Freeman Dyson

Born 1923. Theoretical physicist.
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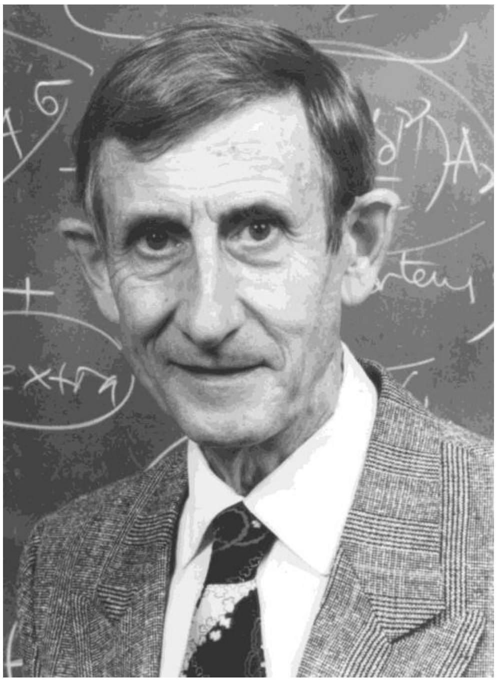

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1. A Profile of Freeman Dyson

Freeman Dyson.

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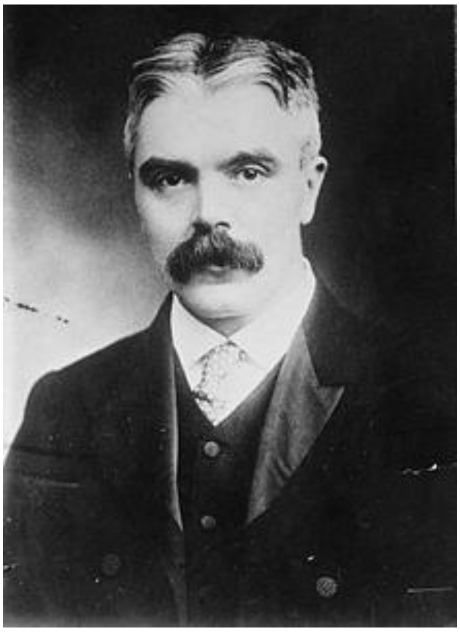
Freeman Dyson loves the metaphor that divides scientists into two groups: Birds, who look down upon everything and have a God's-eye view of the world, and frogs, who spend their time in the mud. The renowned Princeton physicist calls himself a frog. "I'm not against the first group, but they take an exalted view of science. Frogs typically enjoy exploring things locally and developing skills."

The brilliant frog has spent his lifetime developing skills in disciplines ranging from nuclear engineering to science writing. But he is probably better known to the digerati as the father of computer consultant extraordinaire Esther Dyson. Nonetheless, the slightly built Freeman Dyson is a giant among scientists, largely due to his talents as a writer.

His work as an interpreter of science for the general public has brought many rewards, the first of which is a body of work that includes the autobiographical "Disturbing the Universe" (1979), a meditation on nuclear disarmament, "Weapons and Hope" (1984) and a road map for the most important technologies of the coming century, "The Sun, the Genome, and the Internet" (1999). Dyson's writing has been widely praised for its poetry and artistry, and in 1996 he received the Lewis Thomas Prize, a Rockefeller University-sponsored award that recognizes scientists for their artistic achievements. The key to Dyson's life is that he has never been one to shy away from new experiences. Recalling that his real life began at age 45, when he published his first book, Dyson mused in 1992: "So long as you have courage and a sense of humor, it is never too late to start life afresh."

Born in 1923 in Crowthorne, Berkshire, in south England, Dyson grew up as "a mathematically inclined child born into a musical family ... without pretensions to scientific fame." The Dyson home was rich in cultural and artistic influences. His father, the composer/conductor Sir George Dyson, eventually became the director of the British Royal College of Music in London. Dyson's mother, Lady Mildred, a lawyer by training, was intensely interested in literature and language. Dyson recalls that his parents expressed their affection by encouraging him to explore arts and culture; they were in their early 40s when they started their family: "[Being raised by my mother and father] was more like being with grandparents than parents, but they certainly loved us in their own fashion. It was more intellectual than physical."



Missing from Dyson's boyhood home were scientific influences, until the family adopted one -- Sir Frank Dyson (right), Astronomer Royal. He was no relation to the Berkshire Dysons, but he was from the same part of Yorkshire as Sir George Dyson. In his 1992 book "From Eros to Gaia," Freeman Dyson recalls that the breakfast table discussions between his father and other relatives about Sir Frank's exploits heavily influenced his early interest in astronomy and spurred him to take up his pen as a 9-year-old and write a novel based on the activities of Sir Frank.

The unfinished manuscript of "Sir Phillip Robert's Erolunar Collision" is at once a snapshot of the astronomical events of 1931 and a window into a future project that would occupy a good portion of Dyson's professional ruminations: space travel. It's also a good satire of large-scale science projects. In 1931, the orbit of asteroid Eros was going to pass close to the Earth, providing an important opportunity for astronomers to get an accurate reading of the distance between the Earth and the sun. In the incomplete novel, Sir Phillip, director of the British South African Astronomical Society and a character based on Sir Frank, successfully predicts an Erolunar collision by calculating the orbit of Eros 10 years and 285 days in advance.

Dyson turned to one of his science-fiction heroes to help the plot along. The characters decide to rewrite the mission described in Jules Verne's "From Earth to the Moon and a Trip Round It" to change Eros' destination to go directly to the moon and land the astronomers on its surface to witness the collision. Then reality crept into Dyson's fiction: The astronomical society needed money. Sir Phillip spends the final pages of the manuscript trying to raise funds and design a spacecraft. The novel ends before Sir Phillip can leave Earth.

The unfinished novel proved prophetic. That obviously bright and perceptive 9-year-old grew into a scientist who would consult for NASA and work on numerous government-related projects. In the end, the grown scientist would come away with an opinion that was eerily close to that of the aspiring science-fiction author: that large, bureaucratic-run scientific endeavors often exist to justify their own importance. Dyson assessed his early views in "From Eros to Gaia:" "These observations show that the practice of science has changed less than one might have expected between 1933 to 1991."

Dyson's adult journey began with a stint in the Royal Air Force's bomber command in World War II, a role the Gandhian pacifist took after giving serious consideration to being a conscientious objector. After two years in the service, Dyson attended Cambridge University where he completed a bachelor of arts degree in theoretical mathematics in 1945. In 1947, he made his first trip to the United States to Cornell University to serve a scientific apprenticeship at the elbows of the some of the greatest minds in physics.

Cornell in 1947 was the center of a renaissance of pure physics research, born of the ideas and concepts that had lain dormant during the war. One of the chief orchestraters of this rebirth was Dyson's graduate advisor, Hans Bethe, a future Nobel Laureate who spent the war years working on the atom bomb at Los Alamos. Bethe brought other former Los Alamos scientists to Cornell, including Richard Feynman, a young professor of physics who would help influence the course of Dyson's career.

Feynman was then working on a private version of quantum theory that would later become the standard method for making calculations in particle physics. It is a credit to Dyson's scientific acumen and personable nature that Feynman and the other physicists accepted the young graduate student as a colleague straight away.

When Dyson wasn't hard at work on a physics problem Bethe had given him, he was part of a coterie of faculty and grad students ministering to

Feynman. By spending a lot of time around Feynman, Dyson got the opportunity to observe the physicist "at the height of his creative powers." Dyson understood Feynman's work well enough that he was able to do something Feynman couldn't: write about the theories for a broader audience, a skill Dyson would develop into a second career.

Dyson's work at Cornell was short; his program lasted nine months. But during his studies at the Ithaca campus, he raised many philosophical questions for which his advisor had no answer. Philosophy questions in physics were the bailiwick of another one of Bethe's former Los Alamos colleagues, J. Robert Oppenheimer, who was then director of the Institute for Advanced Study at Princeton. Bethe spoke to Oppenheimer about Dyson, and Dyson was off to Princeton in the fall of 1948 for a year of post-graduate work at the Institute for Advanced Study.

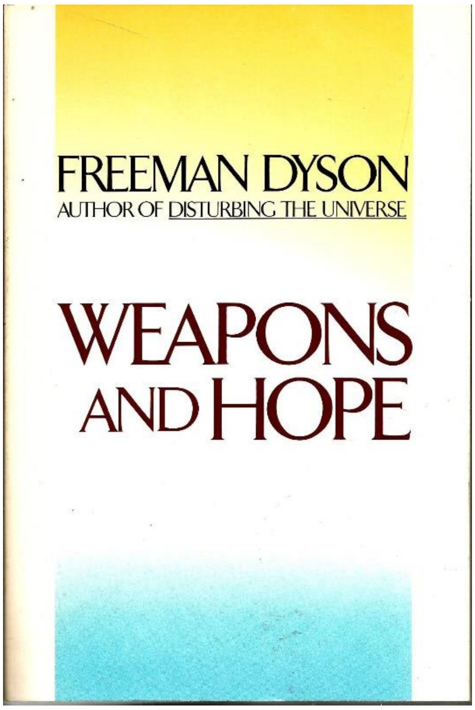
Dyson impressed the legendary Oppenheimer enough with his work that he earned a long-term membership to the institute. Dyson also met his future wife during this time. They married, settled into Princeton, and started a family that grew to six children. By 1953, Dyson had earned an appointment as a physics professor at Princeton's Institute for Advanced Study, a position he held until his retirement in 1994.

