FREEMAN JOHN DYSON
15 December 1923 — 28 February 2020
Freeman John Dyson combined a long career with a short attention span, contributing to five fields of mathematics and 11 fields of physics, as well as to theoretical biology, engineering, operations research, literature and public affairs. At the age of 25 he played a key role in the formulation of quantum electrodynamics—the theory of light and matter that gives us our most precise description of much of the physical world—securing a lifetime appointment to the Institute for Advanced Study that allowed him to cultivate his interests without ever becoming an administrator or obtaining a PhD. In addition to his theoretical pursuits, he helped to design a successful nuclear reactor, developed plans for an interplanetary spaceship, advocated for social justice and engaged in early efforts to mitigate the risks of nuclear weapons and anthropogenic climate change. He was a Fellow of the Royal Society for 67 years, 11 months and 7 days, a record exceeded among the Society’s scientific Fellows only by Sir Hans Sloane (elected 1685, Fellow for 67 years, 11 months and 20 days), Sir John Davis (elected 1822, Fellow for 68 years and 7 months), and possibly by Jean Chardellou (elected 1702), who is believed to have died aged 107 in 1771, but the precise date is unknown.

**Early life**

‘Your old friend Dante had a very amateur conception of Hell,’ wrote Dyson’s father, the distinguished composer Sir George Dyson (KCVO, 1883–1964), then a lieutenant with the British Expeditionary Force, to Geoffrey L. Bickersteth, a noted Dante scholar, on 5 December 1915. ‘The trenches are simply vile in this weather, between knee-deep and thigh deep in mud. Everybody retires to dugouts, and even down there, 20 feet below ground sometimes, the shock blows the candles out.’ A paragraph later the letter was interrupted by the explosion.
of a six-inch shell, killing a horse and badly wounding three men. ‘These matters are in the hands of a blind fate whose decrees it is perhaps well that we cannot foresee,’ George Dyson added in returning to the letter. ‘Atkey is, as you doubtless know, out here somewhere, but I have not come across him yet.’

Freeman Atkey, George Dyson and Geoffrey Bickersteth were fellow instructors at Marlborough College who had passed through France on a motorcycle expedition to Italy in 1912, two years before Dyson and Atkey enlisted together at the outbreak of World War I. Atkey was killed by a sniper on 5 August 1916, and Dyson, who survived the war shell-shocked but otherwise unharmed, married Atkey’s bereaved sister Mildred in 1917. Their second child, born in Crowthorne, Berkshire, on 15 December 1923, was named Freeman in memory of his uncle (figures 1 and 2). They soon discovered they had a mathematical prodigy on their hands.

Figure 1. Dyson at age five on the Upper Wye, summer 1929. Courtesy of the Dyson family.
‘I don’t know how old I was; I only know that I was young enough to be put down for an afternoon nap in my crib,’ Dyson remembered (77).* ‘I didn’t feel like sleeping, so I spent the time calculating. I added one plus a half plus a quarter plus an eighth plus a sixteenth and so on, and I discovered that if you go on adding like this forever you end up with two. Then I tried adding one plus a third plus a ninth and so on, and discovered that if you go on adding like this forever you end up with one and a half. Then I tried one plus a quarter and so on, and ended up with one and a third. So I had discovered infinite series. I don’t remember talking about this to anybody at the time. It was just a game.’

Mildred Atkey Dyson, who had been admitted as a barrister after serving at her father’s law firm during the war, preserved some of Dyson’s earliest calculations. ‘Iff a man walked 6400 sencherres. How many steped did he go?’, he asked in August 1929. ‘The anser is 373.345.600.000.000.000.000.’ A sketch of the Solar System from 27 July 1929, labelled ‘Astronimy’, notes that ‘Jupiter has 7 moones. You can allways tell Saturn because it allways has rings round it. You can hardly ever see Mercery becase the Sun is nearly allways in frount of it.’ The errors show that Dyson was not relying on published facts. He approached any subject, no matter how well established, by working it out for himself.

At age six Dyson was enrolled in a one-room school in Winchester, run by ‘a forcible character and a magnificent teacher,’ Miss Mary S. Scott. In September 1932, at the age of eight, he was sent as a boarding student to Twyford School. ‘It was an abominable school but

* Numbers in this form refer to the bibliography at the end of this memoir.
had an excellent library so that was my refuge,’ he remembered. He began teaching himself physics. ‘There was lots and lots of stuff about electrons and electricity and radio waves and all sorts of things, but nobody ever mentioned protons and I couldn’t figure out why. I remember asking people, “Why is it that they only talk about electrons and not about protons?” Nobody seemed to know.’ Science did not yet feature in a curriculum emphasizing Greek, Latin, mathematics and sports. In company with a few kindred spirits ‘lacking in physical strength and athletic prowess’, Dyson founded a science society that met surreptitiously. ‘We could do no real experiments,’ he added. ‘All we could do was share books and explain to each other what we didn’t understand’ (68).

He also organized a raffle, taking in small amounts of money from other students, paying half of it back to the winner and pocketing the rest. ‘Yes, they knew I was keeping half of it,’ he admits, ‘but they kept giving me their money!’

WINCHESTER

At the age of 12 Dyson was placed first in the three-day examination for a scholarship to Winchester College, where he fell in with three like-minded students: Michael Longuet-Higgins (FRS 1963), Christopher Longuet-Higgins (FRS 1958) and James Lighthill (FRS 1953). ‘Winchester College did not believe in forcing the more gifted boys to learn advanced mathematics and science in formal classes,’ he explained. ‘The teachers wisely recognized that we could do better on our own, and treated us with benign neglect’ (72). The ‘gang of four’ worked their own way through Camille Jordan’s three-volume *Cours d’analyse* (ForMemRS 1919; Jordan 1882–1887), which Dyson believed was placed in the college’s library by their predecessor G. H. Hardy (FRS 1910). The next war loomed. ‘We calculated the odds to be about ten to one that we would be dead in five years’ (47).

Dyson won a series of class prizes, with monetary awards redeemable in books, by which he collected the works of E. T. Bell, A. S. Eddington (FRS 1914), G. H. Hardy, H. G. Wells, H. T. H. Piaggio, Felix Klein (ForMemRS 1885), Srinivasa Ramanujan (FRS 1918) and David Hilbert (ForMemRS 1928), with some other English and some Russian literature mixed in. On his own account, he purchased a copy of *The fifty-nine icosahedra* co-authored by Donald Coxeter (FRS 1950), published in Canada in 1938 (Coxeter et al. 1938). With the Longuet-Higgins brothers, he spent ‘the long evenings in winter, for three years or so’, constructing a set of models of the 59 possible stellations of the platonic icosahedron that ‘give to anyone who looks at them a vivid introduction to symmetry groups’ (84). This hands-on exposure to group theory would resurface in Dyson’s later work (36, 41). As he remembered it, ‘The problem was always getting the very last joint to stick’ (figure 3).

CAMBRIDGE

Dyson and Lighthill won scholarships to Trinity College, Cambridge, at age 15 in 1939, but their entrance was deferred until September of 1941. The older students, along with the younger professors, had left to join the war effort, leaving the younger students and remaining professors to fend for themselves. ‘There is at the moment in the University only one mathematical research student, and she is under Hardy’s wing,’ reported Dyson, beginning his first term with Hardy lecturing on Fourier series to an audience of four, Abram
S. Besicovitch FRS on integration to an audience of three, Leopold Pars on dynamics with four students, and Paul Dirac FRS on quantum mechanics, ‘who does not break new ground as far as I am concerned, but he suffers from an audience of twenty’.

