson, at the Mathematics Department, University of Florida - March 25, 2013.

2.2.2. *Dyson's other mathematical work.* Dyson made several more fundamental contributions to mathematics. We mention just one here.

One of the fundamental questions in the study of irrational numbers is to estimate how closely algebraic irrationals can be approximated by rationals. In 1909, the Norwegian mathematician Axel Thue established a deep result for algebraic numbers of degree at least 3, namely an upper bound on the *irrationality measure* for such algebraic numbers. From this it followed that equations like

$$x^k - dy^k = N,$$

where  $d$  is not a  $k$ th power and  $N$  any integer, have at most a finite number of solutions in integer values of  $x$  and  $y$  if the integer  $k$  is at least 3. In contrast, such an equation can have infinitely many solutions in integers  $x$  and  $y$  if  $k = 2$ . Thue's result on irrationality measures was significantly improved by Carl Ludwig Siegel in 1921. Dyson further improved on Siegel's theorem in 1947, and the final definitive result was established by K. F. Roth in the fifties. Thus Dyson made

a notable contribution to this major mathematical problem when he was still a student.

*2.2.3. Dyson and quantum electrodynamics.* In 1947, Dyson moved to the United States to work under Hans Bethe at Cornell University. There Dyson came into contact with Richard Feynman who simultaneously and independently of Julian Schwinger and Shinichiro Tomonaga had done pioneering work in quantum electro-dynamics. But the method of Feynman which was diagrammatic was very different from the field-theoretic approach of Schwinger and Tomonaga. In 1949 Dyson *proved* [5] that the two approaches were equivalent and this propelled him to stardom in the world of physics.

In the 1960s, as a schoolboy, I had heard my father speak highly of Dyson on many occasions. While working for his PhD in probability and stochastic processes at the University of Manchester, my father met Dyson in 1949 at a conference in Edinburgh where Dyson was hailed as a rising star. Later in 1957–58, when my father was a Visiting Member at the Institute for Advanced Study in Princeton, he interacted more closely with Dyson who was by then a permanent member.

In 1967, my father wrote a paper showing how the Feynman diagrams coalesce in a way that was simpler than Dyson's derivation.

*2.2.4. Contacting Dyson in 1972.* My fledgling research in number theory began in 1972 when I was just entering the BSc class at Vivekananda College of Madras University. I was only 16 then, and I was fascinated by Fibonacci numbers and arithmetical functions. In order to get an assessment of my early research, my father sent my work to very eminent mathematicians to get their opinion and advice. My father also wrote to Dyson because he knew that Dyson had begun his academic life in number theory as an undergraduate. Dyson wrote back saying that my work showed that I had promise, but that a talented youngster should take to a more serious subject like physics, instead of pursuing number theory which he considered "recreational"!

Dyson on many occasions had referred to number theory as recreational, but at the same time, he had emphasized that his investigations in number theory had given him the greatest pleasure.

2.2.5. *Interacting with Dyson at the Institute for Advanced Study (1981–82)*. I spent the academic year 1981–82 as a Visiting Member at the Institute for Advanced Study. My main interaction was with the Fields Medalists Atle Selberg and Enrico Bombieri in the School of Mathematics. Dyson was in the School of Natural Sciences, and so I did not see him in the mathematics seminars. But I did see him at the daily afternoon tea. I conversed with him a few times and he enquired about the work I was doing in analytic number theory and the progress I was making.

My wife Mathura and I had a nice apartment—56 Einstein Drive—on the grounds of the Institute. My daughter Lalitha was just a few months old.

Mathura and I were invited to dinners and parties quite a few times, and we needed a babysitter for Lalitha on those occasions.

I was told that Dyson's daughter Rebecca, who was then 14 years old, would be a good babysitter. So I approached Dyson with this request and he was quite pleased to convey it to his daughter. Indeed, every time Rebecca would babysit Lalitha, Dyson would personally drop his daughter at the front door of our apartment and pick her up later. Each time he would say a warm hello when he dropped her, and a pleasant goodbye when he picked her up.

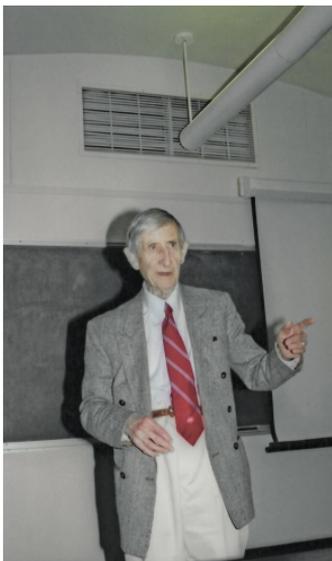
2.2.6. *The Selected Papers of Freeman Dyson*. There was an instance when what Dyson said in the Preface to his *Selected Papers* [8] was useful to me during my term as Chair at the University of Florida. I was at the CIRM outside of Marseille attending a conference in number theory. At the excellent library of the CIRM, I happened to come across [8]. This book is a collection of *all* his papers in mathematics and only a selection of some of his papers in physics. In the Preface to this book, Dyson says that in mathematics, a theorem proved is a theorem forever, and so it is customary to publish the collected works of a mathematician. By contrast, in physics, most papers are speculative, and so only years later would a physicist know which papers are correct and truly significant. So it is customary to publish only selected papers of a physicist. Coincidentally, when I was attending that conference at the CIRM, I received a letter from the Dean saying that the Provost's Tenure and Promotion Committee wanted some justification as to why one of my colleagues was being put up for promotion to full professorship when this person had only 28 research publications instead of the required 30! In

my response I emphasized the quality of work of my colleague; quoting Dyson from his Preface, I added a comment that the 28 papers by this colleague would amount to more than 50 by a physicist or a chemist! The promotion was approved without further questioning.

*2.2.7. 80th anniversary of the Institute for Advanced Study, 2010.* My next meeting with Dyson was in Princeton in September 2010, when I was invited by Professor Peter Goddard, Director of the Institute for Advanced Study, for the conference to celebrate the 80th anniversary of the Institute. Mathura and I were invited to the banquet of the 80th Anniversary Conference. At the banquet, Freeman Dyson gave a magnificent after-dinner speech about the development of theoretical physics at the Institute. In giving a fantastic account of the 80 years of the Institute, Dyson was critical that Robert Oppenheimer, who was the Director from 1947 to 1965, concentrated too much on particle physics. Dyson pointed out that it was at his insistence that a program on astrophysics was started at the Institute in 1958 with the appointment of Bengt Stromgren. Dyson also suggested the great astrophysicist Subrahmanyam Chandrasekhar of the University of Chicago for a permanent appointment at the Institute, but Chandrasekhar was not interested in the offer.

In retrospect, Dyson said that he felt it was better that Stromgren was appointed as a Permanent Member because Chandrasekhar was a “lone wolf” who preferred to work alone and so may not have blended with the culture of the Institute where Permanent Members spend considerable time interacting with visitors. Dyson was known to be frank and forthright, and what he said at the banquet was a confirmation of this.

*2.2.8. Visit to Florida in 2013.* My last interaction with Dyson was when he visited the University of Florida in March 2013 in response to my invitation to deliver the Ramanujan Colloquium. This colloquium series, so generously sponsored by George Andrews, has enabled us to get world-famous mathematicians as speakers every year. Each speaker would give a public lecture of wide appeal, namely the Ramanujan Colloquium, followed by two more specialized seminars during the next two days.



Dyson making a point during his Number Theory Seminar talk at the University of Florida - March 26, 2013.

For the Ramanujan Colloquium, Dyson spoke on the theme “Playing with partitions” in which he described how the work of Ramanujan fascinated him, and how he arrived at the notion of the *rank* to combinatorially explain two of Ramanujan’s partition congruences. He spoke with enormous energy, something you would not expect in someone who was 90 years of age. After making some introductory comments about Ramanujan and partitions, Dyson surprised (or should I say shocked!) everyone by saying: “I hold Hardy personally responsible for the death of Ramanujan.” Dyson pointed out that Ramanujan was away from his family, and that the rigors of life in England during World War I took a toll on his health. He stressed that Ramanujan needed a warm and considerate friend, but Hardy was aloof and did not realize Ramanujan’s needs. As another example of Hardy’s cold demeanour, he pointed out that when he discovered the rank as an explanation of Ramanujan’s congruences, Hardy gave Dyson the cold shoulder and did not show any interest in this work.

In the evening following the colloquium, we had a banquet in honor of Dyson. In my speech at the banquet, I referred to my first contact with Dyson in 1972, and reminded him that he advised me then to take to physics instead of number theory. He smiled and nodded when I turned towards him as I said this.

The next day, Dyson addressed the Number Theory Seminar on the theme “New strategies for prisoner’s dilemma.” His third lecture was a colloquium in the physics department entitled “Are gravitons in principle detectable?”

He started this thrilling lecture by saying the following in a thunderous voice: “I hate dogmas and always question them.” The physics auditorium was overflowing with many students squatting in the aisles and some standing. Dyson’s visit and lectures made a lasting impression on all of us.

### 2.3 Pavel Bleher

January 1992. I am very excited: I am coming to the Institute for Advanced Study (IAS) for one month, where I will be working with Freeman Dyson on the distribution of eigenvalues in quantum integrable systems. This will be a continuation of our joint project with Joel Lebowitz and Zheming Cheng, which we started in Fall 1991. It is midwinter, a very cold late evening, and we (my wife Tanya and I) are tired after our long overseas trip. We enter our apartment at the Institute and find a greeting note from Freeman and Imme, a welcome dinner on a table, and a refrigerator full of food. This was extremely warm and touching.