After becoming a U.S. citizen in 1957, Dyson caught wind of a fascinating project taking shape near the sun-kissed beaches of San Diego. The Orion Project would allow Dyson to marry his boyhood fascination with Jules Verne to his desire to use his mathematical training to solve an interesting problem: Is it possible to create a propulsion system that will allow man to explore the entire solar system for a politically acceptable cost? Orion provided the most exciting and happiest times of Dyson's scientific life, mostly because he became an engineer, a being apart from a scientist. He noted the difference in "Disturbing the Universe": "There are no prima donnas in engineering. In Project Orion ... nobody was working for personal glory ... It did not matter who invented what."

Orion was born at the General Dynamics Corporation, the progeny of several former Manhattan Project scientists and Dyson, all of whom were anxious to find a more noble and peaceful use for nuclear power. Under Orion, a vehicle much larger than Apollo (perhaps as big as a city) would be propelled into space by several repeated nuclear explosions. The craft would carry a large supply of bombs and the requisite machinery for throwing them out at the right time and location.

Dyson saw so much promise in this project that he predicted to writer John McPhee that they would put men on Mars by 1965 and on Saturn by 1970. Unfortunately, Orion met the same fate as Dyson's fictional Erolunar mission: It never made it to the launch pad and was declared dead in 1965. In his own post-mortem of the project in a 1965 Science article, Dyson attributed Orion's demise in part to politics over funding and by the scientific community's disdain for engaging in anything related to engineering. But mostly, Orion was scrubbed because the Nuclear Test Ban Treaty of 1963 outlawed it. Proponents of the treaty, he said at the time, didn't give Orion a chance.

Eventually, Dyson's own position on nuclear test bans would change: He grew to believe that if the U.S. were to stop nuclear weapons testing and production, it would reduce incentives for the Soviets and others to pour time and money into developing their own weapons. In the early 1960s, Dyson became a staff member of the U.S. Arms Control and Disarmament Agency, where he took part in test ban negotiations. Later in the decade, Dyson chaired the Federation of American Scientists, an organization founded in 1945 as the Federation of Atomic Scientists by former Manhattan Project scientists, including Oppenheimer, for the purpose of addressing the dangers and implications of the nuclear age. Dyson later struck an intellectual balance between opposing views: He became a champion of anti-nuclear activists, understanding at the same time why the government military machine would dismiss the protests.



In his 1984 work "Weapons and Hope," Dyson explained the military's stance toward the anti-nuclear movement with a story from his own childhood. At the age of 7, he took part in a group teasing of a younger child. His mother admonished his actions by telling the young Dyson: "You do things together which not one of you would think of doing alone."

Dyson translated his mother's lesson to fit what he believed was happening between the military and anti-nuclear activists: "Wherever one looks into the world of human organization, collective responsibility brings a lowering of moral standards. The military establishment is an extreme case, an organization which seems to have been expressly designed to make it possible for people to do things together which nobody in his right mind would do alone."

What sets Dyson apart among an elite group of scientists is the conscience and compassion he brings to his work. One of his specialties is in the field of adaptive optics, work with mirrors that can, in theory, allow a ground-based telescope to see objects as clearly in the sky as a space-based telescope. Dyson understood the dark side to adaptive optics -- that the technology used peacefully by astronomers could be used by military to focus laser beams on satellites, aircraft and other targets. Before beginning his work on the optics, Dyson studied both the peaceful and the military applications and determined that the latter death ray scenario was more the stuff of science fiction than reality. To this day, Dyson's work is enabling astronomers to make successful observations.

Dyson is so well-known for his theories about taking Jupiter apart to build a star-bound biosphere (known throughout science fiction as a Dyson Sphere), that it's easy to overlook his physics work, which would earn him worldwide recognition -- membership in the national science academies in three countries including the United States -- and numerous scientific awards, including the 1994 Enrico Fermi Award, given by the U.S. government for excellence in physics.

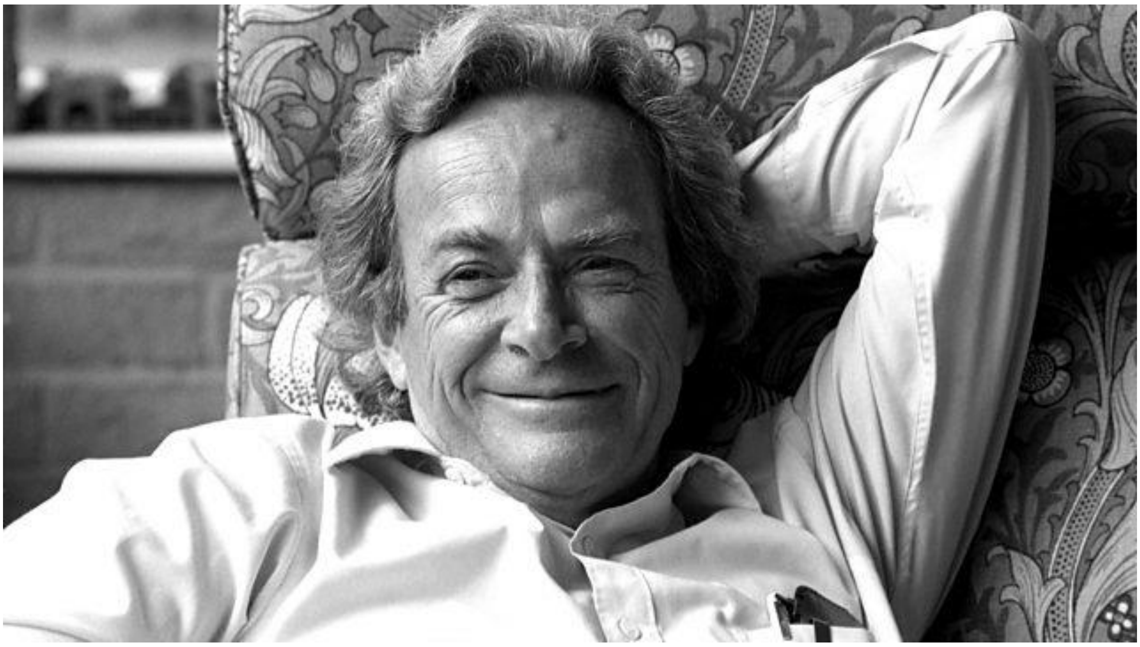
For his own part, Dyson takes a whimsical view of his place in science. He told Omni Magazine in 1978: "It's amusing to think that someday all my 'serious' work will probably be a footnote in a textbook, when everybody remembers what I did on the side."

One could look at Dyson's life and see it as a series of threads, each representing a project he has taken up on the side, all woven together to tell his story. The latest thread concerns Dyson's role as an author, writing about the aesthetics of science and explaining the philosophical and theoretical issues involved in scientific endeavors ranging from nuclear research and space travel to solar power and genetic engineering. In each of his books and articles, Dyson intersperses scientific explanation with meditation on humanism and how the human condition affects science, and vice versa. One of the most eloquent examples of this is in "Disturbing the Universe," where he examines the motivations behind the political actions of the chief architects of the atomic and hydrogen bombs, Robert Oppenheimer and Edward Teller:

Oppenheimer was driven to build atomic bombs by fear that if he did not seize this power, Hitler would seize it first. Teller was driven to build hydrogen bombs by the fear that Stalin would use this power to rule the world. Oppenheimer, being Jewish, had good reason to fear Hitler. Teller, being Hungarian, had good reason to fear Stalin. But each of them, having achieved his technical objective, wanted more ... Each of them became convinced that he must have the political power to ensure that the direction of the enterprise he had created should not fall into hands that he considered irresponsible.

Dyson is a credible analyst because he is a man who has tasted war, having served in the British military while wrestling with his conscience over the morality of war and all that goes with it. In making his observations, he thinks with his heart and hands, qualities he values as an essential part of sound scientific inquiry. This thinking is also an essential part of art. So it is no surprise that Dyson equates scientific inquiry with craftsmanship. Perhaps this is the self-styled frog's greatest legacy: to be down in the mud, engaging in the tactility of life as a human who happens to be a scientist.

2. Freeman Dyson and Richard Feynman



Richard Feynman.

The following chapter was archived in 2022, with acknowledgement and thanks, from the Nautilus website at www.nautilus.us. It has an introduction by Michael Segal.

ll through a long life I had three main concerns, with a clear order of priority. Family came first, friends second, and work third."

So writes the pioneering theoretical physicist Freeman Dyson in the introduction to his newly published collection of letters, Maker of Patterns. Spanning about four decades, the collection presents a first-person glimpse into a life that witnessed epochal changes both in world history and in physics.

Here, we present short excerpts from nine of Dyson's letters, with a focus on his relationship with the physicist Richard Feynman. Dyson and Feynman had both professional and personal bonds: Dyson helped interpret and draw attention to Feynman's work—which went on to earn a Nobel Prize—and the two men traveled together and worked side by side.

Taken together, these letters present a unique perspective of each man. Feynman's effervescent energy comes through, as does Dyson's modesty and deep admiration for his colleague. So too does the excitement each scientist felt for his role in uncovering some of the foundations of modern-day theoretical physics.

