In Memoriam: Freeman Dyson (1923–2020)

George E. Andrews, Jürg Fröhlich, and Andrew V. Sills

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*.

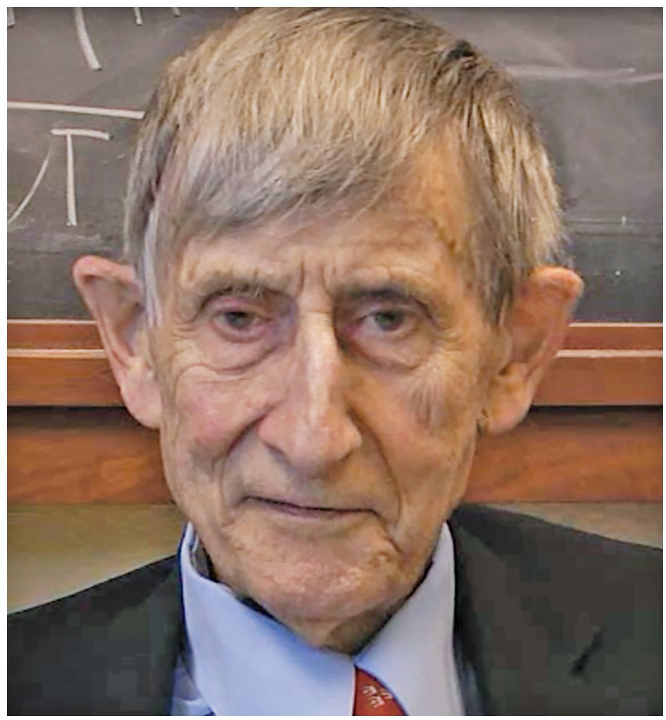

Figure 1.

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 Besikovich, the latter apparently having the strongest influence on his early development and scientific style. Dyson's knowledge of Russian came in handy, as Besikovich 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.

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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 [Dys07, 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.... [F]oxes 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 [Dys96, 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 lifecycle 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[1]) 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. Dyson wrote [Dys96, 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

[1]*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.*

> 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 [Dys79]. 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 [Dys79, 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 [Dys49], 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 [Dys44], 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 [Ram00]:

$$p(5n+4) \equiv 0 \pmod 5, \tag{1}$$
$$p(7n+5) \equiv 0 \pmod 7, \tag{2}$$

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 [ASD54].

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

$$p(11n+6) \equiv 0 \pmod{11}. \tag{3}$$

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 [AG88]. 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 [Rog94], 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 $|q| < 1$,

$$\sum_{n=0}^{\infty} \frac{q^{n^2}}{(1-q)(1-q^2)\cdots(1-q^n)} = \prod_{\substack{j\geq 1 \\ j\equiv \pm 1 \,(\mathrm{mod}\ 5)}} \frac{1}{1-q^j} \tag{4}$$

and

$$\sum_{n=0}^{\infty} \frac{q^{n(n+1)}}{(1-q)(1-q^2)\cdots(1-q^n)} = \prod_{\substack{j\geq 1 \\ j\equiv \pm 2 \,(\mathrm{mod}\ 5)}} \frac{1}{1-q^j}. \tag{5}$$

Dyson delighted in identities of the "Rogers–Ramanujan type" and in fact Dyson's very first published paper [Dys43] was on three modulus 7 analogs of (4) and (5) that were also originally due to Rogers [Rog94].

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 [Dys88b] that his personal favorite of these identities was

$$\sum_{n=0}^{\infty} q^{n(n+1)} \frac{\prod_{k=1}^{n}(1+q^k+q^{2k})}{\prod_{h=1}^{2n+1}(1-q^h)} = \prod_{j=1}^{\infty} \frac{1-q^{9j}}{1-q^j}. \tag{6}$$

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 [Dys49, Dys12].

1.3.3. The Dyson conjecture. In the first of a series of papers on the statistical theory of energy levels of complex systems [Dys62a], Dyson introduced what came to be known as the *Dyson conjecture*: the constant term in the expansion of the product

$$\prod_{1\leq i\neq j\leq n} \left(1-\frac{z_i}{z_j}\right)^{a_i} \tag{7}$$

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 [ZB85]. 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" [Dys72]. 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 unfailing 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

