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.