My first acquaintance with the works of Freeman Dyson was in the earlier seventies, when I was working with Yakov Sinai on the critical phenomena in the Dyson hierarchical models, introduced by Dyson in his proof of the existence of a phase transition in the classical ferromagnetic spin chain with the Hamiltonian

$$H(\sigma) = -J \sum_{i \neq j} \frac{\sigma_i \sigma_j}{|i - j|^a}, \quad \sigma_i = \pm 1,$$

where  $1 < a < 2$  and  $J > 0$ . The Dyson hierarchical model Hamiltonian is

$$H_D(\sigma) = -J \sum_{i \neq j} \frac{\sigma_i \sigma_j}{d(i, j)^a},$$

where  $d(i, j)$  is the hierarchical (2-adic) distance. Since  $d(i, j) \geq |i - j|$ , the interaction in the Dyson hierarchical model is weaker than in the original model with the power-like interaction, hence by Griffiths' inequality the existence of the long range in the Dyson hierarchical model implies the one in the power-like model. Dyson derives a recurrence inequality for the magnetization in the hierarchical model under doubling of the volume, and proves that it remains greater than a positive constant at low temperatures as the volume goes to infinity. This proves the existence of the thermodynamic limit magnetization at low temperatures in the classical spin chain with the power-like interaction.

The Dyson hierarchical model is of great interest for the theory of phase transitions and critical phenomena, because for this model the renormalization group transformation reduces to a nonlinear integral transformation, and this allows a study of critical phenomena unavailable in other models.

In January 1992, Freeman, Joel, Zheming, and I were working on the limiting distribution of the error function in lattice problems and quantum integrable systems. We began with the classical circle problem about the asymptotics, as  $R \rightarrow \infty$ , of the number of lattice points in a circle of radius  $R$ ,

$$N(R) = \# \left\{ (i, j) \in \mathbb{Z}^2 \mid \sqrt{i^2 + j^2} \leq R \right\}.$$

Heath-Brown proved that the normalized error function

$$F(R) = \frac{N(R) - \pi R^2}{R^{1/2}}$$

has a limiting probability density  $p(x)$  in the ergodic sense, so that for every bounded continuous function  $g(x)$  on the line,

$$\lim_{T \rightarrow \infty} \frac{1}{T} \int_0^T g(F(R)) dR = \int_{-\infty}^{\infty} g(x) p(x) dx.$$

Heath-Brown proved that the density  $p(x)$  is an entire function, and it decays, as  $x \rightarrow \pm\infty$ , faster than polynomially. We extended this result of Heath-Brown to the shifted circle problem, with

$$N(R; \alpha)$$

$$= \# \left\{ (i, j) \in \mathbb{Z}^2 \mid \sqrt{(i + \alpha_1)^2 + (j + \alpha_2)^2} \leq R \right\},$$

$$\alpha = (\alpha_1, \alpha_2), \quad 0 \leq \alpha_1, \alpha_2 \leq 1,$$

and we proved that the normalized error function

$$F(R; \alpha) = \frac{N(R; \alpha) - \pi R^2}{R^{1/2}}$$

has a limiting probability density  $p(x; \alpha)$ , which is an entire function. Most importantly, we obtained estimates of  $p(x; \alpha)$  as  $x \rightarrow \pm\infty$ . We showed that for all  $\epsilon > 0$ ,

$$\lim_{x \rightarrow \pm\infty} \frac{\log p(x; \alpha)}{|x|^{4+\epsilon}} = 0,$$

and

$$\lim_{x \rightarrow \pm\infty} \frac{\log P^\pm(x; \alpha)}{|x|^{4-\epsilon}} = \infty,$$

where

$$P^\pm(x; \alpha) = \left| \int_{\pm x}^{\pm\infty} p(x; \alpha) dx \right|.$$

Roughly speaking,  $p(x; \alpha)$  decays at infinity as  $\exp(-cx^4)$ .

I returned to the IAS the following fall, this time for two semesters (later it was extended to the third semester). During my stay at the IAS, Freeman and I worked on various projects. One project was about the variance of the limiting probability density in the shifted circle problem  $p(x; \alpha)$ . We showed that

$$\int_{-\infty}^{\infty} xp(x; \alpha) dx = 0,$$

and we studied the variance

$$D(\alpha) = \int_{-\infty}^{\infty} x^2 p(x; \alpha) dx,$$

as a function of  $\alpha$ . We showed that  $D(\alpha)$  is a continuous function, and for every rational  $\beta \in \mathbb{Q}^2$ , there exists the limit

$$\lim_{\alpha \rightarrow \beta} \frac{D(\alpha) - D(\beta)}{|\alpha - \beta| |\log |\alpha - \beta||} = C(\beta) > 0.$$

Thus,  $D(\alpha)$  is a “wild” function, with a sharp local maximum with infinite derivative at every rational point.

Another project concerned the mean square limit for lattice points in a sphere. Let

$$N(R) = \#\{n \in \mathbb{Z}^3 \mid |n| \leq r\}$$

be the number of integral points inside a sphere of radius  $R$  centered at the origin, and let

$$F(R) = \frac{N(R) - \frac{4\pi R^3}{3}}{R}.$$

Then we prove that the following limit exists:

$$\lim_{T \rightarrow \infty} \frac{1}{T \log T} \int_1^T F^2(R) dR = K,$$

where

$$K = \frac{32\zeta(2)}{7\zeta(3)}$$

and  $\zeta(s)$  is the Riemann zeta function.

During my stay at the IAS, I often had lunch with Freeman, and we discussed various topics. He told me about his life in Cambridge and his teachers, Hardy, Besicovitch, Dirac, and others. In his office at the IAS there were several Russian books, including Dostoevsky and Tolstoy. Freeman learned Russian in Cambridge, talking to Besicovitch and some other Russians. This is brilliantly described in his autobiographical book *Maker of Patterns*.

Once, at lunch, I told Freeman about the problem of spacings between energy levels in quantum linear systems, the problem I was working on at the time. The problem is as follows (I use notations from Freeman's later notes). Let  $\mathbf{w}$  be a vector of frequencies, whose components  $w_j$ ,  $j = 1, \dots, d$ , are a set of  $d$  real numbers linearly independent modulo 1. Let  $R_1$  be a bounded convex region including the origin  $O$  in Euclidean space of  $d$  dimensions, and let  $R_z$  be the region  $R_1$  expanded homothetically by a factor  $z$  leaving  $O$  fixed. Let  $M_z$  be the set of integer points  $\mathbf{m} = (m_1, \dots, m_d)$  in  $R_z$ . Let the number of points in  $M_z$  be  $N_z$ . Consider the set  $Q_z$  of linear combinations modulo 1,

$$(\mathbf{m}, \mathbf{w}) = \sum_{j=1}^d m_j w_j \pmod{1},$$

a set of  $N_z$  distinct real numbers which we imagine to be arranged in sequence around a circle of circumference 1. Let  $D_z$  be the number of different distances that occur between pairs of nearest neighbors in  $Q_z$ . The question I asked Freeman is how does  $D_z$  behave as  $z$  becomes large? In the case  $d = 1$ , it is easy to prove that  $D_z$  is always either 2 or 3. I examined the case  $d = 2$  numerically for various choices of  $\mathbf{w}$  and found that  $D_z$  varies remarkably little. I found that  $D_z$

is usually about 12 and shows no systematic tendency to increase with  $z$ . I was especially interested in the case when the frequencies  $w_j, j = 1, \dots, d$ , together with 1, form a basis in the set of real algebraic integers in an algebraic field. My conjecture was that in this case  $D_z$  is bounded, and the set  $Q_z$  exhibits some self-similarity properties in  $z$ . About four weeks later Freeman brought a marvelous proof of the following theorem.

**Theorem** (Dyson). *Let  $w_j, j = 1, \dots, d$ , be real algebraic integers, all belonging to the same field  $\Phi$  of degree  $(d + 1)$ , and are linearly independent over the rationals modulo 1. Then  $D_z$  has a bound independent of  $z$ .*

Freeman's proof can be divided into two parts. In the first part, the finiteness of  $D_z$  is proved for any *badly approximable* vector  $\mathbf{w}$ , so that for some  $K > 0$ ,

$$(\mathbf{m}, \mathbf{w}) \geq \frac{K}{|\mathbf{m}|^d} \quad \forall \mathbf{m} \neq \mathbf{0};$$

and in the second part, a theorem of Perron is invoked, which shows that under the conditions of Dyson's theorem, the vector  $\mathbf{m}$  is badly approximable.

In May–June 2019 I came to the Institute for Advanced Study for several days for a conference. I was very glad to see Freeman in good health and spirits (he was 95 years old at that time). Freeman asked me about my recent work. I told him about my work with Vladimir Fokin, Karl Liechty, and Thomas Bothner on random matrices and exact solutions of the six-vertex model of statistical physics. Freeman listened very carefully and asked many questions. Then he asked if the results were published. I said yes, there were several papers and also my book with Karl Liechty, *Random Matrices and the Six-Vertex Model*. Freeman asked if I could send him the published results. I said yes, and I was very glad to send him a copy of our book with Karl.

This was my last meeting with Freeman, and I believe I am a lucky man to have known him, talked to him, and collaborated with him on various projects.

## 2.4 Jennifer T. Chayes

I first met Freeman in 1980 at a mathematical physics lunch in Princeton's Jadwin Hall, room 343, during my second year of grad school. The room was filled with legends and future legends of mathematical physics. None was warmer and more encouraging to me than Freeman. The lunches were glorious, unstructured brown bags, where someone would, often spontaneously, go to the blackboard and talk not about a research accomplishment, but about how he (it was almost always he) was stuck on some problem. During the lunches in 1980–83, the remainder of my years at Princeton, we would often discuss atomic physics or statistical physics, areas in which Freeman had made fundamental and beautiful contributions.