Dyson attended A. S. Eddington’s course on relativity with two other students, while for fluid dynamics he faced Harold Jeffreys FRS alone, wondering ‘whether if I had not appeared promptly at nine o’clock, Jeffreys would have given his lecture to an empty room’ (72). Dyson and Lighthill were allowed two years to complete their studies, while many of their classmates were given their wartime assignments after a single year. ‘There is no talk of examinations,’ Dyson reported to his parents, ‘or anything silly like that.’ Besicovitch gave Dyson problems ‘which were impossibly difficult but taught me how to think’ (77). His mathematical style, adopted by Dyson, was to build ‘out of simple elements a delicate and complicated architectural structure, usually with a hierarchical plan, and then, when the building is finished, the completed structure leads by simple arguments to an unexpected conclusion’ (72). They took long walks when only Russian was spoken, and Besicovitch invited Dyson to his private billiard table at his lodgings in Neville’s Court. Hardy, who ‘lectured like Wanda Landowska playing Bach’ (71) and ‘was a passionate billiard-player’ (87), remained aloof, unlike J. E. Littlewood FRS, who ‘refused to go on lecturing unless I should come’.
Dyson, who had joined the Cambridge Mountaineering Club (figure 4) as well as a secret society devoted to climbing the Cambridge buildings at night, was discovered by Littlewood while attempting a one-handed circumscribing of the stone pillars in Neville’s Court. Littlewood, himself a mountaineer, ‘explained to me at length all the finer points, and was only prevented from giving a demonstration by the fact that he was wearing a dressing gown’, Dyson noted in a letter dated 8 May 1943, adding that Littlewood ‘is the only person who can do all the pillars both ways with either hand’.

Dyson published his first three papers during his second year at Cambridge: the first on parallels to the Rogers–Ramanujan identities (2), the second on the partial quotients of a continued fraction (3) and the third a short note for the Journal of the Royal Statistical Society on kurtosis, or the shape of frequency distribution curves, attempting to address the deficiency that ‘statements in statistical literature on kurtosis seem to be rather loose’ (4). He also submitted a paper on simultaneous Diophantine approximations to the Proceedings of the London Mathematical Society, whose publication was delayed until 1946 (9), and two papers to Eureka, the journal of the Archimedean Society, whose publication was delayed until 1944. Dyson’s ‘proof that every equation has a root’ might not be new, he admitted, but ‘has a certain advantage over other proofs in using only the most elementary arguments’ (6). ‘Some guesses in the theory of partitions’ presented three conjectures on Ramanujan’s partitions of integers, a subject Dyson had been puzzling over since 1938. He predicted the existence of a generating function he termed the ‘crank’, suggesting it was ‘unique among arithmetical functions in having been named before it was discovered’, and hoping it would be ‘preserved from the ignominious fate of the planet Vulcan’ (7). Dyson’s crank was discovered by George Andrews after 43 years (Andrews & Garvan 1988).
‘We knew that our time at Cambridge would be short, and we did not want to waste any of it’ Dyson wrote of his 22 months at Trinity (71). He passed Parts II and III of the Mathematical Tripos with 1st class honours and a distinction in 1943. A BA in mathematics, awarded at the completion of his wartime service, would be his only earned degree.

**Bomber Command**

In June of 1943, after declining the offer of ‘a job in the d-c-d-ng department (this I am not supposed to divulge)’, Dyson was sent for an interview with C. P. Snow and assigned to the Royal Air Force Bomber Command’s Operational Research Section at High Wycombe in Buckinghamshire, in the losses division under statistician Reuben Smeed. The reasons for the loss of so many British bombers were poorly understood. Flying closer together increased losses to collision with friendly aircraft while flying farther apart increased losses to enemy attack. Pilots preferred to be lost to enemy fire than to collision and behaved accordingly, even though the actual rate of loss to collision was unknown. Using aircraft density, relative velocity and presented cross-section to estimate an upper bound, Dyson determined that pilots were vastly over-reporting ‘events that looked like collisions: first an explosion in the air, and then two flaming objects falling to the ground . . . [but] probably involved single bombers, hit by antiaircraft shells or by fighter cannon fire, that broke in half’ (79).

Despite all countermeasures, bomber losses persisted at about 5% per mission flown. Recruits were expected to fly 30 missions, so this was an unsustainable rate of loss. As Dyson put it at the time, was there a way to determine whether a particular countermeasure was effective, given that the statistician was forced ‘to divide his data into numerous small categories, the figures within each category being strictly comparable but too small to be significant’? He developed ‘a method for obtaining significant results’ despite these limitations, ‘without introducing any unwarrantable hypotheses concerning external factors, and without wasting any available information’ (5). Dyson’s method was later reinvented to estimate the effectiveness of a drug or medical intervention across a diverse population when the directly comparable sample sizes are small. ‘The search for causes of bomber losses was a problem of epidemiology,’ he noted (72).

In the course of gathering information on aircraft losses and bomb damage assessment, Dyson gained insights that haunted him for life. It was against the policy of the Operational Research Section ‘to give the Command any unpalatable advice’, he noted in 1952, ‘and I was much too young and timid to rebel’. In 1979, Dyson would be sued for libel when he published an insider’s account of the failure to reduce avoidable losses, the low rate of success against military targets and the deliberate attempt to inflict maximum civilian loss of life (57).

**Number theory**

‘Mathematics in England was at a low ebb,’ wrote Dyson of the war years. Most of the young mathematicians ‘had disappeared into the cryptographic establishment at Bletchley, which was like a black hole. People went to Bletchley and were never seen again’ (62). Hardy introduced Dyson to Wilfrid Norman Bailey, with whom he collaborated in the search for new Rogers–Ramanujan identities by exchanging letters during the war. ‘In the cold dark evenings, while I was scribbling these beautiful identities amid the death and destruction of 1944, I felt close
to Ramanujan,’ remembered Dyson. ‘He had been scribbling even more beautiful identities amid the death and destruction of 1917’ (62).

When the war ended, Dyson was assigned a post as a demonstrator at Imperial College, where he published a generalization of Mann’s theorem on the densities of sets of integers—a problem he had begun working on in 1941 when it was still known as the alpha–beta conjecture, before he was preceded in its proof by Henry Mann. In early 1944, ‘during a particularly grim period in the history of Bomber Command . . . when we lost 94 heavy bombers and more than six hundred crewmen in one night’ (72), Dyson had managed to achieve ‘the widest possible generalization of which Mann’s result is capable’, with a proof that he noted was ‘little longer than Mann’s proof of his special case’ (8).

Number theorist Harold Davenport FRS at University College became Dyson’s unofficial advisor. He suggested problems that were ‘difficult but not impossible’ (77), starting with a conjecture on rational approximations of algebraic numbers that had defeated Carl Ludwig Siegel, who had only arrived at a proof of a weak bound. Dyson decided that if he could prove Siegel’s conjecture he would continue with mathematics, but if he failed he would become a physicist. Only able to achieve an ‘inconsequential’ improvement over Siegel (11), he sought to return to Trinity College as a fellow, where he would be free to make the switch. This required a fellowship thesis, so, at Davenport’s suggestion, he tackled the Minkowski conjecture on the product of non-homogeneous linear forms. This had been proven by Hermann Minkowski for \( n = 2 \) and by Robert Remak for \( n = 3 \), but there progress had stopped. Borrowing ‘powerful weapons from the armoury of topology’, Dyson succeeded in a proof for \( n = 4 \), reminding the fellowship committee that ‘it is therefore the achievement of my paper which I value most highly, to have successfully used in one branch of mathematics ideas belonging to another branch apparently so remote from it’ (10, 13). This gained him two published papers as well as the fellowship he sought (10, 12, 13). Upon Dyson’s return to Cambridge in September 1946, his colleague Harish-Chandra (FRS 1973) announced that he was switching from physics to mathematics because ‘physics is such a mess’. Dyson responded that he was switching from mathematics to physics ‘for the same reason’.

