Freeman J. Dyson

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BIOGRAPHICAL *Memoirs*

A Biographical Memoir by
William H. Press
and Ann K. Finkbeiner

NATIONAL ACADEMY OF SCIENCES

FREEMAN J. **DYSON**

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By William H. Press and Ann K. Finkbeiner

Freeman Dyson was a mathematician, physicist, nuclear engineer, military advisor, arms control advocate, essayist, public intellectual, futurist, and visionary—not all at the same time. Dyson added proficiencies as the need arose. That such transformations were possible—even easy—for him says much about the man.

Dyson wrote prolifically about his own life, at first privately, and later publicly. From 1941, when he matriculated at Cambridge University, he wrote at least weekly to his parents and his sister, a wide-ranging correspondence that continued through many decades. When he began to write for a public readership in the 1970s, these letters were source material. Reserved in person, Dyson had little shyness in writing about himself—or toward others writing about him. Historian David Kaiser tells of requesting access to a letter of historical interest, and Dyson responding by handing him the key to his office and telling him to copy whatever he wanted. In Dyson's books and more than 160 essays, he draws frequently on anecdotes from his own life. His seven hours of video interview with physics historian Sam Schweber are another illuminating window. This wealth of autobiographical material is a blessing and curse to the memoirist. In what follows, we often find ourselves paraphrasing Freeman's own words. He is simply his own best source. And, because of our long friendship with him, we hear these words in our minds' ears in his own voice.

Early Life and Education

At his intellectual core, Dyson was a mathematician. Born in Crowthorne, Berkshire, England, on December 15, 1923, he seemed to have always loved playing with numbers. His serious study of mathematics started in high school, at Winchester, where the teachers left talented boys to study on their own. With his classmate James Lighthill (later a famous applied mathematician), Dyson worked through every page of Camille Jordan's decades-old, three-volume *Cours d'Analyse*, a dense work of some 1,800 pages, and similarly for G. H. Hardy and E. M. Wright's *An Introduction to the Theory of Numbers* and H. T. H. Piaggio's *Differential Equations*, with its hundreds of difficult exercises.

Dyson was among the generation in England that grew up under the twin shadows of the recent Great War and the clearly approaching Second World War. His father, George Dyson, a composer and college music educator later knighted, had served in the British army and written the field manual on hand grenades. His namesake uncle Freeman had been killed in the Great War at age thirty-three, before he was born. Based on what he'd heard firsthand of the carnage of the Great War, young Freeman watched the approach of the World War II expecting that he and all his friends would be killed. At age fifteen, he systematically calculated odds of ten-to-one that he would be dead in five years. At the same young age, he took a written examination and won a scholarship to Trinity College at the University of Cambridge, but he was persuaded to defer for two years until 1941.

By then, because of the war, Cambridge had few students. Younger faculty in mathematics had left to assist the war effort, and the older generation of pure mathematicians were left to carry on instruction in tiny classes. Dyson attended lectures (across a small table) by Hardy, John E. Littlewood, and Abram Besicovich in math. Hardy became a mentor; Besicovich, who is today less well remembered, became Dyson's mentor, friend, and billiards opponent. Dyson also attended lectures by Paul Dirac and Arthur Eddington.

At Cambridge, Dyson later wrote, he was taught nineteenth-century mathematics. His mentors knew little and cared less about the twentieth-century mathematics that was flourishing in France before the war, with new general concepts and a far more abstract style. Fortuitously, nineteenth-century math—especially analysis, in which he had been immersed since Winchester—turned out to be exactly the right prerequisite for Dyson's contributions to physics. But number theory, in the style of Hardy, was Dyson's first and greatest mathematical love, the field in which he published occasional results for the rest of his life.

Briefly a fierce pacifist, Dyson realized that the example of Vichy France showed that the English, if invaded, would have to choose between collaboration and resistance. The choice of resistance was for him obvious. He joined the Army officers' training corps and was at Cambridge for less than two years before qualifying for a hurried wartime undergraduate degree. After an interview with scientist-novelist C. P. Snow (whom Dyson took to be no more than a functionary), he was assigned to the new Operational

Research Section of the Royal Air Force Bomber Command at High Wycombe, where he spent two demoralizing years. From the experience he took away two propositions: That most high-ranking military officers were stupid; and that, for the good of the world, it was every scientist's moral duty to engage the military rationally, their stupidity notwithstanding. He wrote about his wartime experiences in a manner factual, sober, and convincingly anti-war. "Bomber Command gave me a lifelong commitment to making sure this sort of tragedy doesn't happen again," he said later. "I'm a passionate fighter for sanity as far as military questions are concerned."