Freeman Dyson

November 19, 1947

Just a brief letter before we go off to Rochester. We have every Wednesday a seminar at which somebody talks about some item of research, and from time to time this is made a joint seminar with Rochester University. I am being taken in Feynman's car, which will be great fun if we survive. Feynman is a man for whom I am developing a considerable admiration; he is the brightest of the young theoreticians here and is the first example I have met of that rare species, the native American scientist. He has developed a private version of the quantum theory, which is generally agreed to be a good piece of work and may be more helpful than the orthodox version for some problems. He is always sizzling with new ideas, most of which are more spectacular than helpful and hardly any of which get very far before some newer inspiration eclipses them. His most valuable contribution to physics is as a sustainer of morale; when he bursts into the room with his latest brain wave and proceeds to expound it with lavish sound effects and waving about of the arms, life at least is not dull.

The event of the last week has been a visit from Peierls, who has been over here on government business and stayed two nights with the Bethes before flying home. He gave a formal lecture on Monday about his own work, and has been spending the rest of the time in long discussions with Bethe and the rest of us, at which I learnt a great deal. On Monday night the Bethes gave a party in his honour, to which most of the young theoreticians were invited. When we arrived we were introduced to Henry Bethe, who is now five years old, but he was not at all impressed. The only thing he would say was "I want Dick. You told me Dick was coming," and finally he had to be sent off to bed, since Dick (alias Feynman) did not materialise. About half an hour later, Feynman burst into the room, just had time to say "so sorry I'm late. Had a brilliant idea just as I was coming over," and then dashed upstairs to console Henry. Conversation then ceased while the company listened to the joyful sounds above, sometimes taking the form of a duet and sometimes of a one-man percussion band. …

March 8, 1948

Yesterday I went for a long walk in the spring sunshine with Trudy Eyges and Richard Feynman. Feynman is the young American professor, half genius and half buffoon, who keeps all physicists and their children amused with his effervescent vitality. He has, however, as I have recently learned, a great deal more to him than that, and you may be interested in his story. The part of it with which I am concerned began when he arrived at Los Alamos; there he found and fell in love with a brilliant and beautiful girl, who was tubercular and had been exiled to New Mexico in the hope of stopping the disease. When Feynman arrived, things had got so bad that the doctors gave her only a year to live, but he determined to marry her, and marry her he did; and for a year and a half, while working at full pressure on the project, he nursed her and made her days cheerful. She died just before the end of the war.

I was wrong when I wrote that Feynman found his wife Arlene in New Mexico. He married her first in a city hall on Staten Island and then took her with him to New Mexico. The story is movingly told by Feynman in the book What Do You Care What Other People Think? (1988), the title being a quote from Arlene.

As Feynman says, anyone who has been happily married once cannot long remain single, and so yesterday we were discussing his new problem, this time again a girl in New Mexico with whom he is desperately in love. This time the problem is not tuberculosis, but the girl is a Catholic. You can imagine all the troubles this raises, and if there is one thing Feynman could not do to save his soul, it is to become a Catholic himself. So we talked and talked and sent the sun down the sky and went on talking in the darkness. At the end of it, Feynman was no nearer to the solution of his problems, but it must have done him good to get them off his chest. I think that he will marry the girl and that it will be a success, but far be it from me to give advice to anybody on such a subject.

March 15, 1948

My own work has taken a fresh turn as a result of the visit of Weisskopf last week. He brought with him an account of the new Schwinger quantum theory which Schwinger had not finished when he spoke at New York. This new theory is a magnificent piece of work, so at the moment I am working through it and trying to understand it thoroughly. After this I shall be in a very good position, able to attack various important problems in physics with a correct theory while most other people are still groping. One other very interesting thing has happened recently; our Richard Feynman, who always works on his own and has his own private version of quantum theory, has been attacking the same problem as Schwinger from a different direction and has now come out with a roughly equivalent theory, reaching many of the same ideas independently. Feynman is a man whose ideas are as difficult to make contact with as Bethe’s are easy; for this reason I have so far learnt much more from Bethe, but I think if I stayed here much longer, I should begin to find that it was Feynman with whom I was working more.

June 25, 1948, Santa Fe, New Mexico

Feynman originally planned to take me out west in a leisurely style, stopping and sightseeing en route and not driving too fast. However, I was never particularly hopeful that he would stick to this plan, with his sweetheart waiting for him in Albuquerque. As it turned out, we did the eighteen hundred miles from Cleveland to Albuquerque in three and a half days, and this in spite of some troubles; Feynman drove all the way, and he drives well, never taking risks but still keeping up an average of sixty-five miles per hour outside towns. It was a most enjoyable drive, and one could see most of what was to be seen of the scenery without stopping to explore; the only regret I have is that in this way I saw less of Feynman than I might have done. …

Sailing into Albuquerque at the end of this odyssey, we had the misfortune to be picked up for speeding; Feynman was so excited that he did not notice the speed limit signs. So our first appointment in this romantic city of homecoming was an interview with the justice of the peace; he was a pleasant enough fellow, completely informal, and ended up by fining us ten dollars with $4.50 costs, while chatting amiably about the way the Southwest was developing. After this Feynman went off to meet his lady, and I came up by bus to Santa Fe.

Going into a sort of semistupor as one does after forty-eight hours of bus riding, I began to think very hard about physics.

All the way Feynman talked a great deal about his sweetheart, his wife Arlene who died at Albuquerque in 1945, and marriage in general. Also about Los Alamos. I came to the conclusion that he is an exceptionally well-balanced person, whose opinions are always his own and not other people’s. He is very good at getting on with people, and as we came West, he altered his voice and expressions unconsciously to fit his surroundings, until he was saying “I don’t know noth’n” like the rest of them.

Feynman’s young lady turned him down when he arrived in Albuquerque, having attached herself in his absence to somebody else. He stayed there for only five days to make sure, then left her for good and spent the rest of the summer enjoying himself with horses in New Mexico and Nevada.

September 14, 1948,17 Edwards Place, Princeton

On the third day of the journey a remarkable thing happened; going into a sort of semistupor as one does after forty-eight hours of bus riding, I began to think very hard about physics, and particularly about the rival radiation theories of Schwinger and Feynman. Gradually my thoughts grew more coherent, and before I knew where I was, I had solved the problem that had been in the back of my mind all this year, which was to prove the equivalence of the two theories. Moreover, since each of the two theories is superior in certain features, the proof of equivalence furnished a new form of the Schwinger theory which combines the advantages of both. This piece of work is neither difficult nor particularly clever, but it is undeniably important if nobody else has done it in the meantime. I became quite excited over it when I reached Chicago and sent off a letter to Bethe announcing the triumph. I have not had time yet to write it down properly, but I am intending as soon as possible to write a formal paper and get it published. This is a tremendous piece of luck for me, coming at the time it does. I shall now encounter Oppenheimer with something to say which will interest him, and so I shall hope to gain at once some share of his attention. It is strange the way ideas come when they are needed. I remember it was the same with the idea for my Trinity Fellowship thesis.

My tremendous luck was to be the only person who had spent six months listening to Feynman expounding his new ideas at Cornell and then spent six weeks listening to Schwinger expounding his new ideas in Ann Arbor. They were both explaining the same experiments, which measure radiation interacting with atoms and electrons. But the two ways of explaining the experiments looked totally different, Feynman drawing little pictures and Schwinger writing down complicated equations. The flash of illumination on the Greyhound bus gave me the connection between the two explanations, allowing me to translate one into the other.

As a result, I had a simpler description of the explanations, combining the advantages of Schwinger and Feynman. …

September 30, 1948

One thing which I must always keep in mind to prevent me from getting too conceited is that I was extraordinarily lucky over the piece of work I have just finished. The work consisted of a unification of radiation theory, combining the advantageous features of the two theories put forward by Schwinger and Feynman. It happened that I was the only young person in the world who had worked with the Schwinger theory from the beginning and had also had long personal contact with Feynman at Cornell, so I had a unique opportunity to put the two together. I should have had to be rather stupid not to have put the two together. It is for the sake of opportunities like this that I want to spend five more years poor and free rather than as a well-paid civil servant.

November 1, 1948, Hotel Avery, Boston

After my last letter to you I decided that I needed a long weekend away from Princeton. I persuaded Cécile Morette to come with me to see Feynman at Ithaca. This was a bold step on my part, but it could not have been more successful, and the weekend was just deliriously happy. Feynman himself came to meet us at the station, after our ten-hour train journey, and was in tremendous form, bubbling over with ideas and stories and entertaining us with performances on Indian drums from New Mexico until one a.m.

Feynman was obviously anxious to talk and would have gone on quite indefinitely if he had been allowed.

Cécile Morette was the brightest of the young physicists who arrived at the institute at the same time as I did. She was the only one who quickly grasped the new ideas of Feynman. We immediately became friends. The fact that she happened to be female was irrelevant to our friendship. She was a natural leader, she understood modern mathematics better than I did, and she had a great sense of humor.

The next day, Saturday, we spent in conclave discussing physics. Feynman gave a masterly account of his theory, which kept Cécile in fits of laughter and made my talk at Princeton a pale shadow by comparison. He said he had given his copy of my paper to a graduate student to read, then asked the student if he himself ought to read it. The student said no, and Feynman accordingly wasted no time on it and continued chasing his own ideas. Feynman and I really understand each other; I know that he is the one person in the world who has nothing to learn from what I have written, and he doesn't mind telling me so. That afternoon Feynman produced more brilliant ideas per square minute than I have ever seen anywhere before or since. In the evening I mentioned that there were just two problems for which the finiteness of the theory remained to be established; both problems are well-known and feared by physicists, since many long and difficult papers running to fifty pages and more have been written about them, trying unsuccessfully to make the older theories give sensible answers to them. When I mentioned this fact, Feynman said, "We'll see about this," and proceeded to sit down and in two hours, before our eyes, obtain finite and sensible answers to both problems. It was the most amazing piece of lightning calculation I have ever witnessed, and the results prove, apart from some unforeseen complication, the consistency of the whole theory. The two problems were the scattering of light by an electric field, and the scattering of light by light.