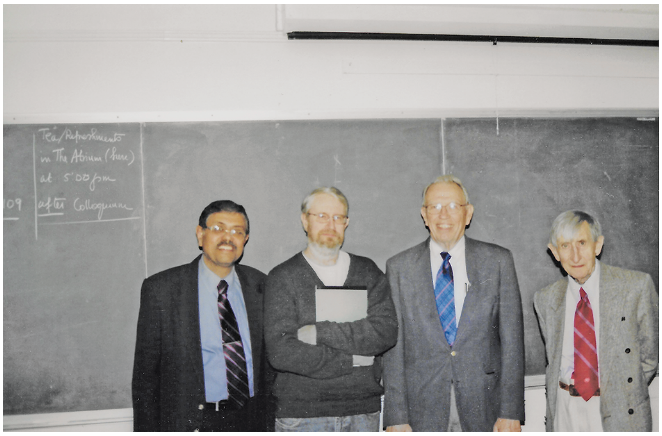

Figure 2.

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 spectacular 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 [Dys44] 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.

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 a not a kth 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* [Dys49] 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* [Dys96] 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 [Dys96]. 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.

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

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

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 $R \to \infty$, of the number of lattice points in a circle of radius R,

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

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,

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

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

$$\begin{aligned} &N(R;\alpha) \\ &\quad = \#\left\{(i,j) \in \mathbb{Z}^2 \mid \sqrt{(i+\alpha_1)^2 + (j+\alpha_2)^2} \le R\right\}, \\ &\qquad\qquad \alpha = (\alpha_1, \alpha_2), \quad 0 \le \alpha_1, \alpha_2 \le 1, \end{aligned}$$

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 $x \to \pm\infty$. We showed that for all $\epsilon > 0$,

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

and

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

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

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

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

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

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:

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

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,

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

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 Φ 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.

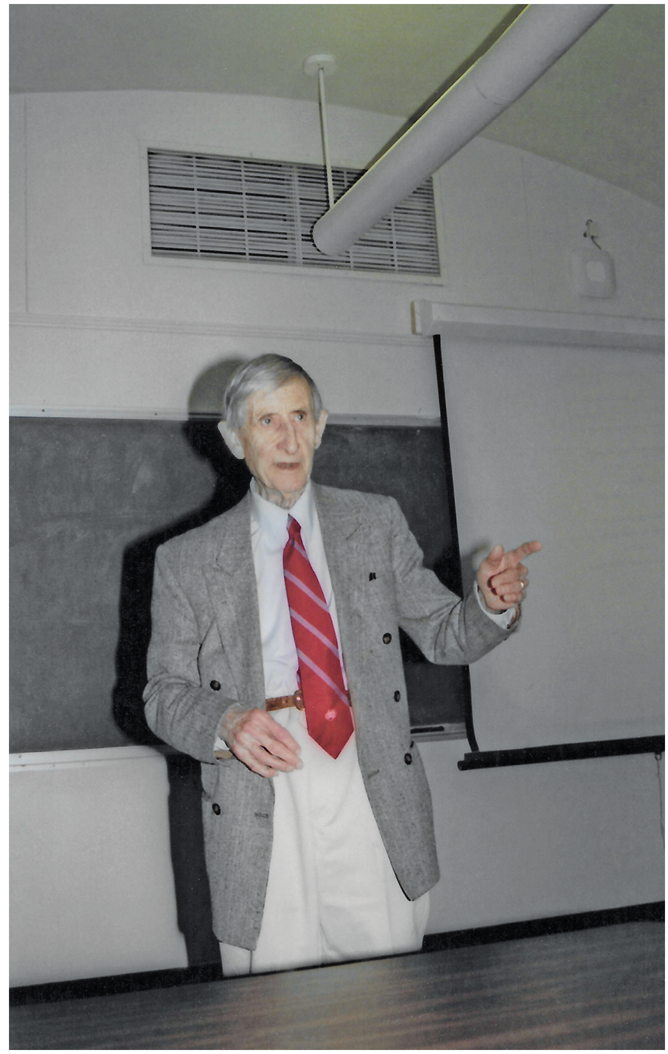

Figure 3.

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 [Dys49]. 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 πR^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 $T \to \infty$.

Let me conclude with one of my favorite Dyson quotes, from his wonderful book, *Infinite in All Directions* [Dys88a, 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 sine 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 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 [Dys62b]. 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 [Wig58] 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 [Gau61] and Mehta [Meh60]. 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 foreward 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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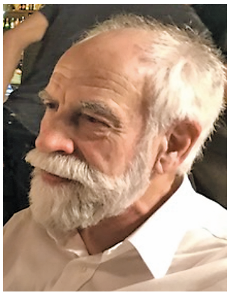

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