The work of Freeman that I studied as a grad student always started with a question in physics, and then took a journey through some lovely mathematics. Freeman is probably most famous for his quantum electrodynamics, but it is his statistical physics that captured my heart and my imagination. One of the problems which Freeman studied was the one-dimensional  $1/r^2$  ferromagnetic Ising model—a simple chain of Ising spins which would have been trivial if they had nearest-neighbor couplings, but was highly nontrivial due to the long-range interactions, especially the magical power of 2. Many legends had made contributions to this problem—among the physicists, Thouless, who conjectured a discontinuity in the magnetization (1969), and Anderson, Yuval, and Hamann, who did an early renormalization group analysis (1971). Among the mathematical physicists who proved some of the physics conjectures were many legends—including Dobrushin and Ruelle, in addition to Freeman, around 1970, and Fröhlich and Spencer about a decade later. Freeman in particular had a spectacularly clever and beautiful analysis where he introduced what is now known as the “Dyson hierarchical model,” for which renormalization properties could be easily established, and used it to bound the actual model and thereby prove one side of the existence of the phase transition. As with much of Freeman's work, he not only established a rigorous result, but also introduced a new way of thinking about the problem (in this case, a model designed for renormalization) which physicists and mathematicians use decades later. In 1988, in collaboration with Michael Aizenman, Lincoln Chayes, and Charles Newman, we proved the discontinuity in the magnetization using many of the ideas going back to Freeman's original

work. Upon seeing me shortly after this, Freeman said, “I knew you would do something important”—which was probably the most thrilling compliment I ever received!

Freeman continued to be an inspiration to me on so many levels. During 1994–95 and 1996–97, when I was a member of the Institute for Advanced Study, I would often stop by and chat with him as he was having lunch (mostly by himself) or having tea in the Fuld Hall lounge. He was my model of how to move through the world, always grounded by mathematics, while venturing bravely into fields over which we have so much less control.

## 2.5 Jürg Fröhlich

I first heard of Freeman Dyson as an undergraduate student of mathematics and physics at the ETH in Zurich, during the second half of the sixties. Two of my teachers, Klaus Hepp and the late Res Jost, who was a close friend of Dyson, followed his scientific work. At that time, Dyson’s and Lenard’s analysis of *Stability of Matter* looked particularly exciting to them. Hepp and Jost greatly admired Dyson as the leading mathematical physicist after World War II, and they conveyed their admiration to us students. Thus, for me, Dyson was the epitome of a highly successful theorist whose example one would have to try to follow. In a seminar for undergraduate students, in 1968, we had to give talks about relativistic quantum field theory, and this was the occasion for us to learn about Dyson’s celebrated work on quantum electrodynamics of 1949 [5]. In passing, I might say that, in retrospect, I find it perplexing that, during that seminar, we neither heard nor talked about the work of the eminent Swiss theorist E. C. G. Stückelberg, a professor at the Universities of Geneva and Lausanne, who had invented a manifestly Lorentz-covariant form of perturbation theory in RQFT already back in 1934 and had introduced the ideas of a positron representing an electron traveling backwards in time and of diagrams to label terms in the perturbation series of a quantum field theory, in 1941, several years before Feynman. To return to Dyson, I should add that we also learned that he had contributed important ideas and results to a development that flourished at the ETH, at the time, namely axiomatic quantum field theory, in the sense of the late Arthur S. Wightman. As an example, I recall that there is a remarkable integral representation

of commutators of local fields in RQFT, called *Jost–Lehmann–Dyson representation*, which has various interesting applications, among them a general proof of Goldstone’s theorem, which says that, in RQFT, the spontaneous breaking of a continuous symmetry is accompanied by the appearance of a massless boson in the particle spectrum of the theory. It should also be mentioned that the outstanding work of Klaus Hepp on renormalized perturbation theory in RQFT built on ideas originally proposed by Dyson (and Stückelberg). Thus, there were many intellectual connections between Dyson and people in the environment in which I grew up as a student. The work of Thomas C. Spencer (IAS) and myself on the phase transition in the  $1/r^2$  ferromagnetic Ising chain was inspired by some of his earlier results.

During several stays at the Institute for Advanced Study between 1984 and 2016, my wife and I developed very friendly ties with Freeman Dyson and his wife Imme. Not only have I lost a colleague whom I deeply admired, we have lost a friend.

## 2.6 Joel Lebowitz

The recent deaths of Freeman Dyson and Phil Anderson, whose birthdays were just two days apart and whose domiciles were less than two miles apart, mark the end of an era in mathematical/theoretical physics. I describe below a few of my interactions with Freeman over a period of more than sixty years.

Freeman’s death came as a sad surprise to me, despite the fact that I knew that he was in poor health. In fact, just a few days before his death, as we walked together from the physics building to the dining room of the IAS, I asked Freeman about his health. His answer was “I could talk about it for hours, *but I will not.*” The accent on the last four words was emphatic. His voice had lost almost none of the resonance which thrilled so many varied audiences for so many years. These audiences included mathematicians, physicists, philosophers, and politicians as well as college and high school students.

I first met Freeman in the spring of 1953, when I was a first-year graduate student at Syracuse University. I drove with my thesis advisor Peter Bergmann from Syracuse to Ithaca for a seminar at Cornell by Joe Doob, the probabilist from Illinois, who was also in the car with us. After the seminar we were invited for

drinks at Dyson's house—Freeman was already a famous professor there. After drinks we all went to an Italian restaurant and Freeman paid for my dinner which, given the fact that my graduate assistant salary was not very large (I believe it was \$1,500, per academic year), was much appreciated. I have been the recipient of many kindnesses from Freeman since then.

My next close encounter with Freeman was during the academic year 1967–1968, when he was a visiting professor at the Belfer Graduate School of Science, Yeshiva University, where I was a faculty member. I remember Freeman giving a wonderful course on astrophysics. I have not been able to find any references to those lectures except for an article by Freeman in the October 1968 issue of *Physics Today*, entitled “Interstellar transport.” The article describes two designs of spaceships powered by nuclear bomb detonations which could enable interstellar voyages “in about 200 years time.” At the end of the article Freeman writes “This article is based on a lecture given at the Belfer Graduate School of Science, Yeshiva University, in January 1968, as an entertainment between semesters.”

My contact with Freeman and his wife Imme increased greatly after my wife Ann and I moved to Princeton in the late '70s, to be closer to Rutgers University where I still work. I spent part of the 1980 academic year at the IAS as a guest of Freeman. We saw each other quite often at seminars and also socially. Whenever we met socially, Ann would kiss Freeman on the cheek, which I think he enjoyed but made him feel a bit uncomfortable. It was not in the style of his British upbringing. He was, however, far from stuffy. He was a good dinner companion, having informed and strongly held beliefs, almost never the conventional ones, about almost any subject. I did not always agree with him but we remained friends.

Let me now come briefly to our direct scientific interactions as mathematical physicists. A quote from Dyson's book *Eros and Gaia* (pp. 164–165) describes his attitude to the subject:

To make clear the real and lasting importance of unfashionable science, I return to the field in which I am an expert, namely mathematical physics. Mathematical physics is the discipline of people who try to reach a deep understanding of physical phenomena by following the rigorous style and method of mathematics. It is a discipline that

lies at the border between physics and mathematics. The purpose of mathematical physics is not to calculate phenomena quantitatively but to understand them qualitatively. They work with theorems and proofs not with numbers and computers. Their aim is to qualify with mathematical precision the concepts upon which physical theories are built.

My first direct contact with Freeman's scientific work came in 1968 when I was working with Elliott Lieb on showing in a "mathematical physics" sense that statistical mechanics can provide a basis for the equilibrium thermodynamics of real matter consisting of electrons and nuclei interacting via Coulomb forces. A very crucial ingredient in our analysis was Dyson's proof with Andrew Lenard (1967) of the stability against collapse of macroscopic Coulomb systems. To quote from the paper with Lieb: "The Dyson-Lenard theorem is as fundamental as it is difficult."

My next scientific interaction, indeed collaboration, with Freeman, concerned the distribution of lattice points, a problem going back to Gauss. Consider a two-dimensional square lattice  $\mathbb{Z}^2$ . Take a disc with radius  $R$  centered at the origin. Find a bound on the deviation of  $N_0(R)$ , the number of lattice points in the disc, from its average value of  $\pi R^2$ .

The Gauss problem is related to the distribution of energy eigenvalues of a particle in a unit torus. In the early '90s, Pavel Bleher, Zheming Cheng, Freeman Dyson, and I considered the following more general problem. Take  $a \in [0, 1]^2$  and define  $N_a(R)$  as the number of lattice points in a disc of radius  $R$  centered at  $a$ , so that the Gauss problem corresponds to  $a = 0$ . So far no randomness. From the point of view of energy level statistics we are interested in the behavior of  $F_a(R) = (N_a(R) - \pi R^2)/R^{1/2}$  as  $R$  varies over some range, e.g.,  $R$  varies uniformly between 1 and  $T$ .

Following ideas by Heath-Brown, we proved the following result. The probability that  $F_a(R)$  lies in the interval  $(x, x+dx)$  approaches  $A \exp[-bx^4]dx$  weakly as  $T \rightarrow \infty$ .

Let me conclude with one of my favorite Dyson quotes, from his wonderful book, *Infinite in All Directions* [7, p. 118]:

To me the most astonishing fact in the universe .... is the power of mind which drives my fingers as I write these words. Somehow, by natural processes still mysterious, a million butterfly brains working together in a human skull have the power to dream, calculate ... to translate thoughts and feelings into marks on paper which other brains can interpret .... It appears to me that the tendency of mind to infiltrate and control matter is a law of nature.... Mind has waited for 3 billion years on this planet before its first string quartet. It may have to wait for another 3 billion years on this planet before it spreads all over the galaxy. Ultimately, late or soon, mind will come into its heritage.

I miss Freeman greatly.

## 2.7 Juan M. Maldacena

Freeman Dyson made crucial contributions to quantum electrodynamics. This is the theory that describes how electrons interact with light, with both electrons and light treated according to the laws of relativistic quantum mechanics. Before Dyson entered the scene, Feynman, Tomonaga, and Schwinger had developed apparently different theories. Feynman's theory led to easy recipes for computation, but essentially nobody else understood it. Tomonaga's and Schwinger's theory was more complicated but it seemed to rest on a more solid foundation, describing the system in the more standard quantum mechanical language. Dyson understood how these different approaches were related. He derived how Feynman's simple rules followed from the more basic rules of quantum mechanics. Dyson popularized the use of *Feynman diagrams*, by explaining how to use them to the researchers visiting the Institute for Advanced Study. After this outstanding contribution, he was made a permanent professor there.