‘It was easy for me to switch from mathematics to physics, because both number-theory and physics are branches of applied mathematics,’ added Dyson. ‘I define a pure mathematician to be somebody who creates mathematical ideas, and I define an applied mathematician to be somebody who uses existing mathematical ideas to solve problems. I was always an applied mathematician, whether I was solving problems in number-theory or in physics. I was never a creator of ideas. The main difference between number-theory and physics is that in number-theory the experimental data are more accurately known’ (72). Number theory remained Dyson’s first love and he returned to it whenever a new problem, or a chance to revisit an old one, crossed his path. He published new results in the theory of partitions in 1969, 1988, 1989 and 2012 (46, 63, 65, 83).

**FIELD THEORY**

In 1946 physics was still in transition from the old physics, where fields were produced by things, to a new physics, where things were produced by fields. America had taken the lead in the experiment-driven study of elementary particles, but Europe remained ahead in the mathematics-driven understanding of fields. Back at Cambridge as a fellow, Dyson
found a mentor in field theorist Nicholas Kemmer (FRS 1956), whose course in quantum field theory led Dyson to conclude that America, where there were more experimentalists producing results than theorists to explain them, offered the best prospects for his skills. With an introduction from G. I. Taylor FRS, who had worked with Hans Bethe (ForMemRS 1957) at Los Alamos during the war, he received a Commonwealth Fellowship from the Harkness Foundation to study under Bethe at Cornell. ‘Fifteen years ago to the day, I was deposited in the strange and forbidding environment of Twyford School,’ he wrote to his parents on board the Queen Elizabeth on 15 September 1947. ‘To-morrow at 6.30 a.m. we dock in New York.’

He found Bethe’s group, including Richard Feynman (ForMemRS 1965), working with the same enthusiasm they had shared at Los Alamos during the war. The source of their excitement was an absorption line in the fine structure of the spectrum of the hydrogen atom that was shifted slightly from where it should have been according to the theory of Dirac. The Lamb shift, named for its measurement by Willis Lamb, had left physics in disarray. ‘If you don’t understand the hydrogen atom, you don’t understand anything,’ Dyson remembered, ‘and to find that things were wrong even with a hydrogen atom was a big shock’ (14). Bethe, whose group was already working on four different approaches to the problem, assigned the 23-year-old Dyson to attempt a relativistic calculation, simplified by disregarding the electron’s spin. Dyson had shown up at the right time. ‘Bethe and Feynman,’ he explained, ‘had been doing physics successfully for many years without the help of quantum field theory’ (72). They did not pay much attention as he set to work. A few weeks later he produced an answer close to that observed by Lamb. The attitude changed. ‘The convergence of the present calculation is noteworthy and somewhat unexpected,’ Dyson reported in a paper that Bethe shepherded into print (14). ‘Bethe was impressed,’ he added. ‘He said it was the first time he had seen quantum field theory do anything useful’ (72). The simplified model, while disregarding spin, was ‘at least conceptually a physical system (which a non-relativistic system is not) [and] gives a convergent expression … very close to the non-relativistic approximations and to the observed shifts’ (14).

Quantum electrodynamics

Quantum electrodynamics (QED) had three apostles at the time of the Lamb shift. Julian Schwinger, based at Harvard, had assembled an elaborate mathematical apparatus that was delivered in a marathon eight hours of lectures to the world. Richard Feynman, based at Cornell, had developed ‘his own private version of quantum theory’, as Dyson put it, by which he ‘just wrote down the solutions out of his head without ever writing down the equations’ (57), using graphical methods that no one else could understand. Sin-Itiro Tomonaga, who had spent two years with Werner Heisenberg (ForMemRS 1955) in Leipzig and continued working in isolation in Tokyo during the war, had developed a third version of QED grounded in the traditions of the European field theorists, which might have gone unnoticed had he not sent a manuscript to Robert Oppenheimer (ForMemRS 1962) by post.

Dyson had the persistence to comprehend the methods of Schwinger, the background in field theory to understand Tomonaga and the good fortune to be taken in by Feynman as his personal confidant and friend. Upon discovering that the calculation on which he had spent ‘several months of work and several hundred sheets of paper’ could be performed by Feynman, ‘calculating on a blackboard, in half an hour’, Dyson decided his main job must
be to explain Feynman’s ideas ‘in a language that the rest of the world could understand’ (57). In June of 1948 Dyson was preparing to attend a symposium where Schwinger would be explaining his new theory when Feynman, who was driving to Albuquerque, New Mexico, offered him a ride west (figure 5). Dyson spent three and a half days with ‘the whole world to talk about’ on the 1800-mile drive from Cleveland to Albuquerque with Feynman, followed by a full two weeks of immersion in the QED of Schwinger, which he endorsed with the statement that ‘I think in a few months we shall have forgotten what pre-Schwinger physics was like’. At the end of the summer, returning from Berkeley to Chicago by Greyhound bus, and ‘going into a sort of semi-stupor as one does after 48 hours of bus-riding, I began to think very hard about physics, and particularly about the rival radiation theories of Schwinger and Feynman,’ he reported. ‘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.’

In Besicovitch style, Dyson constructed a framework revealing the equivalence of the Feynman and Schwinger theories, while introducing a method of renormalization that cancelled the troublesome infinities, produced by the self-energy of the electron, that had plagued previous versions of QED. For the second year of Dyson’s fellowship he had been assigned to the Institute for Advanced Study under Oppenheimer, and now, as he wrote to his parents, exactly one year after landing in America ‘who would have dreamed that I should be coming to Princeton with the thought not of learning but of teaching Oppenheimer about physics?’ Upon arrival in Princeton, Dyson spent ‘about five days stuck in my rooms, writing and thinking with a concentration which nearly killed me’, turning the Greyhound bus results into a 17-page paper offering ‘a considerable simplification of the procedure involved in
applying the Schwinger theory to particular problems, the simplification being the greater
the more complicated the problem’ (15).

Dyson then began the extension of his methods of renormalization to higher orders of
perturbation, completing his next paper, ‘The S-matrix in quantum electrodynamics’, by the
end of the year and announcing that ‘the well-known divergences seem to have conspired to
eliminate themselves’ (16). The conflict between finite observations and calculated infinities
could be reconciled, he suggested, by accepting two different views of the same underlying
physical truth. ‘We interpret the contrast between the divergent Hamiltonian formalism and
the finite S-matrix as a contrast between two pictures of the world, seen by two observers,
a fictitious observer whose apparatus has no atomic structure and ... a real observer, whose
apparatus consists of atoms and elementary particles and ... cannot measure the strength of a
single field undisturbed by the interaction of that field with others’ (16).