At war's end, Dyson was already starting to be interested in physics, but mathematics still beckoned. Number theorist Harold Davenport suggested that Dyson earn his chops on a conjecture by Carl Ludwig Siegel about how often the difference between any real algebraic number and its rational approximation p/q could be smaller than $1/q^2$. Dyson decided that the Siegel conjecture would determine his future. If he succeeded in proving it, he would be a mathematician. If he failed, he would become a physicist. After three months, he admitted failure. The die was cast.

Could he have become a mathematician of the first rank? Dyson himself later thought not. His greatest satisfaction came from the beauty in the technical details of a difficult proof. He was happiest when other people gave him difficult, well-posed problems to solve. Then, he could unleash his formidable technical abilities. He was drawn to the particular and the ingenious, less to the generalities of mathematical structure that became so fruitful in post-War mathematics. Dyson could surely have been a productive mathematician of the second rank, but physics was different: it had well-posed, unsolved problems that were holding back the entire field. Solve any of these, and he would become famous. And so he did.

Dyson returned to Trinity College as a fellow. There, guided by Nicholas Kemmer, he became a physicist. Kemmer happened to possess the only copy in England of Gregor Wentzel's *Zur Quantentheorie der Wellenfelder,* published in Vienna during the war, and Dyson assimilated it. The previous year Dyson had similarly absorbed Walter Heitler's *Quantum Theory of Radiation,* a book that laid out clearly the subject's unsolved problems. Quantum field theory (QFT) was a subject little known in England—even less in America. Dyson, albeit in obscurity, now probably understood more of it than anyone else in the English-speaking world. Kemmer told Dyson that he should go to Cornell University in the United States and study with Hans Bethe, who had emigrated from Germany (by way of England) in 1935. This advice was seconded by Rudolph Peierls when Dyson traveled (by motorbike!) to Birmingham to see him. G. I. Taylor completed the picture for him: "Cornell is where all the brightest people from Los Alamos went when the war was over." That was good enough. Aided by a Commonwealth Fellowship, traveling in style to New York on the Queen Elizabeth, he arrived in Ithaca in the fall of 1947.

Move to the United States

Quantum mechanics (QM), as first formulated in the 1920s by Erwin Schrödinger and Werner Heisenberg, was sufficient to solve a multitude of problems in physics and chemistry, but—not least because it was incompatible with Einstein's Special Theory of Relativity—it was understood from the start to be an approximation. For example, QM could not adequately explain the so-called fine-structure splitting of the hydrogen spectrum. Dirac's triumph in the late 1920s was to formulate a relativistic quantum theory that, among other successes, could calculate the hydrogen spectrum as then measured. But the Dirac equation still treated electromagnetism classically. It must itself be only an approximation. In the 1930s, theorists including Dirac, Wolfgang Pauli, Heisenberg, and (the young) Hans Bethe searched for a quantum field theory (QFT) that could be an exact law of nature.

By the end of the decade, when war intervened, there was agreement on what a high-level QFT should look like. What was unclear was whether this QFT could, in practice, yield clear predictions about experiments. The theory was highly nonlinear. Its perturbation expansion exploded as a profusion of terms with increasing numbers of multiplicative factors whose exact order mattered and was not entirely clear. Even worse, some terms, especially ones associated with the self-energy of a particle's electromagnetic field, were manifestly infinite. Did this mean that the theory was incomplete, that "new physics" was required to cure the infinities? Or was there some as-yet-undiscovered way to make the infinities go away?

American physicists, motivated more by experiment than by theory, paid little attention to these arcane European QFT debates. The development of microwave technologies during World War II had opened new opportunities for high-precision measurement. At Columbia University, Willis Lamb and his student Robert Retherford measured the tiny difference of about 1000 megacycles (in the units of the time) between the hydrogen $2S_{1/2}$ and $2P_{1/2}$ states, which Dirac's theory predicted to be exactly degenerate. Bethe within weeks published a QFT calculation that gave a value close to that observed; but he needed to pick and choose which infinities to cancel in an ad hoc way. The specific version of QFT that applied to electrons and photons, now being called quantum electrodynamics (QED), was suddenly a hot field. Cornell was its epicenter.