We should emphasize that these theories had confusing aspects. In particular, corrections to some quantities seemed infinite. These infinities were removed by correcting the input parameters, such as the mass of the electron by infinite amounts. For many physicists and mathematicians this process seemed to be totally unjustified. The physical significance of these apparently infinite quantities was better understood through the work of Wilson in the '70s. Dyson jumped into

a murky problem and developed clear mathematical rules that all students today learn and apply to describe nature. It is rather paradoxical that this seemingly mathematically ill-defined theory is actually the most accurate of all of science. In fact, there is a particular property of the electron which has been both computed and measured with agreement up to 12 significant digits. Let us say a few words about this quantity. The electron can be pictured as a spinning charged particle. Since a moving charge generates a magnetic field, this behaves as a small magnet. The quantity in question is the strength of this magnet. The Dirac equation predicts a certain value in terms of the charge of the electron and its mass. The theory of quantum electrodynamics corrects it. These corrections come from the fact that, in this theory, the electron is surrounded by a cloud of “virtual particles” which modify its properties slightly. This has been calculated and measured with increasing precision since the fifties. When Freeman heard about the most recent measurements a few years ago, he sent a congratulatory letter to the team that had done the measurement and said, “I remember that we thought of QED in 1949 as a temporary and jerry-built structure, with mathematical inconsistencies and renormalized infinities swept under the rug. We did not expect it to last more than 10 years before some more solidly built theory would replace it. Now, 57 years have gone by and that ramshackle structure still stands... It is amazing that you can measure her dance to one part per trillion and find her still following our beat.”

## 2.8 Hugh Montgomery: A memorable conversation

In the autumn of 1971, I derived an incomplete result concerning the zeta function, and formulated a conjecture describing what I thought lay beyond what I could prove. In that era, in order to establish that a finding concerning zeta was new, one had to first show it to Selberg, in case it was already in his desk drawer. So I arranged to pay a brief visit to the Institute in April 1972. I described my work at the blackboard in Selberg’s office, and he remarked that it was “interesting.” That afternoon at tea I made small talk with Chowla, who noted that Dyson was standing across the room from us. He asked me if I had met Dyson, and when I said no, he insisted on dragging me over to be introduced. Dyson listened patiently to Chowla’s presentation, turned to me, and his first words were, “So

what are you thinking about?" I replied, "I think that the differences between the zeros of the zeta function are distributed with a density one minus the quantity  $\sin \pi u$  divided by  $\pi u$ , quantity squared." Without the slightest hesitation, he calmly responded, "That's the pair correlation of the eigenvalues of a random hermitian matrix." To say that I was stunned would be an understatement. Hilbert and Pólya had speculated that RH is true because of the existence of a certain unknown hermitian operator, but there had never been any evidence to support that idea. I had suspected that there might be some lesson to be learned from my conjecture, and was troubled that I didn't know what it was. Now Dyson was telling me that the zeros of the zeta function seem to be distributed in the way that one would expect, if they were eigenvalues. The conversation lasted a few minutes longer, but I have no recollection of what was said. Perhaps Dyson could tell that the poor graduate student standing in front of him was struggling to process further information, because when I went to bid my adieu to Selberg the next morning, he handed me a note from Dyson, in which he specified the exact pages in Mehta's book that I should read.

Maybe at the time of this conversation I thought that I was lucky to have had a chance encounter with a physicist. I soon realized that random matrix theory to which Dyson referred was constructed in the 1960s by Dyson himself, with just a few others. So it wasn't a matter of *a* physicist but rather *that* physicist. I have sometimes wondered how long it would have taken for the connection to be made, if Chowla had not so strenuously insisted that I be introduced to Dyson. It might have taken decades. Certainly it was fortuitous that the connection was discovered at the first possible instant.

## 2.9 Andrew Odlyzko

Freeman Dyson was one of the most remarkable people I have met, even though I was exposed to only a few facets of his long and astonishingly multifarious and productive life. One of my great regrets is that I did not get to know him earlier and did not interact with him more.

A much smaller but still substantial regret is that I am able to provide only very limited details about my most important contacts with Freeman. This is due to the restrictions on building access caused by the COVID-19 pandemic, which keep me

from the personal hard copy archives that contain my correspondence with him on the main topic that brought us together in the first place. That subject was the distribution of zeros of the zeta function and its conjectured connection with random matrix theory.

This connection resulted from a chance conversation between Freeman and Hugh Montgomery. It occurred at the usual afternoon tea at the Institute for Advanced Study in the spring of 1972. However, I did not become interested in this topic until the late 1970s. At that time I was working at Bell Labs and occasionally drove down to Princeton for interesting lectures. One of them was by Montgomery on his work on the pair correlation of the zeros of the zeta function and its likely link to eigenvalues of random matrices. That lecture aroused my interest in the computation of precise values of large sets of zeros of the zeta function, and in the development of new algorithms for that purpose. It was during this work, in the early 1980s, that I was introduced to Freeman. He had been pointed out to me on early visits to the Institute, back in the mid-1970s, but in those days I did not have any incentives to talk to him. That changed, however, with my dives into the mysteries of the distribution of zeta zeros.

I had extensive correspondence (via regular mail, as he was not using email during that period) and personal conversations with Freeman on eigenvalues of random matrices and ways to test the extent to which they behaved like zeta zeros. He suggested and enthusiastically supported many of the detailed numerical studies that I carried out. He was extremely knowledgeable about random matrices, as was to be expected given his seminal contributions to that subject. But he also knew a lot about the Riemann zeta function, and in general had great insight into many mathematical areas. Freeman had promised to write a preface to my planned book on the conjectures and computations about the zeta function. Unfortunately he passed away before I could call on him to fulfill that promise.

While Freeman's main technical contributions were in physics, he started out in number theory. This contributed to his appreciation of the different goals and approaches taken by mathematicians and physicists, and the different attitudes towards rigor in proofs. He was a major figure of 20th century science, and will be greatly missed.

## 2.10 Craig Tracy and Harold Widom

Freeman Dyson, in the early 1960s, laid the foundation for much of random matrix theory. Motivated by the work of Wigner, Mehta, and Gaudin, Dyson's papers had a number of novelties that continue to this day to influence current research. To quote from Dyson's *Selected Papers*:

I replaced Wigner's ensembles of symmetric matrices by ensembles of unitary matrices. Since unitary matrices form a group, this allowed me to bring the powerful methods of group theory into the analysis. The other novelty was a proof that the eigenvalues of the unitary matrices have precisely the same statistical behavior as the positions of classical point charges distributed with a fixed temperature around a circular wire. The well-known tools of classical statistical mechanics could therefore be applied to the eigenvalues.

A particularly prescient paper is Dyson's "A Brownian-motion model for the eigenvalues of a random matrix," which is a precursor to the  $\text{Airy}_2$  process that is central to current research in stochastic growth models. In a 1970 paper, Dyson returned to the orthogonal and symplectic ensembles to "complete the determination of eigenvalue correlations by finding explicit formulae for all the  $P_{n\beta}$  with  $\beta = 1, 4$ ." [Here  $P_{n\beta}$  are the  $n$ -level correlation functions.]

Our first correspondence with Dyson dealt with the accuracy of his classical Coulomb fluid model as applied to the Gaussian ensembles. This resulted in our first publication (together with Estelle Basor) in the field of random matrices. Subsequently we benefited from correspondence with Dyson concerning the orthogonal and symplectic ensembles.

## 2.11 Horng-Tzer Yau: Freeman Dyson and random matrix theory

In the 1980s, when I studied with Elliott Lieb toward my thesis, Freeman Dyson was a towering figure in every direction I studied. His celebrated work with Lenard on the stability of matter inspired the Lieb-Thirring inequality and stimulated many subsequent works on rigorous analysis of quantum many-body systems. In another work, Dyson established a rigorous upper bound on the ground state energy of hard-core bosons at low density. This upper bound was given a

matching lower bound by Lieb and Yngvason in 1998. This has led to many rigorous works concerning Bose gas and in particular the Gross–Pitaevskii equations for the Bose–Einstein condensates.

To many pure mathematicians, Dyson’s most famous works are perhaps those related to random matrices. After a teatime conversation with Hugh Montgomery in 1972, Dyson wrote to Atle Selberg saying that, “the pair correlation function of the zeta function [as computed by Montgomery] is identical with that of eigenvalues of a random complex matrix of large order.” Given this, one might think that Dyson had worked on the subject for an extended period of time. In fact, most of Dyson’s published works on random matrices occurred between 1962–63. He published a series of five papers under the title, “Statistical theory of the energy levels of complex systems” and the closely related article [11]. The main conclusions of these six papers include calculations of level correlation functions and the groundbreaking classification ( $\beta = 1, 2, 4$ ) of random matrix ensembles by the fundamental physical symmetries of the underlying quantum systems, i.e., *Dyson’s threefold way*. In addition, he wrote another article at the time titled, “A Brownian-motion model for the eigenvalues of a random matrix.” After 1963, Dyson rarely published papers on random matrices.

The random matrix theory community in the 1960s consisted of several nuclear physicists and mathematical physicists. The subject was founded by Eugene Wigner [19] who, according to Dyson in *The Oxford Handbook of Random Matrix Theory*, envisioned that “a random matrix would be a possible model for the Hamiltonian of a heavy nucleus.” Besides the works of Wigner and Dyson, major rigorous works on random matrices were done by Gaudin [15] and Mehta [16]. The works of Wigner, Dyson, Gaudin, and Mehta then laid the foundation of the mathematical theory of random matrices.

Dyson’s Brownian motion paper is very different from his other papers in this direction. In this groundbreaking work, Dyson sought to find dynamics which leave the eigenvalue distribution of a Gaussian random matrix ensemble invariant. I remember that in one dinner conversation with Dyson several years ago, I asked him how he came up with his Brownian motion construction. Dyson replied that he made a huge effort to construct a Newtonian mechanics that leaves the Coulomb gas distributions (which are the eigenvalue distributions of random matrices) invariant. After many failures, he realized that it’s impossible to do that

with purely Newtonian mechanics; the only possible way is through a friction which is exactly the Brownian motion.