‘The present electrodynamics is certainly incomplete,’ Dyson concluded, ‘but it is no
longer certainly incorrect’ (16). Oppenheimer, who had refused to accept a theory based
on the ‘magic tricks’ of Feynman, finally conceded. He wrote ‘Nolo contendere’ in a two-
word response after the last of a series of five lectures given by Dyson in November 1948.
In January 1949 Dyson and Feynman were attending the annual meeting of the American
Physical Society in New York when a speaker mentioned ‘the beautiful theory of Feynman–
Dyson’ in reverent tones. ‘Feynman turned to me,’ Dyson reported, ‘and remarked in a loud
voice, “Well, Doc, you’re in”.’ Oppenheimer’s concession only went so far. ‘When I was
dancing on the table at the IAS Spring Dance dinner,’ noted Dyson, ‘Oppenheimer] sent Kay
Russell to “get that fellow down”.’ There is a common feeling among theoretical physicists
that Dyson’s extraordinary reconciliation of the three very different approaches of Schwinger,
Feynman and Tomonaga to quantum electrodynamics deserved a share of their 1965 Nobel
Prize in physics, but the rules only allowed three recipients.

**Institute for Advanced Study**

Dyson spent most of the next 70 years at the Institute for Advanced Study (IAS). There
were two classes of membership: visiting members in residence for one year and permanent
members in residence for life. Oppenheimer made an exception for Dyson: a five-year at-
large appointment, with no strings attached. The terms of Dyson’s Commonwealth Fellowship
required his return to England for two years, so in late 1949, under a Royal Society fellowship
with Rudolph Peierls FRS, he moved to Birmingham determined to build a complete theory
out of his initial success with QED. ‘I thought I could make it into a consistent scheme,’
he remembered. ‘You needed to prove all sorts of convergence theorems to make sure that
the series converged, and I believed I could do that. But it totally failed. In the summer of
1951 I suddenly understood that the series diverged. The whole thing was an illusion. I was
very glad that I found that out before somebody else.’ He submitted a two-page paper to *The
Physical Review*, outlining the reasons for withdrawing his earlier ‘erroneous argument’ (17)
and suggesting that the failure to make QED into a closed theory ‘in no way restricts the
accuracy of practical calculations that can be made’ and ‘leaves room for new ideas’ (18).

In the autumn of 1951 Dyson accepted Bethe’s invitation to a professorship at Cornell (figure 6), where his lecture notes on advanced quantum mechanics, circulated by mimeograph, became a landmark text (19). He assigned his graduate students, who ‘were
forced by the PhD system to concentrate their attention upon a single problem for two or three years’ (72), to an ambitious programme of meson–proton calculations, but by the time they had completed their work he had already concluded, after consulting with Enrico Fermi ForMemRS, that the approach was a dead end. ‘Fermi simply poked a hole through the whole thing,’ he wrote. ‘He said it was all rubbish.’

On 4 December 1952 Dyson was offered a permanent appointment at IAS, with Oppenheimer ‘emphasizing his youth and special gifts’. He returned to Princeton and a series of collaborations with short-term visitors who brought problems that he could help solve. Dyson compared physics and mathematics to a zoological garden, where some people are interested in the overall architecture and others are just interested in the animals. To Dyson, the IAS was a zoo where every year a new collection of animals appeared.

**Spin waves**

Dyson spent the summer of 1953 with the condensed-matter group at Berkeley, where the problem of determining the spectrum of vibrations in a disordered three-dimensional lattice of atoms whose masses and linkages are all independent random variables was assigned to him by Charles Kittel. Physical examples included both a glass, whose atoms were in a disordered, non-crystalline state, and a ferromagnet whose elements were coupled by their spin. ‘In dealing with the statistical problems of real many-body systems,’ Dyson explained,
‘theoretical physicists have a choice of three alternatives. They may either find approximate solutions of the real problem, or they may find exact solutions of some toy problem that has a qualitative resemblance to the real problem, or they may find approximate solutions of a toy problem’ (72). Dismissing the third approach as worthless and the first approach as unsuited to his mathematical taste, Dyson reduced the problem to a ‘one-dimensional glass’ where atoms of randomly distributed mass were linked into a disordered linear chain. ‘It seems to us remarkable that the 1-dimensional problem can be solved exactly,’ he reported as the summer drew to a close (21).

Dyson extended this result to a general theory of spin waves in 1956 (22). Spin-wave theory, now developed far beyond where Dyson left off, has been applied to a wide range of similarly coupled but disordered systems and their phase transitions, from superconductivity and superfluidity to flocks of birds and schools of fish. Non-linear coupling between non-adjacent elements had less effect than was first assumed. ‘The basic reason why the non-linear effects are small is that spin-waves of long wave-length float over each other like long waves on the surface of the ocean,’ Dyson explained (72). He returned to the subject in 1967, developing a hierarchical model of a one-dimensional ferromagnet with the useful property that ‘each spin has only one nearest neighbor instead of two’ (49).

**RANDOM MATRICES**

Faced with a seemingly intractable problem, Dyson often stopped to ask ‘What’s the worst case possible?’ before showing how useful numerical conclusions could still be drawn. Random matrix theory was his most successful application of this approach. In 1955 Eugene Wigner (ForMemRS 1970) had approximated the distribution of energy levels in a complex atomic nucleus, resistant to analytical treatment, by the eigenvalues of a matrix chosen at random from the ensemble of possible matrices. Dyson, after an initial discussion with Wigner and later in collaboration with Madan L. Mehta, expanded Wigner’s model into a theory that has remained productive for 60 years (34, 38).

‘In ordinary statistical mechanics we assume that we are totally ignorant of the state of a system,’ Dyson explained in 1962. ‘We then deduce properties of the system which hold on the average, where the average is defined with respect to a suitably large ensemble of possible states’ (37). He now formulated ‘a new kind of statistical mechanics, in which we renounce exact knowledge not of the state of the system but of the system itself. We picture a complex nucleus as a “black box” in which a large number of particles are interacting according to unknown laws. The problem then is to define in a mathematically precise way an ensemble of systems in which all possible laws of interaction are equally probable.’ Instead of attempting to account for individual levels and shell structure in a complex nucleus, ‘the highly excited states may be understood from the diametrically opposite point of view, assuming as a working hypothesis that all shell structure is washed out’ (34). Dyson developed both a general classification of ensembles and a series of specific models, including one where ‘the elements of the matrix execute independent Brownian motions without mutual interaction’, yet, despite this underlying disorder, an ordered distribution of energy levels results (35). Random matrix theory soon ‘grew into an elegant baroque construction, far more elaborate than the physics of nuclear energy-levels required,’ he admitted (36, 48, 72). The recent application of random matrix theory to neural networks and deep learning has sparked an unexpected renaissance.
Biographical Memoirs

STABILITY OF MATTER

‘Even to a hardened theoretical physicist it remains perpetually astonishing that our solid world of trees and stones can be built of quantum fields and nothing else,’ Dyson reflected in 1953 (20). Observing the stability of matter and proving the stability of matter are two different things. ‘We know that matter is stable, so why should one need to talk about it?’, Dyson asked. With the wave mechanics of the 1920s ‘one could calculate the orbits of the electrons in an atom, and one could see that these had a definite lowest energy. Once the atom was in its ground state it couldn’t go any farther down and therefore it was automatically stable. At that point everybody breathed a great sigh of relief and the problem was forgotten’ (42).