Dyson found Bethe, Cornell, and Ithaca all welcoming. He admired what he saw as American brashness and egalitarianism. Bethe was happy to pose problems for him, prodding him to find a cure for QFT's deficiencies. But in an unhappy accident of history, Bethe had already assigned the problem of the Lamb shift to another student, and it could not be taken back. Dyson was assigned a less-physical model. Perhaps if he had he been given the full problem, Dyson might have invented QED independently; but that didn't happen. Also at Cornell was the young Richard Feynman—not yet thirty and, for Dyson, charismatic—who was developing his own version of the theory, one that Dyson found maddeningly obscure, seemingly based on little drawings rather than equations. Dyson volunteered to teach Feynman "real" field theory—and was rebuffed.

At the watershed conference at Mount Pocono in the spring of 1948, Harvard's Julian Schwinger awed the audience with a calculation of the electron's discrepant magnetic moment in agreement with Polykarp Kusch and Henry Foley's new experimental result at Columbia University; but Schwinger's Green's-function calculation seemed so heroic as to hardly constitute a useful complete theory. Indeed, Robert Oppenheimer famously remarked that where most lecturers explained how to do something, Schwinger explained that only he could do it. Feynman's talk was even less-well received. With his diagrams, he too could obtain results in agreement with experiment; but his approach seemed completely ad hoc, as if he were picking and choosing terms to get the right answer.

Then, soon after the conference, Oppenheimer circulated copies of a paper he had received from Shinichiro Tomonaga in Japan, who, against all odds, had managed to work on QFT during the war. Tomonaga's results also agreed with the new experiments. Within a few months, the world went from having no viable theory of QED to having three. Were these the same theory? Or were they three different theories, the matter to be resolved by future experiment?

Dyson recognized in Tomonaga's paper QFT as he had absorbed it, albeit with a new approach towards cancelling the self-energy infinities. Unlike Schwinger's and Feynman's obscurities, it was an approach that he himself might have found. In June 1948, Dyson attended Schwinger's lectures at the University of Michigan's physics summer school. The morning lectures were in accord with Oppenheimer's quip; but in the afternoons, Dyson found Schwinger approachable and willing to explain what his theory was really about—and in Dyson, Schwinger must have found the perfect student. Dyson soon understood that, behind their baroque exposition, Schwinger's Green's functions were QFT commutators, and Schwinger's theory was identical to Tomonaga's.

That same summer, Dyson accompanied Feynman on a now-famous road trip from Cleveland to Albuquerque. Feynman talked nonstop, about physics and about life. In Feynman's mind, his diagrammatic approach—vertices where particles interacted and "propagators" between their interactions—was a consequence of the more general fact that a quantum wave amplitude could be viewed as a sum of amplitudes of all possible histories, now known as a path-integral formulation. Dyson saw that the diagrams might embody a crude approximation to path integrals, but how could they possibly be exact? The two argued for days, as Dyson also absorbed the logic behind the seemingly arbitrary choices that Feynman made in his calculations.

Early Career

At the end of the summer, which he spent in Berkeley, Dyson headed by Greyhound bus for Princeton, where he was to spend several months at Oppenheimer's Institute for Advanced Study (IAS). Somewhere in Kansas or Nebraska (a rare divergence in his retellings), the answer came to him in a flash: Feynman's diagrams represented exactly the same perturbation expansion as Schwinger's Green's functions and Tomonaga's commutators. Feynman's seemingly ad-hoc rules were exactly the same as the rules of operator time-ordering in the other two. The three QED theories were all identical. Viewed with this clarity, QED did not require new physics. It was a logical development of "old QFT," a triumph (as Dyson later emphasized) of conservativism in physics.