While the importance of this paper is now well recognized, its relevance to random matrix theory was not known to my generation of mathematical physicists (or mathematicians for that matter) even up to the early 2000s. I first looked into Dyson's Brownian motion around 2006–07. At the time, Erdős, Schlein, and I were interested in the universality conjecture of the eigenvalue statistics of random matrices and had no ideas at all. We were bombarded almost daily at Harvard by the idea of using dynamics (Ricci flow) in the solution of the Poincaré conjecture. Coming off working on dynamics of Bose gas and the Gross–Pitaevskii equations, we were curious if the universality conjecture could be solved by some dynamical idea. From our training, it was natural to start with a matrix Brownian motion and then look into the dynamics of the eigenvalues. After a while, we realized that what we had tried was exactly Dyson's Brownian motion.

Dyson's Brownian motion turned out to be the key tool in the resolution of the universality conjecture on the eigenvalue statistics of random matrices, which many considered to be one of the most fundamental theorems in random matrix theory. Even more surprisingly, nearly sixty years after his paper was written, the universality theorem can still only be proved by invoking Dyson's Brownian motion at some stage of the proof. Although Dyson never mentioned the dynamics he constructed in connection with the universality conjecture (in fact, this conjecture was formulated several years later by Mehta in his book, *Random Matrices*), his motivation to construct dynamics leaving the eigenvalue distributions of Gaussian random matrices invariant was clear.

While random matrix theory is a great success today, it is interesting to note that at the time random matrix theory had failed in its original purpose to serve as a model for nuclear physics. Writing for the foreword of *The Oxford Handbook of Random Matrix Theory*, Dyson recalled, "All of our struggles were in vain. 82 levels were too few to give a statistically significant test of the model. As a contribution to the understanding of nuclear physics, random matrix theory was a dismal failure. By 1970 we had decided that random matrix theory was a beautiful piece of pure mathematics having nothing to do with physics. Random matrix theory went temporarily 'to sleep.'" By the mid 1970s, Dyson seemed to leave random matrix theory completely. Random matrix theory, however, soon started

to take off in many areas of mathematics and physics. The connection between random matrices and zeta functions discovered by Montgomery and Dyson led to many subsequent works by Katz, Rudnick, Sarnak, Keating, and Snaith. In a separate direction, random matrix theory has made major impacts in condensed matter physics and the connection with quantum chaos conjectures was made by Bohigas–Giannoni–Schmit in the 1980s. Going into the 1990s and 2000s, many new aspects of random matrix theory were discovered at an astonishing rate. Random matrix theory, initiated by Wigner, Dyson, Gaudin, and Mehta, has become a fundamental theory in mathematics and physics.

As we reflect on Dyson’s work today, it’s amazing to me how far he was ahead of his time. In the 60s, the prevailing tool of quantum many-body systems was perturbation theory. Dyson showed us that there is a life in the rigorous treatment of quantum many-body systems. In random matrix theory, Dyson did fundamental work regarding its classification and level statistics calculations. Above all, Dyson’s work on matrix Brownian motions is one of the earliest dynamical approaches to stationary problems in mathematics. Many time-dependent methods in mathematics, e.g., Hamilton’s work on the Ricci flow, only gradually emerged in the 1970s. Dyson was a pioneer of his time who was always full of new insights and original ideas.

Dyson once told me that he considered himself an applied mathematician in the sense that he only uses mathematics, but does not work on “pure mathematics.” He said that it is too difficult to invent new mathematics and that’s why he only “uses” mathematics. I did not know how to reply to his statement. I was wondering if what he did was not inventing mathematics, what else could it be?

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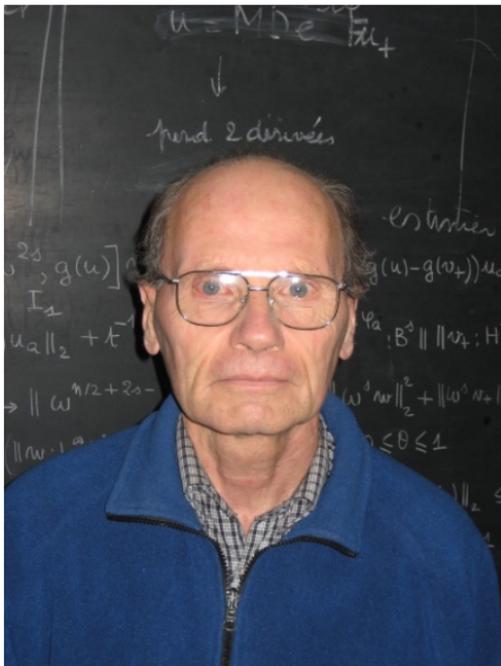
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## Jean Ginibre (1938-2020)



### 1 Jean Ginibre's legacy

by MONIQUE COMBESURE (Lyon)

My collaboration with Jean Ginibre started in 1972, with a work on the “quantum scattering of three body systems,” and continued until my ‘doctorat d’état’ in 1980. Our offices in Orsay were contiguous, and it was very easy to exchange ideas and research tracks. An anecdote from our first joint publication in 1974 made us laugh a lot: the editor asked “indicate the name of the author to whom proofs should be sent ...” and I naively said to Jean “it’s amazing: we just have to give the theorems and they send us the...demonstrations”! Humor was second nature to Jean; throughout his collaboration with Giorgio Velo, I could hear bursts of laughter coming from the adjoining office, and I would have liked to know

what good joke had triggered this hilarity! Running (which he did daily during his lunch break) was one of his passion . . . , which I also shared, but on my own level. Regularly when one of us had a new idea, we wrote it on a blackboard in one of our offices, where it would simmer quietly and boost our respective research. We participated together in international meetings, notably those of the IAMP, but also in Oberwolfach, and MSRI, which broadened the spectrum of our knowledge, and thanks to which other collaborations were initiated. Jean was passionate about music, and sang great masterpieces in choirs almost all his life. After I joined a laboratory in Lyon in 2001, we saw each other less regularly, but kept in touch by email. I visited him in the spring of 2019. He was in good shape; we went for a walk in Gif-sur-Yvette, we talked about many things. He was rather discreet about his “crab” as he used to say, but inexhaustible about music; but we would also chat about the political antics of the brilliant Cédric Villani, and the transformation of the bucolic Yvette valley and the Saclay plateau into a “French silicon valley.”

In this homage to J. Ginibre three scientists present three aspects of his important work. The study of correlation functions of classical gases and the convergence of the virial expansion was developed by David Ruelle using Banach space techniques. Using similar methods, J. Ginibre made an important contribution to the study of quantum gases at low activity in a series of three papers, considering respectively the Maxwell-Boltzmann, the Bose-Einstein, and the Fermi-Dirac statistics. In his contribution to this memorandum, D. Ruelle presents the methods and results of these three papers. Jean Ginibre and Giorgio Velo developed a systematic use of the Littlewood-Paley decomposition that further became standard in the field of dispersive inequalities for nonlinear Schrödinger and wave equations. Terence Tao presents the fruitful methods and main results obtained by Ginibre and Velo, that allowed further insights in this domain. The pioneering work of Jean Ginibre in 1965 on the so-called “Ginibre ensemble of random matrices” has become a remarkable source of inspiration and developments of many scientists after 2004. In a private correspondence of 2005, Ginibre wrote: “As regards to my original motivation about random matrices, it was pure curiosity and I had no applications in mind. The problem appears among the open questions in the last section of the last paper by Dyson and Mehta and I found it both natural and exciting.” In his contribution to this memorandum, Eugene Kanzieper presents the recent developments which show the fertility of this work of Jean Ginibre in various domains of science.

## 2 On Jean Ginibre's article, 'Reduced density matrices of quantum gases'

by DAVID RUELLE (Bures-sur-Yvette)

The wonderful article to be discussed below has been somewhat forgotten, but deserves careful consideration. In this piece of work J. Ginibre [1] studies quantum gases at low activity, both for the Maxwell-Boltzmann (MB) and the quantum (Bose-Einstein and Fermi-Dirac) statistics. The study is based on the Feynman-Kac formula (in fact the rigorous Kac [2] version) and uses the Kirkwood-Salzburg equations (see [3]) for correlation functions in classical statistical mechanics of a system of  $m$  particles. We assume that the particles have pair interactions satisfying natural conditions to ensure thermodynamic behavior.

For classical systems at low activity one can take an infinite volume limit of the Kirkwood-Salzburg equations in the form of a Banach space linear equation:

$$\rho = \zeta + K\rho \quad (*)$$

for the sequence  $\rho$  of correlation functions. In a suitable Banach space, we have  $\|K\| < 1$  for the norm of the operator  $K$  at low activity, and  $\rho$  is therefore uniquely determined [4] by (\*).

For quantum systems one has to replace the sequence  $\rho$  of classical correlation functions by a sequence of  $m$ -particle reduced density matrices  $\rho_m(x^m, y^m)$ . In the MB case one can express these reduced density matrices (using Feynman-Kac) as Wiener integrals of functionals  $\rho_m(\omega^m)$  where the curves  $\omega_j$  start at  $x_j$  and end at  $y_j$ . The sequence of functionals  $\rho_m(\omega^m)$  satisfies equations similar to (\*) and therefore low activity quantum gases with MB statistics can be treated like classical gases.

For Bose-Einstein or Fermi-Dirac statistics one can express the reduced density matrices as Wiener integrals over functionals of curves of various length connecting the the positions  $x^m$  and the positions  $y^m$  after permutations (some minus signs corresponding to odd permutations must be inserted in the Fermi-Dirac case). The combinatorics involved is not really hard but, if one assumes integrable pair potentials, the low activity convergence of the formulas force Ginibre to requires positivity of the pair potentials, i.e., repulsive interactions! It appears that this result cannot be improved by the methods of paper I.

We have summarized above Ginibre's paper I. Paper II proves a cluster property of the reduced density matrices (long distance decorrelation).