The stability of matter in bulk, however, remained a matter of faith. When Andrew Lenard showed up at the IAS to attempt to resolve this deficiency in 1965, Dyson offered to help. A bottle of champagne, which Dyson and Lenard shared after a full year of work, had been offered for a proof. Their ‘monstrously long and complicated’ proof (44, 45), which once took Dyson 13 hours of lectures to explain, established a lower bound to the ground state of a population of charged particles: fermions first, then bosons. Eight years later, Elliott Lieb (ForMemRS 2013) and Walther Thirring produced a more elegant proof. ‘The chief value of our bad proof is the fact that it gave Lieb and Thirring the courage to look for a better one,’ he wrote (72).

Training, Research, Isotopes, General Atomic (TRIGA) Reactors

Dyson remained a theorist until Frederic de Hoffman, a protégé of Edward Teller at Los Alamos and a fellow graduate student under Bethe at Cornell, went into the atomic energy business and offered him a job for the summer of 1956. General Atomic (now General Atomics) was founded in 1955 with the mission, as de Hoffmann put it, to harness controlled fusion and ‘bring the sun down to earth’. The company began operating out of a former elementary school near Point Loma in San Diego, vacant since the exodus of military families after the war. As a pilot project, it was decided, at the suggestion of Edward Teller, to build an intrinsically safe fission reactor, designed to shut down in milliseconds without human or mechanical intervention if the control rods were removed or cooling failed. Dyson distilled the theory behind the safe reactor into a four-page paper, issued as the company’s sixth technical report.

‘If I hadn’t had [Dyson’s] little paper I couldn’t have made the guesses to make those experiments,’ said Brian Dunne, who helped to get the construction of the reactor off the ground. ‘It was like the first draft of what you’d put on a patent application—he gave guesses as to the total amount of U238, U235, zirconium, hydrogen . . . I can trust this as the basis of the design for this thing.’ The physicists gathered at the schoolhouse recaptured the enthusiasm of wartime Los Alamos, advancing from a theory of the warm neutron effect to a full-scale prototype in under two years. When the second United Nations Atoms for Peace conference convened in Geneva in September 1958, General Atomic showed up with a working reactor and stole the show. ‘Everyone wanted to see the blue light,’ said Dunne (1999). ‘They sold those things like hotcakes.’

Sixty-six TRIGA (Training, Research, Isotopes, General Atomic) reactors were sold—at a consistent profit—a record unmatched by any other nonmilitary reactor design. Dyson left his
name, along with Andrew W. McReynolds and Theodore B. Taylor, on a patent for a ‘Reactor with prompt negative temperature coefficient and fuel element therefor’. They received one dollar each for their rights. ‘The primary object of the present invention is to provide an improved neutronic reactor which will not be destroyed even if grossly mishandled,’ they explained (23). De Hoffmann believed the reactor’s safety needed to be demonstrated in a spectacular way. At the public dedication, with Niels Bohr ForMemRS in attendance, ‘we pulled out all the control rods explosively so that the reactor was prompt super-critical with a neutron doubling time of two milliseconds,’ Dyson remembered. ‘That was the worst accident that we could imagine. The reactor quietly shut itself down in a few milliseconds with no damage. We then repeated the process five hundred times.’

**PROJECT ORION**

In 1932, at age eight, Dyson began a treatise on space exploration that was left unfinished at age nine. *Sir Phillip Roberts’s erolunar collision* described a 15-foot diameter spaceship propelled by nitrocellulose on a voyage to observe a predicted collision between the asteroid Eros and the Moon. Dyson first estimated how large a moon-based launcher it would take to escape from the Moon’s gravitational field for the return to Earth, and then calculated how large a terrestrial launcher it would take to deliver the lunar launcher to the Moon (1).

In late November of 1957, in the aftermath of *Sputnik*, Frederic de Hoffmann arrived in Princeton to recruit Dyson to help build an interplanetary spaceship propelled by nuclear bombs. The idea, first suggested by Stanislaw Ulam at Los Alamos, was now being advanced at General Atomic under the code name Project Orion by Theodore B. (Ted) Taylor, Dyson’s TRIGA collaborator, who had just ‘picked the name out of the sky’. On New Year’s Day 1958 Dyson flew to San Diego to consult with Taylor for 10 days. The resulting proposal, submitted to the US Advanced Research Projects Agency (ARPA), ‘included all the necessary practical working features for a very large space vehicle’ that could orbit a payload of 1600 tons. Dyson had ‘derived certain engineering parameters from physical first principles in an amazingly clear way,’ remembers physicist and Air Force General Lew Allen, one of the reviewers of the proposal. ARPA’s contract for the ‘Feasibility study of a nuclear bomb propelled space vehicle’ was issued on 30 June 1958. ‘If the concept is feasible, it may be possible to propel a vehicle weighing several thousand tons to velocities several times earth escape velocities,’ the contract noted. ‘Such a vehicle would represent a major advance.’

Dyson remembered the next 15 months as ‘the most exciting and in many ways the happiest of my scientific life’ (50). Taylor assembled a group that included individuals from 14 different countries, convinced that national boundaries would dissolve in space. All elements of the design were derived from first principles, with Dyson personally writing at least 18 technical reports. Most of the scientific landscape was unexplored, starting with how to convert the energy of a nuclear explosion into momentum that could drive a spaceship and ending with where to go and how much fallout would be left behind.

What happens when a mass of cold, inert propellant in a vacuum is transformed, within a few millionths of a second, into a very hot gas? ‘You get inversion,’ Dyson explained. ‘Something that begins like a pancake becomes like a cigar, and something that begins as a cigar becomes a pancake.’ The thinner the pancake, the narrower the jet. As the jet of propellant expands, it cools to about 10 000 degrees Fahrenheit over the 300 microseconds it
takes to arrive at the surface of the pusher plate, where it is recompressed to a temperature of about 100,000 degrees Fahrenheit; hotter than the surface of the Sun, but cooler than a bomb. How much of the surface is ablated depends on the opacity of the plasma, which increases as it stagnates against the plate. ‘If you squeeze the stuff together it gets blacker,’ noted Dyson. ‘Nobody had calculated this before.’

A note on maximum opacity (25) was Dyson’s first non-classified report. ‘We started doing a much better job than anyone had done before, doing it atom by atom, not just using averages,’ he wrote. ‘It turned out the opacities were pretty high, even just for carbon by itself.’ What happens during the millisecond of contact between the plasma and the plate? ‘I did a calculation looking at the worst case,’ he wrote in Preliminary study of convective ablation (30), which reported the results. ‘Because the time is so short, convection only has time to go around once or twice.’ Supersonic flow past an edge (27) addressed the question of whether the pusher plate might suffer catastrophic instabilities at its perimeter; Stability and control of space vehicle (24) suggested how to maintain stability when misalignment of an explosion produces an eccentric thrust; Optimal programming for vertical ascent of a spaceship in the atmosphere (29) concluded that ‘the starting velocity v(0) at height zero is not zero’, a requirement manifested when the group began high-explosive-driven flying model tests (figure 7).

Two stages of shock absorbers were required to reduce the 1000 g acceleration of the pusher plate to an impulse tolerable to the inhabited section of the ship. Dyson proposed ‘getting rid of the shock absorbers’ in a report titled The bolo and the squid (28). ‘You don’t try to absorb the shock in a mechanical structure, you just absorb it in four cables, on the order of a mile long, and the whole thing is spinning around so the cables are pulled out just by centrifugal force, and the ship is sitting behind, far enough away so it doesn’t get much of the blast,’ he explained. His Dimensional study of Orion-type spaceships (31) ‘was less serious,’ he added, ‘but it answers the question, “Did you explore the outer limits of this technology?” The answer is yes.’ He considered Radioactive fall-out from bomb-propelled spaceships (32) to be his most important report. ‘How many people would die of cancer from the Orion fallout?’, he asked. ‘That was for me the most important question to be answered, and I tried hard to answer it honestly. Unfortunately … my numbers never appeared in documents that outside critics might read, for the same reason that discussion of crew survival rates never appeared in any documents that we wrote at RAF Bomber Command during World War II.’