The year 1949 saw the publication, four months apart, of Dyson's two greatest contributions to physics—first, the unification of the Feynman-Schwinger-Tomonaga theories into a single theory; and second, a proof that the same techniques would cancel the infinities not just to the orders investigated thus far, but to all orders. Dyson was an instant celebrity, everywhere in demand as a speaker. The offer of a coveted faculty position at Columbia followed. His Commonwealth Fellowship required him to return to the United Kingdom, however. He did so and spent two years with Rudolf Peierls at the University of Birmingham.

There, Dyson worked on two projects that, with hindsight, were doomed to fail. First, he attempted to show that QED was not just renormalizable (i.e., its infinities canceling to all orders), but was actually finite—a convergent series. A modern view is that QED, as the low-energy approximation to a unified theory at much higher energies, is surely divergent—and that the question of its convergence is at best a mathematical curiosity. Second, Dyson attempted to construct a QED-like field theory for nucleons and mesons, that is, for the strong nuclear interactions. This was doomed by the fact that nucleons and mesons are not in fact elementary particles, but, as we now know, composites of quarks and gluon fields, and by the fact that the role of non-Abelian gauge theories was not yet appreciated by physicists.

Dyson returned to Cornell in 1951 as a professor. He never bothered to get a Ph.D. But by then, QED was over for him—he had convinced himself that its expansion was not convergent. He continued to work on the problem of strong interactions, however, with a small army of graduate students. Understanding that strong interactions would defeat any series expansion, his group employed a different set of approximations, so-called Tamm-Dancoff methods. By Herculean calculation, they obtained theoretical results not too different from then-recent meson experiments of Enrico Fermi. In the spring of 1953, Dyson traveled to Chicago to show Fermi their results. Noting the superficial agreement, Fermi asked Dyson how many free parameters were in his theory. Dyson counted them: four. Fermi responded: "I remember my friend Johnny von Neumann used to say, with four parameters I can fit an elephant, and with five I can make him wiggle his trunk." This aphorism has since become famous. But we might harbor suspicions about whose quip it actually is, since Dyson, its only source, waited four decades to mention it.

Fermi's rejection was a turning point for Dyson. Winding down his group at Cornell, he accepted a professorship in late 1953 at Oppenheimer's IAS, a position he held for more than six decades. He abandoned strong interaction physics and never formally took on another Ph.D. student. When he became publicly famous, it was not for his subsequent contributions to theoretical physics. Still, some of those are worth mentioning. In the mid-1950s, from Charles Kittel, Dyson became interested in ferromagnetism. His development of a theory of spin waves not only introduced field-theoretic methods to the field, but also lastingly raised the standard for mathematical rigor in all of condensed-matter physics.

In the 1960s, Dyson took hold of Eugene Wigner's speculation that the energy levels of a complex nucleus might be described statistically by a random Hamiltonian matrix and developed a beautiful, quite general theory of random matrices. The original application never gained much traction. The work might have faded into obscurity but for Dyson's regular attendance at the IAS afternoon teas. In 1972, mathematician Hugh

Montgomery found a puzzling statistical property in the zeros of the Riemann zeta function—potentially shedding light on the greatest unsolved problem in mathematics, the Riemann Hypothesis. At tea, Dyson recognized exactly that property from his random matrices. The similarity breathed new life into a mostly forgotten conjecture of George Pólya: that the zeta zeros corresponded to eigenvalues of an unbounded self-adjoint operator. It remains possible that Dyson's work will be seen as a steppingstone to a future proof.

Also in the 1960s, with Andrew Lenard, Dyson supplied the first rigorous proof that an ensemble of quantum-mechanical fermions with positive and negative charges had a ground state with finite density—in other words, that stable matter was possible. While this might seem superficially obvious—not least because of the Pauli exclusion principle—the actual proof was devilishly difficult, one of the most sophisticated applications of analysis to theoretical physics up to that time.

About the time Dyson moved away from QED and into other sub-fields of physics, his career made an even sharper turn, into scientific advising for the U.S. government and its military. Though sharp, the turn was not surprising. Cornell was not only the epicenter of work on QED, it had also been the landing place of many Los Alamos scientists and was thus a hotbed of discussions about the control of nuclear weapons; and at IAS Oppenheimer, with many earlier government connections, was an important influence. Dyson became interested in the peaceful uses of nuclear energy and in 1956 was invited to spend a summer in La Jolla by Frederic de Hoffmann, a young physicist who had worked on the Manhattan Project and whom Dyson knew from Cornell, at de Hoffmann's new company, General Atomic. Dyson was charged with figuring out how to build a commercial nuclear reactor that could neither blow up nor melt down, and thus could be used safely by non-specialists in hospitals, industry, and research. Dyson knew nothing about nuclear reactors but was eager to learn. He joined de Hoffmann's small group of chemists, engineers, and physicists, including Edward Teller, whom de Hoffman had known from the Manhattan Project.