The restriction to positive potentials for quantum statistics in paper I is of course unsatisfactory. This problem is remedied in paper III where one assumes the existence of a

rotationally symmetric hard core for the pair potential (both for MB and quantum statistics). As a result it will be possible to remove of the positive potential condition. In the hard core situation one again introduces an operator  $K$  on functionals  $\rho_m(\omega^m)$  to study reduced density matrices. With a change in the definition of the norm of the Banach space on which  $K$  acts in (\*), one achieves that  $K$  is again a bounded operator and the results of I and II for MB statistics are recovered. For quantum statistics, the hard core condition means that each particle effectively interacts with only a finite number of other particles. Therefore negative (attractive) values of the pair potential are allowed for the study of low activity quantum gases and for proving the cluster property of reduced density matrices.

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### 3 Strichartz inequalities

by TERENCE TAO (Los Angeles)

A fundamental feature of wave equations is that of *dispersion*: the different frequency components of a wave move in different directions, and as time passes, the amplitude of the waves decays to zero, even if other features of the wave such as the total energy remain conserved. Mathematically, one can model wave phenomena by considering solutions to partial differential equations such as the *linear Klein-Gordon equation*

$$-\partial_{tt}u + \Delta u = m^2u \quad (3.1)$$

(where  $u : \mathbb{R} \times \mathbb{R}^3 \rightarrow \mathbb{R}$  is a function of time  $t$  and space  $x$ ,  $\Delta = \sum_{j=1}^3 \partial_{x_j x_j}$  is the spatial Laplacian), and  $m \geq 0$  is a constant, or the *linear Schrödinger equation*, which we will normalize here as

$$i\partial_t u + \frac{1}{2}\Delta u = 0 \quad (3.2)$$

where the unknown field  $u : \mathbb{R} \times \mathbb{R}^3 \rightarrow \mathcal{C}$  is now complex-valued. One can also consider nonlinear variants of these equations, such as the *nonlinear Klein-Gordon equation*

$$-\partial_{tt}u + \Delta u = m^2u + \lambda|u|^{p-1}u \quad (3.3)$$

and the *nonlinear Schrödinger equation*

$$i\partial_t u + \frac{1}{2}\Delta u = \lambda|u|^{p-1}u \quad (3.4)$$

where  $\lambda = \pm 1$  and  $p > 1$  are specified parameters; however, for short times or small data one can hope that perturbative methods can allow one to understand these nonlinear equations once one obtains a sufficient understanding of their linearized counterparts, including their dispersive behavior. There are countless other further variations (both linear and nonlinear) of these sorts of equations, collectively referred to as *dispersive equations*, but for this discussion we shall restrict attention to these four. To avoid technicalities, we will always assume that solutions are smooth enough, and have sufficient decay at spatial infinity, that all formal computations can be justified.

One can formalize the decay phenomenon mentioned at the beginning of this section by *dispersive inequalities* that assert, roughly speaking, that if a solution  $u$  to one of these equations is sufficiently localized in space at an initial time such as  $t = 0$ , then it will decay as  $t \rightarrow \infty$  when measured in suitable function space norms, such as the  $L_x^\infty(\mathbb{R}^3)$  norm.

For instance, from the formula for the fundamental solution to (3.2), one can represent a solution  $u$  to (3.2) explicitly in terms of the initial data  $u(0)$  by the formula

$$u(t, x) = \frac{1}{(2\pi it)^{3/2}} \int_{\mathbb{R}^3} e^{-i|x-y|^2/2t} u(0, y) dy$$

for all  $t \neq 0$  and  $x \in \mathbb{R}^3$ , where the quantity  $(2\pi it)^{3/2}$  is defined using a suitable branch cut. From the triangle inequality, this immediately gives the dispersive inequality

$$\|u(t)\|_{L_x^\infty(\mathbb{R}^3)} \leq \frac{1}{(2\pi|t|)^{3/2}} \|u(0)\|_{L_x^1(\mathbb{R}^3)}, \quad (3.5)$$

and so if the solution is initially spatially localized in the sense that the  $L^1$  norm  $\|u(0)\|_{L_x^1(\mathbb{R}^3)}$  is finite, then the solution  $u(t)$  decays uniformly to zero as  $t \rightarrow \pm\infty$ . A similar (but slightly more complicated) dispersive inequality can also be obtained for solutions to the linear Klein-Gordon equation (3.1).

On the other hand, from the pointwise mass conservation law

$$\partial_t |u|^2 = \sum_{j=1}^3 \partial_{x_j} \text{Im}(\bar{u} \partial_{x_j} u) \quad (3.6)$$

for solutions to (3.2), one can easily derive  $L^2$  conservation:

$$\|u(t)\|_{L_x^2(\mathbb{R}^3)} = \|u(0)\|_{L_x^2(\mathbb{R}^3)}. \quad (3.7)$$

In particular, the  $L^2$  norm of the solution will stay constant in time, rather than decay to zero. To reconcile this fact with the dispersive estimate, we observe that solutions to dispersive equations such as (3.2) spread out in space as time goes to infinity, allowing the  $L^\infty$  norm of such a solution to go to zero even while the  $L^2$  norm stays bounded away from zero.

Conservation laws such as (3.7) and dispersive inequalities such as (3.5) can already be used, in conjunction with standard inequalities such as the Hölder or Minkowski inequalities, to control solutions to nonlinear equations such as (3.3) or (3.4) and to obtain local and global existence, decay and scattering results for such equations; see for instance this paper of Lin and Strauss [1]. However, the arguments were *ad hoc* in nature, and not always optimal with respect to dependence on various parameters such as the dimension  $d$ , the nonlinearity exponent  $p$ , or the regularity  $s$  of the initial data. It was the landmark work of Ginibre and Velo [2], [3] that found a systematic, efficient and conceptually simple framework in which to exploit conservation and dispersive inequalities to analyze

nonlinear dispersive equations. Their starting point was the work of Strichartz [4], who had adapted then-recent work [5] on the Fourier restriction problem to obtain spacetime estimates for solutions to linear dispersive equations, a typical one of which is

$$\|u\|_{L_t^4 L_x^4(\mathbb{R} \times \mathbb{R}^3)} \leq C \|u(0)\|_{L_x^2(\mathbb{R}^3)}$$

for solutions to (3.2), basically by cleverly interpolating between the two estimates (3.5), (3.7). Ginibre and Velo realized that the proof method in fact yielded a larger and more flexible family of *mixed* spacetime norm estimates such as

$$\|u\|_{L_t^q L_x^r(\mathbb{R} \times \mathbb{R}^3)} \leq C \|u(0)\|_{L_x^2(\mathbb{R}^3)}$$

for  $\frac{1}{q} + \frac{1}{r} = \frac{1}{2}$  and  $2 \leq q < \infty$ ; such estimates are now known as *generalised Strichartz estimates*, or often more succinctly as *Strichartz estimates*. Furthermore, these Strichartz estimates were perfectly suited to construct and then estimate solutions to nonlinear dispersive equations by converting them (via the Duhamel formula) to integral equations that can be solved via the contraction mapping principle in mixed spacetime norm spaces such as  $L_t^q L_x^r(\mathbb{R} \times \mathbb{R}^3)$ . This basic scheme is now the standard foundation for the modern theory of such equations, although over time the function spaces and estimates used have become more sophisticated. For instance, in Ginibre and Velo's own work [3], it became advantageous to replace Lebesgue and Sobolev spaces with Besov spaces; this paper pre-saged the more systematic use of Littlewood-Paley decompositions that are now standard in the field.

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## 4 Ginibre’s contribution to random matrix theory

by EUGENE KANZIEPER (Holon)

The paper on “Statistical Ensembles of Complex, Quaternion, and Real Matrices” [1] – Ginibre’s only work on random matrix theory – has unique standing in his legacy. Published at the dawn of Ginibre’s career in 1965 (almost in parallel with a series of three papers [2] related to his PhD/DSc research [3]), this elegant piece of mathematics remained largely unattended for almost three decades to become Ginibre’s second most cited work, see Fig. 4.1.

Ginibre’s paper [1] focusses on three ensembles of asymmetric Gaussian random matrices – nowadays often coined as GinOE, GinUE, and GinSE – that were derived from the celebrated Gaussian Orthogonal (GOE), Gaussian Unitary (GUE), and Gaussian Symplectic (GSE) random matrix ensembles [4] by relaxing a Hermiticity constraint. By Ginibre’s own account [5], “*as regards my original motivation, it was pure curiosity and I had no application in mind. The problem appears among the open questions in the last section of the last paper by Dyson and Mehta [6] and I found it both natural and exciting.*”

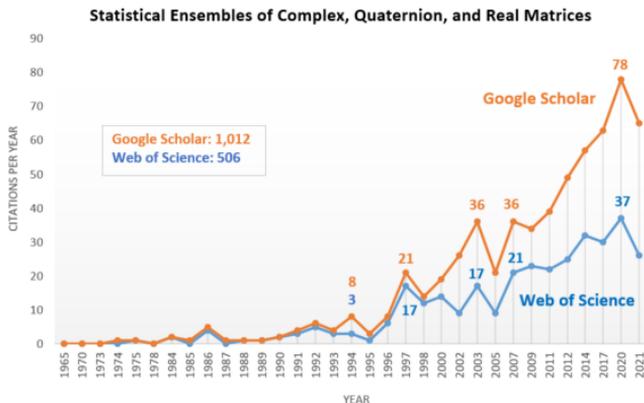


Figure 4.1: Citation record for Ginibre’s paper since 1965. Note a citation spike in 1997. Retrieved on August 30, 2021.

Three non-Hermitian descendants of GOE, GUE and GSE are defined by the Gaussian probability measure

$$d\mu^{(\beta)}(\mathcal{S}) = e^{-\text{tr} \mathcal{S}\mathcal{S}^\dagger} d\mu_L^{(\beta)}(\mathcal{S}), \quad (4.1)$$

where  $d\mu_L^{(\beta)}(\mathcal{S})$  represents a family of natural flat measures corresponding to the spaces  $\mathbb{T}_\beta$  on which the matrices  $\mathcal{S}$  vary:  $\mathbb{T}_1(n)$ ,  $\mathbb{T}_2(n)$ , and  $\mathbb{T}_4(n)$  are spanned by all  $n \times n$  matrices with real (GinOE,  $\beta = 1$ ), complex (GinUE,  $\beta = 2$ ), and real quaternion (GinSE,  $\beta = 4$ ) entries, respectively. The violated Hermiticity,  $\mathcal{S}^\dagger \neq \mathcal{S}$ , brings about the two major phenomena: (i) complex-valuedness of the random matrix spectrum and (ii) splitting the random matrix eigenvectors into a bi-orthogonal set of left and right eigenvectors. Ginibre's work deals with the spectral statistics only.