The plan was to launch a 4000 ton ship on a shakedown cruise to Mars in 1965, followed by a voyage to one of the moons of Jupiter or Saturn, where propellant could be replenished for the return. Dyson’s Trips to satellites of the outer planets (26) outlined these mission profiles, with Saturn’s Enceladus offering both abundant propellant and a velocity match with the arriving ship. ‘We knew very little about the satellites in those days,’ he wrote. ‘Enceladus looked particularly good. It was known to have a density of .618, so it clearly had to be made of ice plus hydrocarbons, really light things which were what you need both for biology and for propellant.’ Dyson, who argued for a voyage ‘like Darwin’s Beagle, which took five years’, envisioned ‘round-trips to satellites of Jupiter in 2 years, or to satellites of Saturn in 3 years, with takeoff and landing on the ground at both ends’.

After the limited nuclear test ban of 1963, Taylor kept up the hope of launching a treaty-compliant joint venture with the USSR. Even Niels Bohr lent his support, but without a mission from NASA, Orion came to an end. Dyson published an obituary in Science under the title ‘Death of a project’, with a one-sentence abstract: ‘Research is stopped on a system
of space propulsion which broke all the rules of the political game’ (40). In 2012, he revisited the questions of ‘whether I believe Orion has a future . . . whether I share a hope that some new version of Orion might take us to the stars . . . [and] whether our dreams of fifty years ago are dead. The answer to all three questions is no’ (88).

**ARMS CONTROL**

At the end of his first summer at General Atomic, Dyson received his security clearance and was flown to Los Alamos the next day. ‘To my amazement they simply stuffed me with all their information about bombs,’ he remembered. ‘I hadn’t asked for that, I wasn’t particularly interested in bombs. They wanted to tell me everything they’d been doing, as if they’d just sort of been burning to talk about this to somebody, all the designs that they had done and what they were planning to do.’ A similar visit to Livermore National Laboratory—where ‘they were really fooling around, like a fireworks show, you could shoot up almost anything you wanted!’—convinced Dyson that the arms race was being driven not only by the contest between the US and the USSR, but by the rivalry between the national labs. It had become a team sport.
Dyson had opposed a test ban treaty until he realized that every test was being conducted to answer an important question, but after each test at least two new questions turned up. Testing was growing exponentially and had to be stopped. He spent the summers of 1962 and 1963, while the negotiation of a test ban treaty was under way, at the Arms Control and Disarmament Agency in Washington DC, assigned to Frank Long, chief of the Science and Technology Bureau, who went to Moscow with Averell Harriman to negotiate the terms. President Kennedy was determined to reach an agreement, leaving it to Long and Harriman to decide where to draw the line. ‘The Russians insisted that peaceful explosions had to be included in the ban,’ Dyson explained. ‘The United States insisted that they shouldn’t. And at that point, just by accident everybody else was away at the weekend and I happened to be there. And the question came back from Harriman through Long to Washington: “Can the United States accept giving up on peaceful explosions?” I was alone in the office. I thought very hard: “This is the death of Orion, and is it right or is it wrong?” And I said, “Yes, sure we can.” The treaty went through, very fast, within a matter of days.’ He remained an advocate for unilateral nuclear disarmament from then on. The pacifists welcomed his conviction that nuclear weapons are inherently immoral, and the warriors respected his technical arguments that they are ineffective in fighting real wars.

JASON

JASON, a group of elite scientific advisors to which Dyson belonged for 60 years, has no meaning as an acronym, no fixed home and no defined membership requirements. ‘The government didn’t want or need more help,’ said Harold Lewis, one of the founding members of the group. ‘They wanted and needed different help (Lewis 2006).’

Classified problems constitute about half of its work. JASON reviews have helped end a long series of misguided projects, some defying the laws of physics, that were growing in secret and technically flawed. Dyson viewed adaptive optics (52) and laser propulsion (55) as his most constructive JASON-sponsored work. In 1972 he joined a JASON study into whether ground-based telescopes could correct for the turbulence of the atmosphere in real time. He developed both a general theory of optical image improvement and a specific algorithm for obtaining the best image correction possible. ‘We take a “God’s-eye view” of the system,’ he wrote in a 128-page report, ‘calculating the degree of consonance between the actual wavefront and the perfect wavefront that is invisible to mortal eyes’ (52).

THE SEARCH FOR EXTRATERRESTRIAL TECHNOLOGY

‘It is easy to imagine a highly intelligent society with no particular interest in technology,’ Dyson noted in 1966. The search for extraterrestrial intelligence is a misnomer, he argued, because ‘when we look into the universe for signs of artificial activities, technology is the only thing we have any chance of seeing’ (43). When the search for alien radio signals began in 1959, he remembered ‘I got thinking, “Suppose the aliens don’t want to communicate? Could we also detect them without any cooperation from their end?” If you’re an advanced civilization using a lot of energy, you’d have to radiate the infrared as a way of getting rid of the waste heat. So I proposed looking for infrared sources to see if anything funny was going on.’
Dyson submitted a one-page paper to *Science*, suggesting that the search for alien civilizations should assume that ‘one by-product of their energy metabolism is likely to be the large-scale conversion of starlight into far-infrared’, and speculating that ‘within a few thousand years of its entering the stage of industrial development, any intelligent species should be found occupying an artificial biosphere which completely surrounds its parent star’ (33). In requesting Oppenheimer’s approval to publish what was ‘certainly not Advanced Study’ but also ‘not quite crazy’, he wondered if it ‘would be better if the title were changed to “Search for Point Sources of Infra-Red Radiation” or something similarly unsensational’. With ‘artificial’ remaining in the title, the idea became known as the Dyson sphere.

None has been discovered so far. ‘We are not interested in guessing what an average technological society might look like,’ Dyson later added. ‘We have to think instead of what the most conspicuous out of a million technologies might look like. To be skeptical does not mean that one should not search’ (39, 43).

**ORIGINS OF LIFE**

‘In the long run biology will be more exciting, but not yet,’ Dyson advised Francis Crick FRS, who switched to biology from physics against Dyson’s advice (80). Crick later became a fellow at the research institute established in La Jolla, California, in 1960 by Jonas Salk with proceeds from the polio vaccine and 27 acres granted by Mayor Charles Dail of San Diego, a polio survivor himself. While awaiting construction of their headquarters, the Salk Institute occupied some war-surplus army barracks on the Torrey Pines mesa—the style of accommodation that suited Dyson best. When Dyson returned to La Jolla for a sabbatical in 1964–65, teaching at the new University of California campus also in temporary residence at the former army base, he developed a friendship with Salk Institute fellow Leslie Orgel FRS, an experimental chemist and one of the pioneering theorists of the origins of life.