After supper Feynman was working until three a.m. He has had a complete summer of vacation and has returned with unbelievable stores of suppressed energy. On the Sunday Feynman was up at his usual hour (nine a.m.), and we went down to the physics building, where he gave me another two-hour lecture of miscellaneous discoveries of his. One of these was a deduction of Maxwell's equations of the electromagnetic field from the basic principles of quantum theory, a thing which baffles everybody including Feynman, because it ought not to be possible. Meanwhile Cécile was at mass, being a strict Catholic. At twelve on the Sunday we started our journey home, arriving finally at two a.m. and thoroughly refreshed. Cécile assured me she had enjoyed it as much as I had. I found a surprising intensity of feeling for Ithaca, its breezy open spaces and hills and its informal society. It seemed like a place which I belonged to, full of nostalgic memories. I suppose it really is my spiritual home. …

February 28, 1949, Chicago

On Thursday we had Feynman down to Princeton, and he stayed till I left on Sunday. He gave in three days about eight hours of seminars, besides long private discussions. This was a magnificent effort, and I believe all the people at the institute began to understand what he is doing. I at least learnt a great deal. He was as usual in an enthusiastic mood, waving his arms about a lot and making everybody laugh. Even Oppenheimer began to get the spirit of the thing and said some things less sceptical than is his habit. Feynman was obviously anxious to talk and would have gone on quite indefinitely if he had been allowed; he must have been suffering from the same bottled-up feeling that I had when I was full of ideas last autumn. The trouble with him is that he never will publish what he does; I sometimes feel guilty for having cut in front of him with his own ideas. However, he is now at last writing up two big papers, which will display his genius to the world; and it is possible that I have helped to make him do this by making him conscious of being cut in on, which if it be true is a valuable service on my part. …

October 23, 1965

We are all excited because my three friends Tomonaga, Schwinger, and Feynman won the Nobel Prize. You may remember that it was just after their great work in 1947 that I started my career by carrying further what they had begun. I am happy that the prize is given to the three of them equally. To some extent I can take credit for this, since Schwinger originally had all the limelight and Tomonaga and Feynman were struggling in obscurity. It was my big paper "The Radiation Theories of Tomonaga, Schwinger and Feynman" that first did justice to all three of them. I am now writing the historical account of their work which will appear next week.

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3. Dawkins and Dyson: an Exchange

Freeman Dyson.

This chapter was archived in 2022, with acknowledgement and thanks, from the Edge website at www.edge.org. It was written by John Brockman, editor of Edge.

In July 2007, Freeman wrote a provocative essay in the New York Review of Books entitled "Our Biotech Future" in which he wrote:

Biology is now bigger than physics, as measured by the size of budgets, by the size of the workforce, or by the output of major discoveries; and biology is likely to remain the biggest part of science through the twenty-first century. Biology is also more important than physics, as measured by its economic consequences, by its ethical implications, or by its effects on human welfare.

These facts raise an interesting question. Will the domestication of high technology, which we have seen marching from triumph to triumph with the advent of personal computers and GPS receivers and digital cameras, soon be extended from physical technology to biotechnology? I believe that the answer to this question is yes. Here I am bold enough to make a definite prediction. I predict that the domestication of biotechnology will dominate our lives during the next fifty years at least as much as the domestication of computers has dominated our lives during the previous fifty years.

Citing the work of Carl Woese, an expert in the field of microbial taxonomy, and Nigel Goldenfeld, a physicist, Freeman called for "a new biology for a new century":

Woese's main theme is the obsolescence of reductionist biology as it has been practiced for the last hundred years, with its assumption that biological processes can be understood by studying genes and molecules. What is needed instead is a new synthetic biology based on emergent patterns of organization. Aside from his main theme, he raises another important question. When did Darwinian evolution begin? By Darwinian evolution he means evolution as Darwin understood it, based on the competition for survival of noninterbreeding species. He presents evidence that Darwinian evolution does not go back to the beginning of life. When we compare genomes of ancient lineages of living creatures, we find evidence of numerous transfers of genetic information from one lineage to another. In early times, horizontal gene transfer, the sharing of genes between unrelated species, was prevalent. It becomes more prevalent the further back you go in time.

Whatever Carl Woese writes, even in a speculative vein, needs to be taken seriously. In his "New Biology" article, he is postulating a golden age of pre-Darwinian life, when horizontal gene transfer was universal and separate species did not yet exist. Life was then a community of cells of various kinds, sharing their genetic information so that clever chemical tricks and catalytic processes invented by one creature could be inherited by all of them. Evolution was a communal affair, the whole community advancing in metabolic and reproductive efficiency as the genes of the most efficient cells were shared. Evolution could be rapid, as new chemical devices could be evolved simultaneously by cells of different kinds working in parallel and then reassembled in a single cell by horizontal gene transfer.

Freeman's article appeared in July 2007. The following month, I hosted a seminar at Eastover Farm to explore new definitions of life required by the recent advances in genomics. I invited three of the participants—Freeman, and genomic pioneers George Church and J. Craig Venter—to come up a day early in order to spend time discussing and evaluating the import of Freeman's essay. It was interesting that Freeman, a mathematician and physicist, was now making pronouncements about evolution. Why would the mainstream evolutionary biologists care about what he has to say?

What better way to find out than to ask Richard Dawkins, the author of The Selfish Gene, and the standard bearer of Darwinism. I wrote to Richard and asked if he would comment on Freeman's ideas about horizontal evolution and "the end of the Darwinian interlude." Richard promptly responded (while noting that his hastily written piece was solely for the purpose of the meeting).

From: Richard Dawkins

"By Darwinian evolution he [Carl Woese] means evolution as Darwin understood it, based on the competition for survival of noninterbreeding species."

"With rare exceptions, Darwinian evolution requires established species to become extinct so that new species can replace them."

These two quotations from Dyson constitute a classic schoolboy howler, a catastrophic misunderstanding of Darwinian evolution. Darwinian evolution, both as Darwin understood it, and as we understand it today in rather different language, is NOT based on the competition for survival of species. It is based on competition for survival WITHIN species. Darwin would have said competition between individuals within every species. I would say competition between genes within gene pools. The difference between those two ways of putting it is small compared with Dyson's howler (shared by most laymen: it is the howler that I wrote The Selfish Gene partly to dispel, and I thought I had pretty much succeeded, but Dyson obviously hasn't read it!) that natural selection is about the differential survival or extinction of species.

Of course the extinction of species is extremely important in the history of life, and there may very well be non-random aspects of it (some species are more likely to go extinct than others) but, although this may in some superficial sense resemble Darwinian selection, it is NOT the selection process that has driven evolution.

Moreover, arms races between species constitute an important part of the competitive climate that drives Darwinian evolution. But in, for example, the arms race between predators and prey, or parasites and hosts, the competition that drives evolution is all going on within species. Individual foxes don't compete with rabbits, they compete with other individual foxes within their own species to be the ones that catch the rabbits (I would prefer to rephrase it as competition between genes within the fox gene pool).

The rest of Dyson's piece is interesting, as you'd expect, and there really is an interesting sense in which there is an interlude between two periods of horizontal transfer (and we mustn't forget that bacteria still practice horizontal transfer and have done throughout the time when eucaryotes have been in the 'Interlude'). But the interlude in the middle is not the Darwinian Interlude, it is the Meiosis / Sex / Gene-Pool / Species Interlude.

Darwinian selection between genes still goes on during eras of horizontal transfer, just as it does during the Interlude. What happened during the 3-billion-year Interlude is that genes were confined to gene pools and limited to competing with other genes within the same species. Previously (and still in bacteria) they were free to compete with other genes more widely (there was no such thing as a species outside the 'Interlude'). If a new period of horizontal transfer is indeed now dawning through technology, genes may become free to compete with other genes more widely yet again.

As I said, there are fascinating ideas in Freeman Dyson's piece. But it is a huge pity it is marred by such an elementary mistake at the heart of it.

From: Freeman Dyson

Dear Richard Dawkins,

Thank you for the E-mail that you sent to John Brockman, saying that I had made a "school-boy howler" when I said that Darwinian evolution was a competition between species rather than between individuals. You also said I obviously had not read The Selfish Gene. In fact, I did read your book and disagreed with it for the following reasons.

Here are two replies to your E-mail. The first was a verbal response made immediately when Brockman read your E-mail aloud at a meeting of biologists at his farm. The second was written the following day after thinking more carefully about the question.

First response. What I wrote is not a howler and Dawkins is wrong. Species once established evolve very little, and the big steps in evolution mostly occur at speciation events when new species appear with new adaptations. The reason for this is that the rate of evolution of a population is roughly proportional to the inverse square root of the population size. So big steps are most likely when populations are small, giving rise to the "punctuated equilibrium" that is seen in the fossil record. The competition is between the new species with a small population adapting fast to new conditions and the old species with a big population adapting slowly.