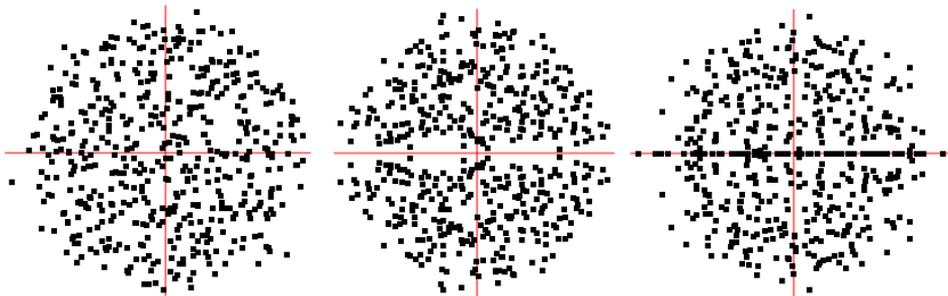


Figure 4.2: Plots of complex eigenvalues demonstrating different spectral patterns in GinUE (left panel), GinSE (middle panel), and GinOE (right panel). Numerical simulations [7].

The three matrix models exhibit profound differences in their spectral patterns, see Fig. 4.2. Random eigenvalues are (i) scattered almost uniformly in GinUE, (ii) depleted from the real axis in GinSE, and (iii) accumulated along the real axis in GinOE. While these peculiarities were realized by Ginibre in his work, their quantitative description had not been always complete. For this reason, below we comment on both Ginibre's results and complementary contributions of other authors. In doing so, we adopt the order [1] of increasing difficulty.

**GinUE** (Section 1).—The joint probability density function (j.p.d.f.) of  $n$  complex eigenvalues  $(z_1, \dots, z_n)$  of a random matrix  $\mathcal{S} \in \mathbb{T}_2(n)$  with complex valued entries was found to be of the form [1]

$$P_n^{(2)}(z_1, \dots, z_n) = \left( \pi^n \prod_{\ell=1}^n \ell! \right)^{-1} \prod_{\ell_1 > \ell_2 = 1}^n |z_{\ell_1} - z_{\ell_2}|^2 \prod_{\ell=1}^n e^{-z_\ell \bar{z}_\ell}. \quad (4.2)$$

It describes a determinantal point process on  $\mathbb{C}$  with the scalar kernel

$$K_n^{(2)}(z, z') = \frac{1}{\pi} e^{-\frac{1}{2}|z|^2} e^{-\frac{1}{2}|z'|^2} \sum_{\ell=0}^{n-1} \frac{(z \bar{z}')^\ell}{\ell!}. \quad (4.3)$$

Consequently, the GinUE  $p$ -point correlation function admits a determinantal representation

$$R_p^{(2)}(z_1, \dots, z_p; n) = \det \left( K_n^{(2)}(z_k, z_\ell) \right)_{k, \ell=1}^p. \quad (4.4)$$

These results served as the starting point for studying various limiting spectral distributions as  $n \rightarrow \infty$ . In particular, asymptotic analysis of the mean spectral density

$$R_1^{(2)}(z; n) = \frac{\Gamma(n, |z|^2)}{\pi \Gamma(n)} \quad (4.5)$$

led Ginibre to advocate emergence of the circular law [8]. Closing Section 1, Ginibre pointed out that the j.p.d.f. (4.2) can be interpreted as a distribution of the positions of charges of a two-dimensional Coulomb gas confined by an harmonic oscillator potential  $U(z) = |z|^2/2$ , at the inverse temperature  $1/T = 2$ .

**GinSE** (Section 2).—The spectrum of a random matrix  $\mathcal{S} \in \mathbb{T}_4(n)$  drawn from GinSE consists of  $n$  pairs of complex conjugated eigenvalues  $(z_1, \bar{z}_1, \dots, z_n, \bar{z}_n)$ . While Ginibre managed to determine their joint probability density function

$$P_n^{(4)}(z_1, \dots, z_n) = ((2\pi)^n n! \prod_{\ell=1}^n (2\ell - 1)!)^{-1} \prod_{\ell_1 > \ell_2 = 1}^n |z_{\ell_1} - z_{\ell_2}|^2 |z_{\ell_1} - \bar{z}_{\ell_2}|^2 \times \prod_{\ell=1}^n |z_\ell - \bar{z}_\ell|^2 \exp(-z_\ell \bar{z}_\ell), \quad (4.6)$$

where the factor  $\prod_{\ell=1}^n |z_\ell - \bar{z}_\ell|^2$  in (4.6) is directly responsible for the depletion of eigenvalues along the real axis, he did not report any progress on calculating spectral correlation functions, plainly noticing that their determination “*appears to be considerably more difficult than in the complex case, and the electrostatic interpretation [...] breaks down.*” A year later, this task was accomplished by Mehta and Srivastava [9], see also Ref. [4], who essentially discovered that GinSE eigenvalues form a Pfaffian point process on  $\mathbb{C}$ . A physical analogy of GinSE with a two-dimensional Coulomb gas appears to be much less transparent; it has been discussed much later in Ref. [10].

**GinOE** (Section 3).—Algebraic structures behind random real asymmetric matrices  $\mathcal{S} \in \mathbb{T}_1(n)$  appeared to be the most challenging and least studied in Ginibre’s paper. The difficulties faced in the analysis of the GinOE can be attributed to the fact that its, generically complex, spectrum may contain a finite fraction of real eigenvalues; the remaining complex eigenvalues always form complex conjugated pairs. This very peculiar feature of GinOE – accumulation of eigenvalues along the real axis – can conveniently be accommodated by dividing the entire space  $\mathbb{T}_1(n)$  spanned by all real  $n \times n$  matrices  $\mathcal{S} \in \mathbb{T}_1(n)$  into  $(n + 1)$  mutually exclusive sectors  $\mathbb{T}_1(n/k)$  associated with the matrices  $\mathcal{S}_k \subset \mathcal{S}$  having exactly  $k$  real eigenvalues, such that  $\mathbb{T}_1(n) = \bigcup_{k=0}^n \mathbb{T}_1(n/k)$ . Consequently, the j.p.d.f. of all  $n$  eigenvalues of  $\mathcal{S} \in \mathbb{T}_1(n)$  is contributed by the partial j.p.d.f.’s:

$$P_n^{(1)}(w_1, \dots, w_n) = \sum_{k=0}^n P_{\mathcal{S} \in \mathbb{T}(n/k)}(w_1, \dots, w_n). \quad (4.7)$$

Notice that half of the sets  $\mathbb{T}_1(n/k)$  are empty: this occurs whenever  $n$  and  $k$  are of different parity. Due to significant technical hurdles, Ginibre only succeeded in finding explicit expression for the simplest partial j.p.d.f. in the sector  $\mathbb{T}_1(n/n)$  where all eigenvalues are real; not surprisingly, it coincided with the one for the GOE [4].

In entire generality, partial j.p.d.f.'s were determined by Lehmann and Sommers [11] a quarter of a century after Ginibre's work, and rediscovered by Edelman [12] a few years later. For completeness, we quote their result for the  $k$ -th partial j.p.d.f. ( $0 \leq k \leq n$ ):

$$\begin{aligned}
 P_{\mathcal{S} \in \mathbb{T}(n/k)}(\lambda_1, \dots, \lambda_k; z_1, \dots, z_\ell) & \\
 &= \frac{2^{\ell-n(n+1)/4}}{i^\ell k! \ell! \prod_{j=1}^n \Gamma(j/2)} \prod_{i>j=1}^k |\lambda_i - \lambda_j| \prod_{j=1}^k \exp(-\lambda_j^2/2) \\
 &\times \prod_{j=1}^k \prod_{i=1}^{\ell} (\lambda_j - z_i)(\lambda_j - \bar{z}_i) \prod_{i>j=1}^{\ell} |z_i - z_j|^2 |z_i - \bar{z}_j|^2 \\
 &\times \prod_{j=1}^{\ell} (z_j - \bar{z}_j) \operatorname{erfc} \left( \frac{z_j - \bar{z}_j}{i\sqrt{2}} \right) \exp \left( -\frac{z_j^2 + \bar{z}_j^2}{2} \right). \tag{4.8}
 \end{aligned}$$

Here, the parameterisation  $(w_1, \dots, w_n) = (\lambda_1, \dots, \lambda_k; z_1, \bar{z}_1, \dots, z_\ell, \bar{z}_\ell)$  was used to indicate that the spectrum is composed of  $k$  real and  $\ell$  pairs of complex conjugated eigenvalues so that  $k + 2\ell = n$ . The above j.p.d.f. is supported for  $(\lambda_1, \dots, \lambda_k) \in \mathbb{R}^k$ ,  $(\operatorname{Re} z_1, \dots, \operatorname{Re} z_\ell) \in \mathbb{R}^\ell$ , and  $(\operatorname{Im} z_1, \dots, \operatorname{Im} z_\ell) \in (\mathbb{R}^+)^{\ell}$ .

In spite of this tremendous progress summarized in a somewhat complicated Eq. (4.8), it took another decade and a half to establish that the GinOE eigenvalues form a Pfaffian point process and explicitly determine [13, 14] the Pfaffian representations of correlation functions for real-real, complex-complex and real-complex eigenvalues.