That first summer, the group met in a red schoolhouse in San Diego. Dyson liked General Atomic and found working on a reactor unexpectedly enjoyable. He spent the summer of 1956 working on the reactor called TRIGA (for Training, Research, Isotopes, General Atomic). The group in charge of the reactor's design, which included Dyson, was led de facto by Teller. Teller and Dyson disagreed over the design and "fought like cats." Teller was accustomed to winning arguments by threatening to quit, but in the arguments over design, Dyson prevailed. Dyson thought Teller a power-hungry, unscrupulous, spoiled brat. But Teller was kind when Dyson had to get rabies shots, played the piano sensitively, and had brilliant ideas, and he was altogether somebody Dyson could still happily work with. The two remained friends. In later years, Dyson defended Teller and his actions to less-forgiving colleagues.

TRIGA's design relied not on engineering details, but on the physics of neutrons interacting with atomic nuclei to halt a nuclear runaway. When it was demonstrated three years later at a dedication, it immediately powered itself up to the edge of catastrophe, then, in milliseconds, quenched itself back down to a steady output, not by human control, but by the laws of physics. As of today, 66 TRIGAs have been built in 24 countries; it is the most widely used research reactor in the world.

The Orion Project and Military Research

Fatefully, de Hoffman invited Dyson to join another General Atomic project, a visionary nuclear-powered spaceship that was proposed by Ted Taylor who, at Los Alamos, had designed small nuclear weapons. The spaceship, called Orion, would be powered by ejecting and then exploding small nuclear bombs, thus heating an inert propellent, also ejected. On paper, Orion could carry large payloads all over the solar system. It would be one hundred times cheaper than ordinary chemical rockets. It could reach Mars, even Saturn, in weeks, not years. The project's motto was "Saturn by 1970."

Dyson had no doubt that the rocket would work technically. It also mattered to him that Orion was a peaceful way to use—and use up—nuclear weapons. In 1958 he took a one-year leave from IAS to work in San Diego on Orion's physics problems of radiation and hydrodynamics, in particular the radiation-coupled instabilities that might occur when the shock wave reflected off the flat pusher plate at the bottom of the rocket.

But the issue with Orion was of course fallout. Dyson calculated that, with a few kilotons per bomb, ascent through the atmosphere would require a few hundred kilotons of nuclear yield and spread radioactive fallout accordingly. In 1958 the Soviet Union and the United States were still testing, with brio, megatons of nuclear yield in the atmosphere. Orion's launches would raise the total fallout only by one percent, a release comparable to the radioactivity of trace uranium and thorium in coal burning. But, ultimately fatal to Orion's prospects, the United States and Soviet Union were moving towards an international treaty to ban atmospheric tests of nuclear weapons. Even after Orion lost its government funding, Dyson publicly supported the project, though support now meant arguing against the proposed test-ban treaty. He wrote a 1960 article in *Foreign Affairs* that was, in his own later words, a diatribe against the test ban. He took several years to change his mind. In 1962-63, Dyson worked summers for the new U.S. Arms Control and Disarmament Agency (ACDA). Now weighing a ban of the exponentially rising number of nuclear tests against nuclear-powered space travel, the test ban won. In 1963, he testified in favor of the test ban during a Senate hearing. The treaty was ratified.

About this time, Marvin Goldberger invited Dyson to join a newly formed advisory group called JASON, named for the mythic ancient Greek hero. Goldberger was a Princeton physicist who had also consulted for General Atomic. JASON was (and still is) a group of mostly academic scientists who met summers as paid consultants to answer technical questions for the U.S. government, especially the Department of Defense. Learning that he would not be needed at ACDA in further summers, Dyson became a JASON member. His reasons, he later said, were that he needed the money for his growing family, he wanted practical problems to work on, and, anyway, Princeton emptied out in the summers. These reasons can't be the whole truth, given that Dyson spent five to seven weeks during most summers for the next fifty years working on JASON studies, many related to the same global issues that he had already worked on and thought about.