Second response. It is absurd to think that group selection is less important than individual selection. Consider for example Dodo A and Dodo B, competing for mates and progeny in the dodo population on Mauritius. Dodo A competes much better and has greater fitness, as measured by individual selection. Dodo A mates more often and has many more grandchildren than Dodo B. A hundred years later, the species is extinct, and the fitness of A and B are both reduced to zero. Selection operating at the species level trumps selection at the individual level. Selection at the species level wiped out both A and B because the species neglected to maintain the ability to fly, which was essential to survival when human predators appeared on the island. This situation is not peculiar to dodos. It arises throughout the course of evolution, whenever environmental changes cause species to become extinct.

In my opinion, both these responses are valid, but the second one goes more directly to the issue that divides us.

Yours sincerely,

Freeman Dyson

X. Interviewed at 94

Maker of Patterns: An Interview with Freeman Dyson by Austin A. Morris.

Soon to be ninety-five years old, Freeman Dyson may well be considered the grand old man of modern physics. His life has been diverse and steeped with controversy. A pure mathematician and theoretical physicist deemed prodigy, he unified three competing theories of quantum electrodynamics and was consequently given a lifetime appointment to the Institute for Advanced Study by Robert Oppenheimer, father of the atom bomb. During the 1950s, Dyson spent a year working on Project Orion, a secret study to design a nuclear-powered spaceship. He afterwards qualified as a member of the elite government advisory group called JASON, which he has served for more than five decades. Aside from being a great scientist, he has written many books on ethics and being a good human being. His most recent book titled Maker of Patterns recounts signal moments of his life spanning forty years of letters sent to his parents.

I conducted the following interview with questions posed by students from the University of Edinburgh. The last two questions come from Alumnus Richard Henderson and Honorary Professor Michael Atiyah, Nobel Prize and Fields Medal recipients, respectively.

What do you think was the greatest scientific achievement of your lifetime? What was the biggest setback? -Vedant Bhargava.

My most important contribution was the unification of the Feynman-Schwinger-Tomonaga versions of quantum electrodynamics. My biggest mistake was claiming that weakly-interacting charged bosons could not exist because they would contradict the stability of matter. This claim was demolished by the discovery of the W and Z particles a few years later.

[The 1965 Nobel Prize in Physics would have almost certainly been awarded to Dyson if it had not been for the three-person limit, which still exists today. The prize was shared between Feynman, Schwinger, and Tomonaga.]

Do you think there is any scientific theory that should be abandoned, i.e. one that is not supported by observations (e.g. supersymmetry)? - Christos Kourris.

No scientific theory should be believed as permanent truth, and no theory should be abandoned as unsupported by observations. All theories are uncertain and all might turn out to be useful. Example of a theory that was like supersymmetry for a long time but turned out to

be useful: Lie Algebras. Only after the invention of quantum mechanics did Lie Algebras unexpectedly become the language of particle physics. A wicked problem is an intractable global problem with seemingly no satisfactory solution.

Are there any wicked problems today that you would like to see addressed? -Ethan van Woerkom.

The wicked problem that affected me most directly was the wartime socialist ethic that prevailed in England during World War II. The socialist economy and the socialist ethic that allowed it to flourish are remembered by survivors of my generation as a magic time when we all shared the hardships and money was unimportant. The wicked problem is, why do we have to have a war to make socialism work?

Your mentor, GH Hardy, in his 1940 release "A Mathematician's Apology", revealed an antipathy for applied mathematics as compared to pure mathematical endeavors. His example of number theory as an innocent pursuit ironically backfired when it was used during the war to crack German enigma codes. You were influenced by Hardy, whose preference was to stay in the ivory tower. You, on the other hand, became a statistician during the conflict, applying mathematical principles to flight formations for the RAF. Was this a difficult decision for you to make?

I am not sure whether the code-breakers in World War II used number theory to crack codes. I think the use of elliptic curves in cryptography came later. The enigma operations probably had nothing to do with pure mathematics in the style of Hardy. My own job at Bomber Command had nothing to do with mathematics, either pure or applied. I was put into the job by the novelist C. P. Snow, who was then a bureaucrat responsible for placing young scientists into wartime jobs. My job was collecting intelligence about bomber losses, trying to find out how the Germans were so successful in shooting our bombers down. My most useful source of information was talking to the one per cent of bomber crews who survived being shot down and walked home through France and Spain. I had no difficulty in accepting the job. The job was frustrating because our boss Basil Dickins never told our Commander in Chief Arthur Harris anything that Harris did not want to hear. The obvious way to reduce bomber losses was to stop flying deep into Germany in poor weather when we could not expect to do much damage. But Harris did not want to hear that.

Hardy was sixty-seven years old when he lamented in the Apology that his creative talents were passé. You are ninety-four. Did you at some point in your life experience a similar epiphany? Is your recently published book, Maker of Patterns, in any way analogous to the Apology, or is it more akin to the playful musings of Richard Feynman?

Like Hardy and other mathematicians, I encountered a mid-life crisis around age forty-five, when I saw that I was no longer as smart as the young people around me. I needed another line of work besides doing research. Following Hardy's good example, I started to write books. For the second half of my life, I spent half of my time doing science and half writing books. I wrote books in a totally different style from Hardy. My style was personal, addressed to the general public and telling stories about people I happened to know and historic events that I witnessed. Hardy's style was impersonal, addressed to students and professional mathematicians, except for one book, “A Mathematician's Apology”, written at the end of his life. The Apology is addressed to the general public but is still impersonal, talking mostly about mathematics and not much about himself. My most recent book, Maker of Patterns, takes its title from Hardy but has no resemblance to Hardy's Apology; it also has no resemblance to the personal writings of Feynman. Maker of Patterns is a collection of letters written long ago with no thought of publication. I published it as a family chronicle, to give my children and grandchildren a better awareness of their roots.

Do you think private entrepreneurship could result in cheap and commonly beneficial colonisation projects in space, and will nuclear rockets ever supersede conventional chemical rockets? -Myles Khela.

The age of private entrepreneurship in space is already here. A few days ago I heard a television talk by the chief of a private company called Planet Lab in San Francisco. Planet Lab has 300 spacecraft already in orbit around the earth, Cube-sats with high-quality cameras taking pictures of the earth all the time, covering the whole earth with five-meter resolution every day. The pictures are distributed every day to his customers as needed. The customers are farmers, forest managers, fishing-boat owners, traffic managers, environmental monitors of all kinds. They pay a modest fee for his services. He says it costs him more to distribute the information than to collect it. The cube-sats are amazingly cheap since they are a by-product of the commercial cell-phone camera industry. He ended his talk saying he had bad news and good news. The bad news is that he just lost twenty cube-sats on the launch-pad when the launch-rocket exploded. The good news is that the loss does not affect his business. The twenty cube-sats are quickly replaced, and the flow of information to the customers is not interrupted.

Of course Planet Lab is not a colonization project, but the style of Planet Lab will certainly be adapted to colonization before long, with big advantages in cost and flexibility. The answer to the question whether nuclear rockets will supersede chemical rockets is no. Different missions need different types of propulsion. Chemical rockets will be good for local missions. Solar electric propulsion is good for longer-range low-acceleration missions.

Laser propulsion and microwave propulsion will be good for high-velocity missions. There does not seem to be any type of mission that is well matched to nuclear rockets. Nuclear rockets are too heavy for local missions and too slow for high-velocity missions.

To follow up on the nuclear rocket question: would a doomsday asteroid deflection scenario warrant an [Project] Orion reboot? From what I understand nuclear propulsion offers tremendously more payload, thrust, and blowback power than a chemical rocket.

I say no to Orion reboot. Asteroid deflection requires momentum, not energy. A slow push is far more effective than a quick jolt. The best way would be a mass-driver landing on the asteroid and throwing off mass at low velocity. This could be done as quickly as an Orion reboot, and with much higher efficiency. Of course you can make up a story with Orion doing the job quicker, but in the real world this seems unlikely.

[The Project Orion nuclear spaceship would require more than eight hundred atomic bombs of varying charge momentum to reach orbit. Propelled by successive nuclear explosions, the ship would be jolted upwards, protected at its base by a blast-proof pusher-plate. Because of its massive payload, the rocket could be fitted with nuclear cannons capable of razing entire continents. Then President John F. Kennedy, who had been through one nuclear standoff already, dismissed the notion of producing such a device.]

I received several questions related to the Dyson Sphere, which you say is a misnomer. What was your original conceptualization of an artificial biosphere and how might an advanced civilization go about building one (e.g. homopolar generator acting on a planet)?

The ‘Dyson Sphere’ arose out of asking the question, “How could we detect an advanced civilization that does not wish to communicate?” To be detectable at a big distance, the civilization must emit big amounts of energy, comparable with the total output of a star. The star must be surrounded by some kind of shell where the aliens live at a comfortable temperature; the shell will absorb most of the starlight and emit thermal infrared radiation from its warm surface. We detect the infrared emission. This statement so far is correct and uncontroversial. Unfortunately I went on to speculate about possible ways of building a shell, for example by using the mass of Jupiter. Jupiter could be spun up to rotate much faster, and mass would move out from Jupiter's equator to form an orbiting ring. The mass in the ring could be moved away from Jupiter to form a shell around the star. This could be done in a few thousand years using the energy from the star. These remarks about building a shell were only order-of-magnitude estimates, but were misunderstood by journalists and science-fiction writers as describing real objects. The essential idea of an advanced civilization emitting infrared radiation was already published by Olaf Stapledon in his science fiction novel Star Maker in 1937.