□

An outside reader of this brief note may be misled into thinking that, after publications [13, 14], a long and exciting journey started by Ginibre in 1965 has come to an end. Luckily, this is by no means the case. Born out of mathematical curiosity and lacking immediate physical applications at the time, Ginibre's random matrices as well as their by-now-numerous deformations have surfaced, by E. Wigner's "miracle of the appropriateness", in various disciplines and systems – quantum chaology and mathematical statistics, statistical and condensed matter physics, quantum chromodynamics, complex biological and neural networks, the theory of random functions and more. Having come a long way from oblivion to a flourishing research field, non-Hermitian random matrices continue to challenge ever new generations of mathematical and theoretical physicists.

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# Time's Arrow

## Scientific anniversaries

1921.

Albert Einstein won the Nobel Prize “for his services to theoretical physics, and especially for his discovery of the law of the photoelectric effect.” (Actually awarded in 2022.)

1971.

Aguilar, Balslev, and Combes introduced the method of complex scaling for resonances.

[Martin Gutzwiller published his trace formula](#)

[Kenneth Wilson introduced the renormalization group.](#)

## 2021 Nobel Prize

Giorgio Parisi has won the Nobel Prize in Physics “for the discovery of the interplay of disorder and fluctuations in physical systems from atomic to planetary scales.” (The prize is shared with Syukuro Manabe and Klaus Hasselmann for their work modeling the Earth’s climate.)

## Recent personal celebrations

Ingrid Daubechies [was profiled in the \*New York Times\*.](#)

Masamichi Takesaki [had an 88th birthday conference.](#)

## Lost luminaries

[Derek Robinson](#), 2 September, 2021.

[Steven Weinberg](#), 23 July, 2021.

Readers are encouraged to send items for “Time’s Arrow” to [bulletin@iamp.org](mailto:bulletin@iamp.org).

## Treasurer's Report

IAMP currently operates a Euro account at a bank in Bielefeld (Germany) and a US Dollar account at a bank in Birmingham (USA). The following table details the assets in these accounts (as of June 30, 2021).

Account	Balance	Currency	Euro equivalent
Bielefeld Checking	85.367	EUR	85.367
Bielefeld Savings	86.000	EUR	86.000
US Checking	2.215	USD	1.866
US Savings	19.751	USD	16.642
TOTAL Checking:			87.233
TOTAL Savings:			102.642
TOTAL (EUR):			189.875
As of Jun 30, 2006:			112,598
As of Jun 30, 2009:			96,963
As of Jun 30, 2012:			125,545
As of Jun 30, 2015:			130,978
As of Jun 30, 2018:			117,068

The sharp increase in assets in the last funding period stems from the fact that prizes had not yet been awarded and that a number of conferences had to be postponed or even cancelled due to the Covid situation (see below).

In the current situation there is no income from interest on our savings accounts, thus funds have not been shifted from the checkings to the savings account in Bielefeld. Since IAMP is an association founded under the Swiss Civil Code, we are restricted in the ways of investing our capital and there is little room for improvement here.

IAMP's main source of income are our membership dues. We welcome donations and thank all members who continue to contribute in this way. At the moment, we have about 700 members (ordinary ones, most of whom are in good standing, lifetime and associate members, as well as members paying a reduced fee as first year members or members with

economical hardship or from developing countries). A number of members experienced difficulties paying dues via the database this year. The EC is working on a solution.

IAMP gratefully acknowledges generous support from the Daniel Iagolnitzer Foundation for the Henri Poincaré prize and from Springer Nature Switzerland AG for the Early Career Award. This year, the Henri Poincaré prize was awarded to Rodney J Baxter, Demetrios Christodoulou, Yoshiko Ogata, and Jan Phillip Solovej. The Early Career Award was awarded to Amol Aggarwal.

IAMP also gratefully acknowledges Dietmar Kähler as well as our secretary Michael Loss for the maintenance of the website and the database, and thanks Günther Stolz who has been managing the US Dollar account for years.

The following table details IAMP's financial activities 2018-2021:

Activity	2018-2021 (EUR)	2018/19 (USD)
Dues	35712	2268
Interest	22	74
DIF	93000	N/A
Springer/AHP/EMS	25,000	N/A
Total Bank/CC fees	-3,444	-8
Internet Hosting	-306	N/A
Conference support	-29181	-5014
ECA Prize '15	-4447	N/A
HPP/ICMP18 Support	-59716	0
<b>Total Gain/Loss</b>	<b>56640</b>	<b>- 2680</b>

The credit card system, by which members can pay their dues or make donations online on IAMP's web-page, currently costs roughly 450 Euros per year and an additional 450 Euros Agio per year. The last item could be avoided if more members were to wire their dues directly to our Bielefeld account. IAMP strongly encourages European members to do so since SEPA transactions within Europe cost as little as within one country.

While IAMP provides no regular funds for our main conference, the ICMP, most of IAMP's expenses are conference support. IAMP supports conferences in mathematical physics with 1 – 3 KEuros on a competitive basis; funding requests are evaluated by a three-member committee and the final decision is made by IAMP's Executive Committee

(EC). Given IAMP's comfortable financial situation, the EC had decided in 2018 to slightly reduce its capital by roughly 3 KEuro per year in order to sponsor more conferences. We plan to continue doing so, hoping that the Covid situation will continue to unwind and the community continues to make strong proposals. A list of conferences supported in the last funding period is found in the subsequent tables. Due to the Covid situation, a number of conferences were shifted or even cancelled, thus leaving us with a surplus in the years 2020/21. According to the EC's meeting in 2021, part of this surplus as well as further funds from our checking accounts shall be invested in a new database and website structure.

Conference [Location] (2018)	Amount
Mathematical Challenges in Quantum Mechanics [Rome]:	3,000 EUR
Random Matrices, Integrability and Complex Systems [Yad Hashmona, IS]	1,500 EUR
Summer school (Springer conference) "Current Topics in Mathematical Physics" [Fields Institute Toronto, Canada]	3,714 USD
Spectral Theory and Mathematical Physics [Santiago, Chile]	1.500 EUR
EMS-IAMP summer school "Universality in probability theory and statistical mechanics" [Rome]	3,000 EUR
Results in Contemporary Mathematical Physics [Santiago, Chile]	1,500 EUR
Quantum Roundabout [Nottingham, UK]	500 EUR
Mathematics of Non-equilibrium Statistical Mechanics [Montreal]	1,500 EUR
Progress and visions in quantum theory in view of gravity [MPI Leipzig]	1,000 EUR
Total conference support:	13,500 EUR + 3,714 USD
Related to ICMP 2018 (Montreal) :	
DIF Travel Support	6.654 EUR

Conference [Location] (supported in 2019)	Amount
Random Physical Systems [Puerto Natales, Chile]	1,000 EUR
Mathematics of interacting QFT models [York, UK]	1,500 EUR
From Quantum to Classical (Springer conference) [CIRM, France]	1,000 EUR
Stochastic and Analytic Methods in Mathematical Physics [Yerevan, Armenia]	1,750 EUR
Operators, Functions, and Systems of Mathematical Physics [Baku, Azerbaijan]	1,000 EUR
Quantissima in the Serenissima III [Venice]	1,000 EUR
QMath 14 [Aarhus, Denmark]	1,000 EUR
Mathematical Physics at the Crossings [Virginia Tech]	656 EUR + 600 USD
Summer school (Springer conference) "Quantum Random Walks, Quantum Graphs and their spectra in Mathematics, Computer Science and Physics" [Como, Italy/Germany]	1,250 EUR
Non-commutative Manifolds and their Symmetries [Scalea, Italy/Germany]	1,000 EUR
The Analysis of Large Quantum Systems [CIRM, France]	1,000 EUR
Spring School in Analysis and Mathematical Physics [Santiago, Chile]	1,000 EUR
Great Lakes Mathematical Physics meeting [Oberlin College, USA]	700 USD
<b>Total</b>	<b>13,156 EUR + 1,300 USD</b>
Conference [Location] (supported in 2020)	Amount
CIRM SMF Conference	1.000 EUR
Meeting ISC for ICMP 2021	798 EUR
IAMP Zoom Seminar	588 EUR
<b>Total</b>	<b>2.386 EUR</b>

# News from the IAMP Executive Committee

## New individual members

IAMP welcomes the following new members

1. PROFESSOR STEFANO MANCINI, Università di Camerino, Italy
2. MR. DIAA EL-RAHMAN RAYAN, Central Metallurgical Research & Development Institute (CMRDI), Egypt
3. DR. TLAS TAMER, American University of Beirut, Lebanon
4. DR. FABIO PIZZICCHILLO, CEREMADE, Université Paris Dauphine, France
5. DR. YORGO SENIKOGLU, Koc University, Turkey
6. DR. SALMAN LAHBABI, ENSEM, UHII and UM6P, Morocco
7. DR. SHREECHARAN TANGIRALA, ICFAI Foundation for Higher Education, Hyderabad, India
8. DR. RANJANI S SREE, IcfaiTech, Faculty of Science and Technology, ICFAI foundation for higher education, Hyderabad, India
9. DR. YULIA MESHKOVA, St. Petersburg State University, St. Petersburg, Russia

## Recent conference announcements

### Quantum Trajectories Fall School

October 18-22, 2021, Toulouse, France.

### The 2021 QGraph Network meeting

December 8-9, 2021, Stockholm, Sweden

### Workshop on Spin Glasses at Swissmap Research Station

February 27- March 4, 2022, Les Diablerets, Switzerland

## Open positions

### **Postdoctoral position in spectral geometry of graphs and quasicrystals.**

The Department of Mathematics, Stockholm University, is looking for a postdoc in mathematics/mathematical physics to work with Prof. Pavel Kurasov. The subject of the studies will be crystalline measures, quantum graphs and quasicrystals. This project is based on the recent discovery that spectra of Laplacians on metric graphs lead to exotic one-dimensional measures being Fourier quasicrystals. We shall try to understand the connection to sphere packing and Fourier interpolation.

The position is for two years and is funded from Wallenberg Foundation (Sweden). Please contact Pavel Kurasov for details at [kurasov@math.su.se](mailto:kurasov@math.su.se).

For an updated list of academic job announcements in mathematical physics and related fields visit

<http://www.iamp.org/page.php?page=pagepositions>

**Michael Loss** (IAMP Secretary)

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