In JASON, Dyson tended to work on science underlying arms control and strategic stability, including anti-submarine warfare (whose mirror-image was protection of U.S. strategic missile submarines) and ballistic missile defense. In later years, he worked on JASON's climate studies and on methods for countering terrorism and biological warfare. He loved shooting down scientifically unsound—sometimes truly crackpot—projects proposed by the military or their contractors, for example detecting submarines by their emission of neutrinos, or beaming gravitational waves as a weapon.

JASON's mode of operation was one with which Dyson was deeply comfortable: being asked to solve problems, each different from the last; working usually alone but in an interesting, talkative group that reminded him of graduate school at Cornell; and the chance to keep open communications between science and the military, a goal preserved whole from his experience in Bomber Command. "We should have a military that is sane and does sensible things and talks to the outside world," he wrote. Unlike some colleagues, Dyson remained a JASON member during the Vietnam War, though he tried to avoid direct war work. An exception was a study by Dyson, Steven Weinberg, and two others that debunked proposals by some government officials to use tactical nuclear weapons against North Vietnam. As scary as this sounds to us now, it was then equally terrifying to the JASON team. No one in government had requested the study, and it is unclear that it had any direct influence, though Dyson was happy they'd done it.

Decades later in the 1970s and 80s, and much more influential, was Dyson's work on adaptive optics, a technology applicable to both civilian astronomy and military surveillance of space. Observed through the turbulent atmosphere, images of a star, planet, or orbiting satellite twinkle and jitter with time. Adaptive optics was a system to correct the jitters. Light sensors detected the changes that turbulence causes in the brightness of the image of a bright star. Then, a flexible mirror backed by fast actuators responded to each change by creating the equal and opposite change in the mirror. That is, the mirror cancelled out any changes the atmosphere made to an image. Dyson developed an efficient algorithm to generate electronic instructions for the actuators, such that the whole system produced the best possible image correction. His solution to the problem, he said with uncharacteristic immodesty, was "a beautiful piece of mathematics."

Even with Dyson's approach, adaptive optics was practical only with bright reference stars, called guide stars, that allowed the turbulence to be measured. But everyone, astronomers and military both, also wanted sharp images of things that were dim. JASON member Will Happer knew that the ablation of meteors produced a layer of neutral sodium atoms at a height of about 90 kilometers in the atmosphere. Illuminating that layer with a focused laser tuned to a resonance of the sodium atom at a wavelength of 589 nanometers could produce a bright spot—an artificial guide star—close enough to any object of interest for an algorithm like Dyson's to work on both. Happer, Dyson, and several other JASON members, including Gordon MacDonald and Claire Max, worked on the problem. Because of its relevance to the missile defense initiative (popularly known as "Star Wars") in Pres. Ronald Reagan's administration, the whole laser guide star/adaptive optics system was tightly classified.

Max, who later became director of the University of California Observatories, spent years quietly lobbying for the system to be declassified. She and Dyson understood that laser guide stars and adaptive optics on astronomical telescopes with large, 8- to 10-meter mirrors could enable ground-based telescopes to achieve the kind of resolution that the Hubble Space Telescope delivered from space. The work was declassified in 1991. Today, seven astronomical telescopes with 8- to 10-meter mirrors use adaptive optics with laser guide stars, as will the upcoming group of "extremely large telescopes" with 20- to 40-meter diameters.

Later Years and Spiritual and Philosophical Writings

Up to the 1970s, Dyson's public writings—those apart from his scientific papers—were few and specialized. His published articles on arms control seem derivative of his sometime mentor Oppenheimer in style—closely argued, but narrow, pompous, and preachy. Then, in the 1970s, culminating with his first book, *Disturbing the Universe* (1979), a new Freeman Dyson emerged. This one wrote beautifully. He was engaging, visionary but direct, poetic but clear, and often controversial and contrarian—the expansive public intellectual whom we recognize as the much-admired Dyson of the next three decades. Where on Earth, or elsewhere, did this new Dyson come from? The mystery was resolved in part only two years before his death, with the publication of a volume of his letters over decades to his parents and sister. Wide-ranging and often elegantly written, these letters show that Dyson, the thinker, seeker, and essayist, was there all along.