When will any sort of life be discovered on another planet (e.g. Mars), and when will intelligent life be discovered anywhere else in the universe? Note that Francis Crick published a book in 1981 called 'Directed Panspermia'. -Richard Henderson (Nobel Prize recipient 2017).

I have made a bet long ago that the first extraterrestrial life discovered will not be on a planet. The bet is still open. It makes no sense to guess when this discovery might happen. The whole point is that discoveries are unpredictable and nature is always ready to surprise us. This statement applies equally to unintelligent and intelligent life.

[Readers should here be warned that Dyson once told a young Francis Crick not to leave physics for biology. Dyson's bet was wrong and Crick went on to win a Nobel Prize for his work in the discovery of the double-helix structure of DNA. In this case, Richard Henderson does not need any reassurance. He left physics for biology and has already won a Nobel Prize. For those of us that have not: look no further than Dyson's last line (above).]

Now a question from Michael Atiyah. He claims to have recently proven the Riemann Hypothesis, the million-dollar problem of mathematics. Literally. Is the proof of the Riemann Hypothesis hidden inside a black hole? - Michael Atiyah (Fields Medal recipient 1966).

I would prefer the question to say: is the proof of the Riemann Hypothesis hidden inside a quasi-crystal? I see no plausible connection between the Riemann Hypothesis and black holes. Now I am waiting for Michael Atiyah to reveal his magic, to prove me wrong and Riemann right.

[I have since shared this answer with Michael Atiyah. He insists there is a connection between the Riemann Hypothesis and black holes. "Dyson is ninety-four years old," Atiyah said, implying age to be a factor. "Careful, Dr. Atiyah. If Dyson hears about this he might investigate your proof and end up proving it himself." Atiyah laughed, then got serious. I could not read his lips but I think he said something like: "game on."

Freeman Dyson and Michael Atiyah have known each other since meeting at the Institute for Advanced Study in the 1950s. Atiyah recalls a story from Cambridge when Dyson, then a pure mathematician, told Harish-Chandra he was going into physics. "Why are you going into physics? It's a mess," Harish-Chandra said. Dyson responded, "I am going into physics because it's a mess." A maker of patterns he is. Freeman Dyson went into physics to clean it up, and that is exactly what he did.]

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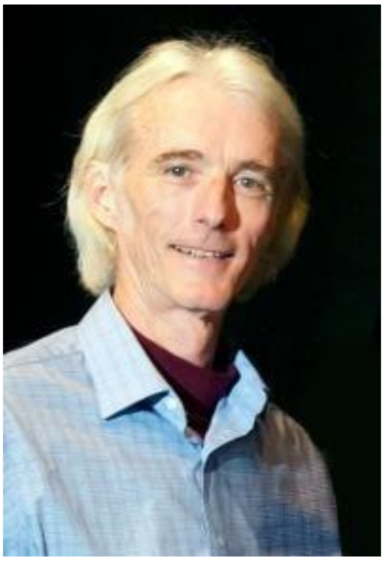
5. Memories of Freeman Dyson

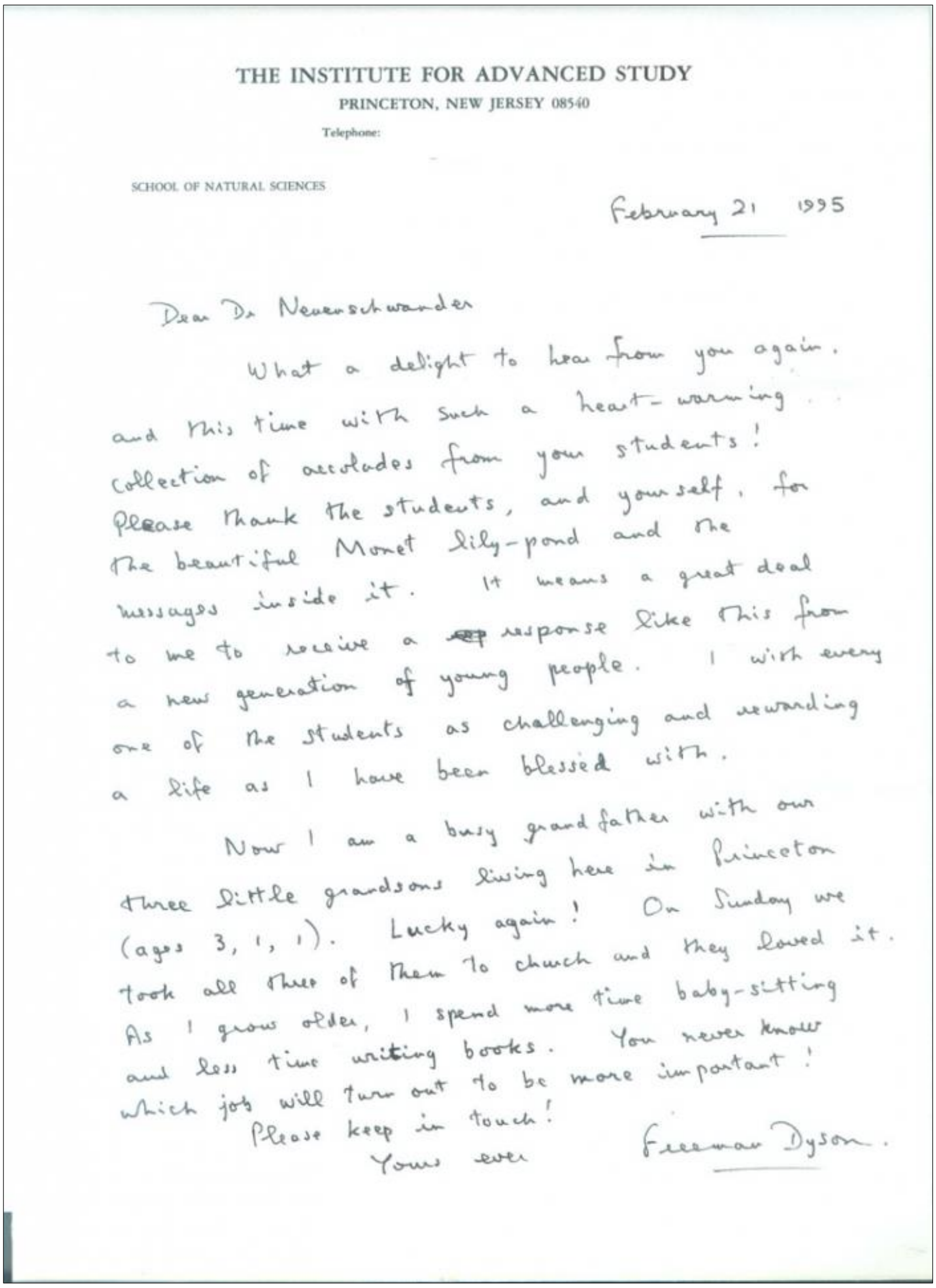
The following chapter was archived in 2022, with acknowledgement and thanks, from the website of MIT Technology Review at www.technologyreview.com. Technology Review asked several colleagues of Freeman Dyson to comment on his life and work. The article was published in March 2020.

Dwight Neuenschwander, Southern Nazarene University



At the reception before Professor Dyson gave a speech accepting the Templeton Prize in 2000, a long line of distinguished people were waiting to shake hands with him and his wife, Imme. I was standing off to the side, watching. Suddenly his grandchildren burst through the door, ages toddler to about 6. They ignored the line of dignitaries and ran toward Freeman shouting, “Papa! Papa!” The next few seconds were touching. Professor Dyson turned from the line of dignitaries and got down on his knees, and those kiddos swarmed all over him. The people waiting in line had to wait. But they did not seem to mind—we all had the privilege of watching a precious moment in the lives of half a dozen grandchildren and their beloved grandfather.

By that time I had been corresponding with him for years. In 1993, together with some students, I had written a letter to him with some questions and comments about his book “Disturbing the Universe”, hoping for a brief response. He wrote back at length a few days later, which was the beginning of a correspondence that would last for decades. (or not; it’s characteristic of him), after he received the Templeton Award he used some of the award funds to endow a scholarship at my university, so students could travel to our field station, the Quetzal Education Research Center in the Talamanca Mountains cloud forest of Costa Rica. It was always a financial struggle for students to go there to take courses and do research, but for several years now we have had the Freeman Dyson Travel Scholarship.


THE INSTITUTE FOR ADVANCED STUDY
PRINCETON, NEW JERSEY 08540
Telephone:

SCHOOL OF NATURAL SCIENCES

February 21 1995

Dear Dr Neuenschwander

What a delight to hear from you again, and this time with such a heart-warming collection of accolades from your students! Please thank the students, and yourself, for the beautiful Monet lily-pond and the messages inside it. It means a great deal to me to receive a response like this from a new generation of young people. I wish every one of the students as challenging and rewarding a life as I have been blessed with.

Now I am a busy grandfather with our three little grandsons living here in Princeton (ages 3, 1, 1). Lucky again! On Sunday we took all three of them to church and they loved it. As I grow older, I spend more time baby-sitting and less time writing books. You never know which job will turn out to be more important!

Please keep in touch!
Yours ever
Freeman Dyson.

A 1995 letter from Freeman Dyson to Dwight Neuenschwander.