In an undated ‘Model for pre-biotic evolution’, credited to his conversations with Orgel, Dyson showed how ‘imperfectly replicating molecules may evolve into perfect replicators by a process of uphill diffusion against the gradient of probability’. Even under generous estimates as to monomer population size, polymer length and the number of chances at synthesis over geological time, Dyson calculated that ‘to synthesize a perfect polymer molecule in a single step from the primaeval soup requires a miracle’. Given some initial tolerance for imperfect reproduction, however, ‘without invoking miracles . . . the evolution of a perfect molecule is an almost instantaneous event’ (89). This was followed by ‘a simple abstract model which has unexpected evolutionary properties’, despite the absence of either natural selection or genetic drift. ‘Even without selection there is a strong and consistent tendency to evolve chemically precise structures,’ Dyson observed, and ‘a perfect creature may evolve out of random polymers over many generations without requiring a single improbable event to occur. The creatures evolve monotonically toward perfection as if they know in advance the goal they are striving to reach’ (54).

In 1981, at the invitation of Martin (now Lord) Rees FRS, Dyson developed a more detailed model, noting that if the number of species of monomer is less than nine, order and disorder are unable to coexist, and nothing happens no matter how long you wait. Life as we know it uses four nucleotides and 20 amino acids, which Dyson cited as evidence that proteins came first and nucleic acids followed, or, as he concluded, ‘cells came before enzymes.
enzymes before genes’ (58). Dyson’s subsequent Tarner Lectures were published as a small book, *Origins of life*, followed by an updated edition in 1999 including new developments such as ribosymes and the discovery of thermophilic undersea life (61, 76). In his dual-origin hypothesis, metabolism comes first, with primitive cells capable of crude statistical self-reproduction serving as hosts for parasitic or symbiotic self-replicating molecules that eventually became the genetic apparatus of the cell. Tolerance for error led the way, and error correction followed. ‘Either life began only once, with the functions of replication and metabolism already present in rudimentary form and linked together from the beginning, or life began twice, with two separate kinds of creatures, one kind capable of metabolism without exact replication, the other kind capable of replication without metabolism,’ Dyson explained. ‘If we admit that the spontaneous emergence of protein structure and of nucleic acid structure out of molecular chaos are both unlikely, it is easier to imagine two unlikely events occurring separately over a long period of time’ (61).

Dyson included a graph where the number of species of monomers increases along the x-axis and the effectiveness of the polymer catalysts increases along the y-axis. The fitness landscape is divided along the diagonal into an upper region, labelled ‘Immortal: Garden of Eden’, where populations settle into permanently ordered states, and a lower region, labelled ‘Dead: Cold Chicken Soup’ in 1985 and ‘Dead: Hot Sulphur Soup’ in 1999, where populations never make the jump to ordered states. In the transition zone, populations of molecules are able to move both ways. ‘As soon as the droplets described by the model begin to exhaust the available resources and to compete with one another for survival, Darwinian evolution begins,’ Dyson concluded. ‘Darwinian selection requires death, and death is the transition from order to disorder. Life had to invent death to evolve’ (76).

**CAN WE CONTROL THE CARBON DIOXIDE IN THE ATMOSPHERE?**

In October 1955 a conference on the dynamics of climate was held at the IAS, including a discussion of ‘the theory that the carbon dioxide content of the atmosphere has been increasing since the beginning of the industrial revolution, and that this increase has resulted in a warming of the atmosphere since that time’. In his opening address, Oppenheimer ‘drew a parallel between the present conference dealing with problems of the general circulation of the earth’s atmosphere and the conference held at Los Alamos, New Mexico, in preparation for work on the atomic bomb’ (Pfeffer 1960). The conference was hosted by the IAS computational meteorology group, led by Jule Charney under a temporary appointment at IAS. ‘The meteorologists were the people making the most creative use of the computer,’ noted Dyson, who lobbied unsuccessfully for a permanent appointment on Charney’s behalf. ‘We started climate studies here. He would have created a world center for climate studies, long before it became fashionable, and that’s what I would have liked to have done. But the decision to close the project down had already been made.’

In 1975, Alvin Weinberg, post-war director of Oak Ridge National Laboratory and founder of the associated Institute for Energy Analysis, began investigating the effects of increased carbon dioxide in the atmosphere, and brought Dyson on board. Dyson’s report, titled ‘Can we control the amount of carbon dioxide in the atmosphere?’ concluded that ‘the carbon dioxide generated by burning fossil fuels can theoretically be controlled by growing trees’ (53). He assumed it ‘inevitable that we shall continue for many decades to burn fossil fuels and to
increase the level of atmospheric CO2’, and entirely possible ‘that with the rising level of CO2 we run into an acute ecological disaster. Would it then be possible for us to halt or reverse the rise in CO2 within a few years by means less drastic than the shutdown of industrial civilization?’ He believed the answer was yes. ‘Since the annual photosynthetic turnover is 20 times larger than the annual increase in atmospheric CO2, it should be possible in case of a world-wide emergency to plant enough trees and other fast-growing plants to absorb the excess CO2 and bring the annual increase to a halt’ (53).

‘The long-term response, if such a catastrophe becomes imminent,’ he argued, ‘must be to stop burning fossil fuels and convert our industry to renewable photosynthetic fuels, nuclear fuels, geothermal heat and direct solar-energy conversion.’ But a complete shift, ‘too slow to avoid a CO2 disaster’, would take 50 years. ‘An emergency plant-growing program’, however, could ‘provide the necessary short-term response to hold the CO2 at bay while the shift away from fossil fuels is being implemented’, and ‘the estimated costs, when converted into a tax on polluters of the atmosphere, turn out to be easily supportable’ (53). Dyson’s paper was ignored. ‘My argument pleased neither side in the ongoing debate,’ he explained. ‘The conservatives rejected it because I said that large-scale action by governments might be necessary. The gloom-and-doom environmentalists rejected it because I said the situation was not hopeless’ (67).

Through JASON, Dyson was kept updated on the work of Charles Keeling and other atmospheric scientists at Scripps. ‘Keeling calculates that 57 percent of the fossil-fuel carbon has been retained in the atmosphere, while 43 percent has gone somewhere else,’ Dyson noted in 1990, citing this missing carbon as grounds for hope. ‘If nature, without our help, already by natural processes counteracted half of the damage we are doing to the biosphere, this encourages us to believe that by intelligent cooperation with nature we may be able to counteract the other half.’ He argued for ‘mobilizing international efforts on a grand scale. If trees and topsoil are nurtured, as for a variety of ecological and economic reasons they ought to be nurtured, the growth of carbon dioxide in the atmosphere will be slowed down or halted incidentally. I am firmly on the side of “Action First”’ (67).

Despite his acknowledgement of anthropogenic global warming, Dyson declared himself a heretic and added his voice to the anti-consensus side. His views that computer models are good for understanding climate but not predicting it, and that beneficial effects of carbon dioxide should be taken into account, out-shadowed his analysis as to how the problem could be solved (81). Unlike the debate over nuclear weapons, where Dyson took a position respected by both sides, the climate debate left no middle ground.

**Prisoner’s dilemma**

In 2011, astrophysicist, computer scientist, and JASON colleague William Press was running a computer simulation of the Iterated Prisoner’s Dilemma, a two-player game that is the model organism of choice among both theoretical biologists and human behaviourists studying the evolution of cooperation in all forms. He was hosting a contest between memory-one strategies, where the players remember one preceding round of the game. The model was structured as an eight-dimensional hypercube, and kept crashing on a particular four-dimensional hyperplane. Press reviewed the entire program but could not find a bug—after it had crashed 1000 times ‘that was when I knew that I needed Freeman Dyson’ he
confessed. ‘The exact complement to computer intelligence, as yin to yang, is Freeman Dyson intelligence. This problem needed both kinds. I emailed a description of my puzzling results to Freeman. A day later, he sent back a note with the general result all worked out’ (Press 2021).