Jump ahead to 2012, when the physics honor society Sigma Pi Sigma held its quadrennial meeting or "congress" in Orlando. About 800 people attended that meeting; some 600 of them were undergraduate physics students. Professor Dyson was a featured plenary speaker, scheduled to speak on Saturday morning. The conference began on Thursday evening. That evening, quite unexpectedly, in walked Professor Dyson, straight from the airport, holding his briefcase. He was immediately surrounded with the spontaneous reception one might envision for a member of a royal family who also happens to be a rock star.

"These days I spend more time babysitting and less time writing books. You never know which job will turn out to be most important!"

For the rest of the meeting, during any break, a very long line immediately formed before Professor Dyson. Everyone wanted to shake his hand, have him sign a book, or get a photo made with him. He patiently talked to each and every individual. On Saturday morning he joined the students in the roundtable breakout groups. When the meeting broke up on Saturday night, I was helping the staff take down the registration booth at 10 p.m. The convention center was deserted except for a few stragglers. Those stragglers were students who were still having conversations with Professor Dyson. Other than the meeting staff and convention center personnel, he was literally the last one to leave the meeting. He did not leave until everyone who wanted to talk with him had done so. Of course, he was much younger then—a mere 89!

In a hand-written letter to my class, he described spending time with his daughter's children, saying, "These days I spend more time babysitting and less time writing books. You never know which job will turn out to be more important!" I have thought about that a lot over the years as I have tried to balance the sometimes orthogonal demands of raising children and building a career.

My students asked a lot about science and religion. In his very last letter to us of December 10, 2019, in response to our question about the "optimal relationship between doubt and faith," he replied, "The optimum relationship between doubt and faith is peaceful coexistence. Both are essential to the evolution of a creative human society. Faith to pursue impossible goals, doubt to recover from disastrous mistakes. We have to learn to tolerate a wide variety of faiths and doubts."

Professor Dyson was more to me than the author of a beloved textbook. He was an inspiration and he became a friend. I am so blessed that my path crossed his. And I speak for over 3,000 students who feel the same way, who over the past 25 years have come to meet him and share his wisdom through his books and letters.

Harold Feiveson, Princeton University

The last time I saw Freeman was three weeks ago, when he came to a talk I was giving at Princeton on the role of scientists in World War II. Freeman was of course one of those scientists, working in the operations research group of the Royal Air Force. I started my talk by observing that in early 1942, with the Nazis controlling all of Europe except Great Britain and the Japanese ascendant everywhere, few would have been confident that the Allies would prevail. Freeman immediately disagreed, with his impish sense of humor. No, he said—once the Germans had invaded the Soviet Union, he was confident that the Allies would win the war. I drove Freeman home that day and his mind was as sharp as ever, though he was not so sure of his body.

That was three weeks ago. But I think back over 50 years ago, when I first heard of Freeman. In 1963 I joined the Science Bureau of the US Arms Control and Disarmament Agency, an agency newly created by the Kennedy administration. I was shown a study that Freeman had done during the summer of 1962 for the agency, "Implications of New Weapons Systems for Strategic Policy and Disarmament."

It was quite a study, with several intriguing thoughts on possible future technical developments such as low-yield nuclear weapons and laser anti-missile systems. What was more interesting, however, was the first intimation of themes that Freeman subsequently stated with even greater force: that nuclear weapons are immoral and not very useful, and should be gotten rid of; that anti-missile defensive systems are not necessarily bad; and that disarmament could come about in ways not then imagined.

To provide warmth and air, trees would be grown on the comets, and because of the comets' low gravity, the trees could reach heights of a hundred miles!

On this last point, Freeman subsequently brought to our attention the book The Camel and the Wheel, by Richard Bulliet, a historian of early Arab civilization. As Bulliet argued, the technology of wheeled transport, well known in the Middle East in Roman times, began to disappear around 500 A.D., as caravans of camels took over the transportation business. Roads soon fell into disrepair; the skills needed to build and repair wheeled carts were forgotten. Within a couple of generations, wheeled vehicles vanished throughout the Arab territories. Even the memory of their existence disappeared from the Arab world. Freeman noted that if nuclear weapons are to disappear, it is likely they will follow a similar path, gradually falling into irrelevance because no one will have use for them.

I didn’t really get to know Freeman until I got to Princeton in 1967, when I was introduced to him as an environmentalist. In 1972, my colleague Robert Socolow and I organized a colloquium series with the title “On Wilderness.” Freeman’s talk in this series, “Outer Space: A Final Wilderness,” was striking. In this talk, Freeman dismissed the asteroids or planets as fit places for colonization and wilderness adventure, but speculated instead about comets, which have abundant water, nitrogen, and carbon. To provide warmth and air, trees would be grown on the comets, and because of the comets’ low gravity, the trees could reach heights of a hundred miles! Freeman read from the diaries of Governor William Bradford to show how enormously we have underestimated the human and economic costs of the Mayflower colony, including the costs to the indigenous people. In many respects these costs, argued Freeman, are comparable to and perhaps greater than those that we would face in the next century in setting out to establish a space colony. Already in Freeman’s talk were several themes which he later made much more of. I will mention three.

Quick is beautiful. If new kinds of industrial processes, transportation systems, energy technologies, and so forth take more than a short time to produce, they are probably a bad idea; it takes too long to find mistakes and fix them. (This does not mean that Freeman looked with favor only at small technologies; he participated in Project Orion, Ted Taylor’s project to build nuclear-explosion-propelled space ships!)

Technology is unpredictable. Because of unpredictability, we want to remain flexible enough to change if we have to because of unforeseen environmental impacts. To elaborate on this point, Freeman drew on the work of Lynn White, whose paper “Technology Assessment from the Stance of a Medieval Historian” showed how impossible it would have been to do a “technology assessment” of most of the technologies developed in the Middle Ages—such as eyeglasses, the distillation of brandy, the crossbow, knitting, the spinning wheel, buttons, and the fireplace. For example, by increasing privacy, the chimney and fireplace may have (in White’s view and the words of L.J. Dresbeck) “affected the art of love more than the troubadours did.”

Diversity is to be praised. Freeman’s praise of diversity goes deep into many fields of human endeavor, but for the environment it is mainly, I think, a plea for scientists and others not to all work on the same problem, but rather to tackle a whole range of issues.

All this led Freeman to be a strong advocate for renewable energy despite his well-known skepticism of many of the computer models of global warming. Freeman believed that renewable energy technologies, by virtue of their scale and technological simplicity in the field, and by virtue of the fact that almost all developing countries are rich in sun, wind, and biomass, might at last allow people to shape energy to the real needs of people, including the rural poor in developing countries. On the subject of global warming, I should also mention Freeman's strong advocacy of growing biomass on a very large scale to take carbon out of the atmosphere.

As he would say of himself, Freeman was obsessed about the future. He thought of how our actions today will impact future generations, and he was, in almost a religious sense, optimistic about that future.

Arthur Jaffe, Harvard University

I first met Freeman Dyson when I was a graduate student at Princeton almost 60 years ago. He already had a towering reputation, and was something of an enigma to my generation of students.

I recall that Dyson began his course on quantum theory by telling us, "If anyone tells you that they understand quantum theory, they are not telling the truth." We were fascinated by his lectures, and so I invited him with a small group of friends for dinner. I remember him warning us that the biggest change in our lives would result from the economic development of China. This was a scenario that few people were prepared to believe would change the world to the extent that it has. No one predicted at that time how China's economic emergence, and the accompanying government prioritization of education and research, would lead to the overwhelming pool of extraordinarily talented young Chinese mathematicians and physicists that we have today.

My teacher Arthur Wightman had enormous respect for Dyson, and he often pointed to Dyson's many accomplishments in quantum field theory and many-body quantum systems, including the Dyson series, the Dyson representation, his work on stability of quantum matter, etc. Wightman also said that Dyson's first draft of a paper would generally be its last draft, as he could formulate his ideas and words so coherently before setting them on paper. Furthermore, he reported that Dyson was a voracious reader; each day he could recount at lunch the new developments he read in the preprints that had just arrived in the mail.

I have long been fascinated by two of Dyson's essays. In his 1972 Gibbs lecture to the American Mathematical Society, entitled "Missed Opportunities," Dyson wrote:

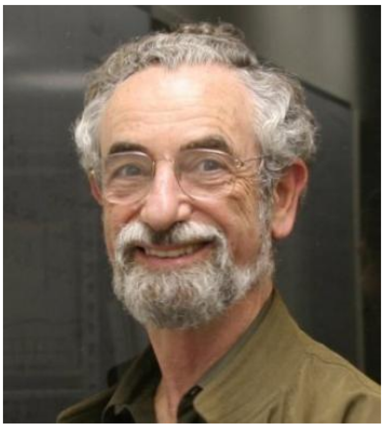
I happen to be a physicist who started life as a mathematician. As a working physicist, I am acutely aware of the fact that the marriage between mathematics and physics, which was so enormously fruitful in past centuries, has recently ended in divorce. The divorce for a time was so complete that Dyson remembered staring at a sequence of numbers that he thought in retrospect should have seemed familiar: 3, 8, 10, 14, 15, 21, 24, 26, 28, 35, 36, ... He wrote:

As I was, for the time being, a number theorist, they made no sense to me. My mind was so well compartmentalized that I did not remember that I had met these same numbers many times in my life as a physicist … the number theorist Dyson and the physicist Dyson were not speaking to each other.