Dyson had seen that the outcome of the game depended on a matrix whose determinant could be set to zero by a malevolent player, with results the opponent cannot escape. Press and Dyson termed these ‘Zero Determinant Strategies’, and, as they wrote in the introduction to their resulting paper, ‘a player X who is witting of these strategies can (i) deterministically set her opponent Y’s score, independently of his strategy or response, or (ii) enforce an extortionate linear relation between her and his scores. Against such a player, an evolutionary player’s best response is to accede to the extortion. Only a player with a theory of mind about his opponent can do better, in which case Iterated Prisoner’s Dilemma is an Ultimatum Game’ (82). Press and Dyson had poked a stick into both the core model and the core beliefs of evolutionary game theory. It took about two years for the field to settle down.

**Missed opportunities and unfashionable pursuits**

In two characteristic essays, Dyson catalogued a series of instances where mathematicians had failed to grasp breakthroughs that were right in front of them (51) and explained why science requires nonconformists to advance (59). An undercurrent of missed opportunity—from the failure to reduce bomber losses to the failure of Project Orion to the failure to unravel the Riemann hypothesis—ran through Dyson’s own nonconformist career. He saw himself as a craftsman rather than a deep thinker, who just happened to be at the right place at the right time with QED. He helped lead the way in the application of symmetry groups to particle physics which culminated in the standard model, but as the enterprise grew fashionable and the SU(6) model he was working on was superseded by the quark model, he left the field (41).

Dyson preferred to work on problems that could be solved in weeks or months, not years. He divided the scientific temperament into two tribes (80). ‘Unifiers are people whose driving passion is to find general principles which will explain everything. They are happy if they can leave the universe looking a little simpler than they found it,’ he wrote. ‘Diversifiers are people whose passion is to explore details. They are happy if they leave the universe a little more complicated than they found it’ (64). He sided with the diversifiers, favouring a Universe where a multitude of theories coexist. He subscribed to the principle of complementarity—that opposing views can both be true at the same time—and was against consensus in all forms. ‘If we speak with one voice, the aliens will not hear much that is interesting,’ he warned (69).

Dyson’s openness to controversial hypotheses elevated him to something of a patron saint among thinkers on the fringe. He counted misfits, underdogs and even a few cranks among his friends, under the assumption that ‘it is better to be wrong than to be vague’.

**Books**

In April of 1949, *The New Yorker* sent its Talk of the Town correspondent on the train down to Princeton to visit the IAS, where they ‘saw Oppenheimer from a distance . . . and were introduced to several thinkers occupied with their labors. One was doodling on a scratch pad, presumably developing a momentous equation; one was staring out the window at four crows in a field; and a third was writing a letter to his mother on a portable typewriter.’ The typist was
Dyson, who 20 years later would be writing for *The New Yorker* himself. Jeremy Bernstein, *The New Yorker*’s science correspondent, introduced Dyson to editor William Shawn, who commissioned a reflection on Dyson’s wartime experiences at Bomber Command. Dyson’s ‘Memoirs of a pacifist war criminal’, published as ‘The sellout’ in 1970 (47), gained the attention of the Alfred P. Sloan Foundation, who were launching a programme to further the public understanding of science by encouraging scientists to write books. *Disturbing the universe* was their first project (57) with Dyson going on to publish 13 more books (60, 61, 64, 70, 72, 73, 75, 76, 78, 80, 85, 86, 87).

**Family**

Dyson’s priorities, in descending order, were family, friends and work. He wrote weekly letters to his father, mother and sister Alice until their deaths. His marriage to Verena Huber-Dyson (née Huber, in 1950) ended in divorce; his marriage to Imme Dyson (née Jung, in 1958) lasted 62 years. He was proud to have raised six professionally successful children, and a stepdaughter, not one of whom obtained a PhD. Dyson’s favoured model for child development was 10-year-old Emily left among a ship full of pirates to fend for herself in *A high wind in Jamaica* (Hughes 1929). He approached his own children as an experimentalist: changing the initial conditions one at a time, staying out of the way and observing the results (figure 8).
TIME WITHOUT END

The IAS is known among impoverished graduate students at nearby Princeton University, who sometimes attend seminars, as the Institute for Advanced Lunch. Dyson rarely missed lunch. On 25 February 2020, while carrying his tray of food to his customary table in the dining hall, he tripped and fell, hitting his head, and died three days later in the Princeton hospital. He left an unfinished calculation concerning a conjecture in population genetics, sparked by a discussion with William Press, on his desk.

On 10 June 1978, Dyson had been beaten and robbed while walking down C Street to a meeting of the National Academy of Sciences in Washington DC. ‘And now, at this unlikely moment, my spirit is filled with peace,’ he described his thinking at the time. ‘The green leaves and the blue sky are beautiful. Everything else fades into insignificance. This life is good and this death is good also. I am a leaf like the others. I am ready to float away on the blue wave of eternity’ (66).

While recovering from his injuries in 1978, Dyson began an investigation into whether the laws of nature allow life and intelligence to continue for eternity or not. He calculated four possible fates for the Universe as a whole. ‘If I were compelled to choose one of the four alternatives as more likely than the others, I would choose (ii),’ he concluded. ‘If (ii) is correct, human-sized objects will disappear with the lifetime $10^{\wedge}10^{\wedge}26$ years, but dust grains with diameter less than about 100 microns will last for ever’ (56).

AWARDS AND HONOURS

1952 Fellow of the Royal Society
1964 Member of the US National Academy of Sciences
1965 Dannie Heineman Prize, American Institute of Physics
1966 Lorentz Medal, Royal Netherlands Academy
1968 Hughes Medal, Royal Society
1969 Max Planck Medal, German Physical Society
1970 J. Robert Oppenheimer Memorial Prize, Center for Theoretical Studies, Miami, USA
1975 Corresponding Member of the Bavarian Academy of Sciences
1977 Harvey Prize, Technion, Haifa, Israel
1981 Wolf Prize in physics
1988 Gemant Award, American Institute of Physics
1989 Honorary Fellow of Trinity College, Cambridge
1989 Associé Étranger de l’Académie des Sciences, Paris, France
1990 Britannica Award for dissemination of knowledge
1990 Matteucci Medal, Accademia Nazionale delle Scienze dei Quaranta, Rome, Italy
1991 Oersted Medal, American Association of Physics Teachers
1994 Wright Prize, Harvey Mudd College
1995 Enrico Fermi Award
1996 Lewis Thomas Prize
1996 Antonio Feltrinelli International Prize, Accademia Nazionale dei Lincei, Rome
1998 Joseph A. Burton Forum Award, American Physical Society
2000 Templeton Prize for Progress in Religion
Acknowledgements

Dyson left a summary of his scientific work that has been drawn upon heavily here (72). His role in the development of quantum electrodynamics has been chronicled by Silvan Schweber (Schweber 1994), who also, with Graham Massey and Christopher Sykes, recorded Dyson’s definitive oral history, archived at https://www.webofstories.com/play/freeman.dyson/1. I thank William Press, Jennifer Chayes and Ashutosh Jogalekar for comments on the manuscript, and Lord Martin Rees FRS and Peter Goddard FRS for encouragement along the way.

The frontispiece portrait of Dyson in Bellingham, USA, was taken in August 1993 by Ann Yow-Dyson.

Author profile

George Dyson

George Dyson, the second of Dyson’s six children, is an independent historian of technology and the author of five books.

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