As a result, Dyson missed out on discovering a fundamental connection between two different mathematical objects called Lie algebras and modular forms. Thankfully physics and mathematics have had a reconciliation, so some scholars like Dyson are once again respected both as mathematicians and as physicists. In his 2009 essay “Birds and Frogs,” Dyson compared two approaches to discovery in mathematics by likening them to those creatures:

Birds fly high in the air and survey broad vistas of mathematics out to the far horizon. They delight in concepts that unify our thinking and bring together diverse problems from different parts of the landscape. Frogs live in the mud below and see only the flowers that grow nearby. They delight in the details of particular objects, and they solve problems one at a time. I happen to be a frog, but many of my best friends are birds .... Mathematics needs both birds and frogs. We will miss Dyson not only as a friend but as an unusual visionary, unafraid to challenge conventional thought whenever and wherever he could.

Elliott Lieb, Princeton University



To talk about Freeman’s career is like being put in the position of the blind Jain monks who were asked to describe an elephant. His scientific work covers so many areas in such depth that few, if any can comprehend more than parts of it. If we look at the non-scientific, political, literary, and unpublished governmental work as well, then it is altogether an elephant with at least six legs and maybe two trunks.

However, Freeman might not wish to be compared to an elephant—although it has to be said that he is on record as once referring to himself as a frog scientist who likes to play in the local mud instead of a bird scientist who pretends to an exalted view. In fact, he was both. Be that as it may, an elephant won't do.

At one point I had the pleasure of walking in a tropical rain forest and hit on the right metaphor for Freeman, one that more appropriately captures his activities. In the forest one can find tremendously huge trees, each supporting all kinds of ecosystems clinging to it at various heights. Freeman is like such a giant tree standing in the middle of the statistical mechanics forest. Many of the topics we work on would not be alive if Freeman had not started an enterprise that grew into a cluster of activity centered around his original insight. An example is "Dyson dynamics," invented in 1962, whose relevance to random matrix theory was recently discovered and led to a major breakthrough. Moreover, these activities retain their vitality, which is more than can be said for some of the fads that occasionally mark the progress of theoretical physics.

His career, which started in high school, was at first in pure mathematics, specifically number theory. He describes this aspect of his work as applied mathematics—the reason being that pure mathematics is concerned with the invention of new mathematical ideas and not with the solution of old problems. As is well known, he never bothered to get a PhD, which fits him well, but there are few people like him who can have a stellar scientific career without passing through the rites set by the profession.

The original proof of the quantum-mechanical stability of matter by Dyson and Andrew Lenard in 1967 certainly must be counted as one of the most advanced pieces of hard mathematical analysis up to that time. It had two outstanding Dyson hallmarks. One was the ability to recognize a core problem in physics—even though the received wisdom at the time was that there was nothing interesting here. The other is the ability to create the mathematics necessary to crack the problem.

Since that time mathematical physics has come a long way, and we are not surprised to see occasional breakthroughs, with newly invented bulldozers clearing paths through the forest. But that kind of performance had not been seen previously.

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Having cited these aspects of Freeman's contributions, we must come back to the epicenter of his dynamic life. Freeman described himself as an expert in mathematical physics, which he characterized as "a discipline of people who try to reach a deep understanding of physical phenomena by following the rigorous style and method of mathematics." He continued, "It is a discipline that lies at the border between physics and mathematics. The purpose of mathematical physicists 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."

Let me end by indulging in a few personal reminiscences about my own indebtedness to Freeman. My first interaction with him was as a graduate student in the 1950s. There was essentially no book available to learn the modern quantum field theory from, except for Freeman's book Advanced Quantum Mechanics. These course notes have been recently republished and are available online. He wrote it in 1951, when he was 28 years old. How many people can write a leading-edge book at that age? I tried to understand it and didn't really do so until I was 38, but that didn't stop me from writing a PhD thesis on the topic in 1956!

Freeman's positive 1967 review in Physics Today of my book with Dan Mattis on one-dimensional physics helped us a lot, but the point for the moment is that it showed, once again, his interest in the crazy ideas and his willingness to go to bat for them. He wrote, and I quote, "A man grows stale if he works all the time on insoluble problems, and a trip to the beautiful world of one dimension will refresh his imagination better than a dose of LSD."

Portions of Elliott Lieb's essay previously appeared in articles in Communications in Mathematical Physics and Worlds Scientific celebrating Freeman Dyson's 80th and 90th birthdays, and are used here with permission.

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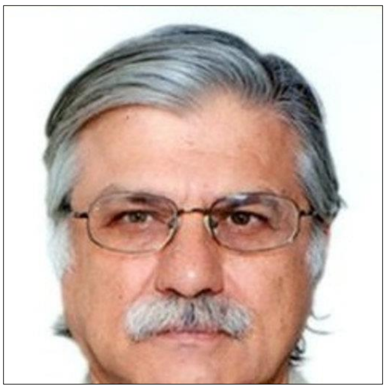

Freeman Dyson came to the Max Planck Institute of Physics and Astrophysics in Munich for an extended visit in 1974. I had moved there from UCLA the year before. Dyson's office was two doors down from mine. Werner Heisenberg's office was two doors after his. Heisenberg would still come to the Institute once a week, and on most such visits he would also go down to the basement to play Ping-Pong.

In October 1974, Dyson brought news that Russell Hulse and Joseph Taylor had discovered a binary pulsar. This was one of the most remarkable astrophysical objects ever discovered: Hulse and Taylor would later win the Nobel Prize. Nobody was yet sure what it was—probably a neutron star or a black hole closely orbited by a companion star. Dyson got us talking about the importance of studying the system. Because it was the most compact gravitationally bound system yet discovered, it was an ideal laboratory for studying Einstein's general theory of relativity.

Dyson conducted his discussions in clear and impeccable German. One of the tests of GR that I had been particularly interested in was the so-called Stanford Gyroscope Test proposed by the renowned physicist Leonard Schiff in 1960, which predicted very small, but detectable, effects that general relativity would have on the precession of a spinning gyroscope. Even in 1974, nearly 15 years after the initial proposal, it could not be carried out because of its extreme technical complexities. (Gravity Probe B, a NASA probe launched in 2004 would later confirm Schiff's calculations.)

What interested me about this at the time was that it remained one of the untested predictions of Einstein's theory. After the very first discussion meeting, it became obvious to me that with pulsars being the most mechanically stable gyroscopes, this binary pulsar system was the best place to see the effect in nature. So in the third week of October 1974, I made a preliminary calculation of this effect and, finding it to be several thousand times the effect predicted by the Stanford experiment, showed it to Dyson. Dyson was very encouraging and brought it to the attention of the discussion group immediately. His "Hari Dass hat hier calculiert ..." still rings vividly after 46 years! That a man with so many great achievements to his credit so readily encouraged a youngster made a deep impression on me.

Though he was very encouraging, he mildly admonished me for using circular orbits and asked me to do a more realistic calculation using elliptic orbits. We started to discuss how to observe this effect in the binary system. In first week of November I was to drive from Germany to India by road through Austria, Yugoslavia, Bulgaria, Turkey, Iran, and Afghanistan. I specifically asked Dyson how the radio telescopes in India could be used for this purpose. It amazed me to find out the details of the Indian radio telescopes Dyson was already aware of! He was of the strong view that the Ooty telescope had many advantages over telescopes elsewhere.

The binary pulsar had to be put on the back burner till I reached Bangalore, India, in late December. Upon the recommendation of Ramanath Cowsik, the astrophysicist in whose car we did the journey to India, I met V. Radhakrishnan, the renowned radio astronomer and the director of Raman Research Institute in Bangalore. I showed him my calculations and also recounted my discussions with Dyson about observations. We then worked out in detail how the effect might be observed by monitoring the pulse width and polarization sweeps.

In the meantime I became acutely aware of a serious theoretical lacuna in my calculations of the observable effect; they had used the existing calculations based on Einstein’s GR, which were valid only when the gyroscope mass was negligible compared with that of the gravitating body (Earth, in the case of the Stanford experiment). But in the binary pulsar case, the two component pulsars were comparable in mass. My confidence in reworking the gravitational two-body problem was shaky. But at that time I was coming increasingly under the influence of Julian Schwinger’s source theory. Upon returning to Munich in February, I was relieved to find that the astrophysicists had not clinched the issue yet, and Dyson was still there! I inducted my fellow particle physicist Ching-Fai Cho (also at the Max Planck Institute) into a source-theory-based calculation, which we finished in less than two months. The Ehlers group too finished their calculations, though a month or so after us, and the two agreed! We were euphoric at having beaten general-relativistic methods in the game, and we got carried away in our manuscript. Again we discussed our work with Dyson on a continual basis. When we showed him our finished manuscript, he quipped that we had made a valuable contribution but some of the “blowing one’s own trumpet” could be done away with!

Dyson’s office was always open. When we gave him our manuscript for his comments and went to see him about it a few days later, he pulled out a large manila envelope on which a large number of names were written, including ours, with some crossed out. This was his “to read” list of papers, written by young, old, established, and beginning scientists … all treated with equal seriousness.

We used to meet for lunch almost every day. Lot of brainstorming used to accompany these lunches, which brought out even more warmly the human side of Dyson. On one particularly memorable occasion he quipped that in his opinion, the origin of languages is an even harder problem than the origin of life. Several decades later I still brood over that.

With Freeman Dyson gone, the restless universe will be even more restless.