Dyson's essays have various outward forms: personal recollections, thought pieces, biographical and historical sketches, book reviews, and combinations of these. They are collected in eight books published between 1979 and 2015, all of which attracted glowing reviews and significant readerships. They teem with ideas, far too many to summarize here. He loved the particular, the one-off, the exceptional, and he collected stories about kindred spirits. Worth mentioning here, however, are two threads in his published writings that are particularly illuminated by the finally published letters.

The first thread is Dyson's surprising spirituality. His self-description as "loosely attached to Christian beliefs by birth and habit" barely scratches the surface. From his mother, Dyson absorbed the idea of a universal "world soul," something beyond the reach of science that gave unity and purpose to the cosmos. Later, as a scientist, Dyson speculated that the world soul's imprint could be observed at three scales. He saw the probabilistic nature of quantum mechanics as a kind of primitive consciousness: "It appears that mind, as manifested by the capacity to make choices, is to some extent inherent in every atom." Human consciousness occupied an intermediate scale. And—after first expressing his reluctance to use the word God—he writes, "Atoms and humans and God may have minds that differ in degree but not in kind. We stand, in a manner of speaking, midway between the unpredictability of atoms and the unpredictability of God."

He called this his "personal theology." Other beliefs followed as plausible corollaries. Teleology (the idea that events might bend towards desired outcomes) was not incompatible with physics, he thought, if it acted on the largest of his scales—the plan of the Universe as a whole. For example, Dyson rejected the pessimistic idea that nuclear war, or any other apocalyptic event, might doom the human species. The reason, he wrote, was his belief that the Universe had a purpose and that our minds were a part of that purpose. Since the goodness of the Universe was revealed in our existence as observers, we could rely on the goodness of the Universe to allow us to continue to exist—these are his exact words. His use of the word *revealed* is telling.

The second thread is the surprisingly profound effect of the Orion project, and its abrupt cancellation, on Dyson's worldview. "The story of Orion is significant," he wrote soon after, "because this is the first time in modern history that a major expansion of human technology has been suppressed for political reasons." Although he later renounced this particular statement, he never gave up the view that progress was created by individuals and small groups, and that bureaucracies, committees, and consensus were the enemies of that progress.

We can speculate on why Orion was so consequential to Dyson. In a purposeful Universe, the spaceship (on which he had dared hope to travel even personally) could be seen as a bridge between nuclear weapons as evil and as good—indeed as part of a plan for the expansion of human minds into the cosmos. The demise of Orion meant that Dyson was fated to remain on Earth physically. The remaining alternative was to explore and evangelize the physical Universe and its world soul with his mind.

Dyson expected the Universe to be full of life—including intelligent life. He was enthusiastic about SETI, the search for extraterrestrial intelligence, and proposed new searches for infrared sources that might signal advanced civilizations harvesting the full power of their stars by surrounding themselves on Solar System scales with what we now call "Dyson spheres."

And, if the Universe was not already full of life, then humans ought to make it so. He imagined that most humans would ultimately live off-Earth on comets or asteroids, sources of carbon and water. Robotic butterflies, their wings solar sails, would self-reproduce and might become interstellar messengers. Genetic engineering, he thought, would produce vacuum-resistant trees that could grow on comets and warm-blooded potatoes that could grow on Mars. Dyson credited parts of his vision to J. D. Bernal, a mid-twentieth-century crystallographer and intellectual herectic, and to the science fiction writer Olaf Stapledon; but it was Dyson who turned the particulars into lasting popular memes.

Dyson wrote often about rebels overthrowing establishment views; amateurs succeeding where professionals failed; mavericks, dreamers, poets, loners, heretics—even (in one essay) believers in paranormal psychology. The valid modes of human experience included, for him, not just science, but also religion and poetry. He believed, with his friend and fellow British expatriate Oliver Sacks, that the experiences of people with autism and other cognitive disorders could illuminate our own thinking. Frequently, he described himself as a heretic. "I am proud to be a heretic," he wrote. "The world always needs heretics to challenge the prevailing orthodoxies." In an interview, he added, "I always feel uneasy if I happen to be joining the majority."

Climate Science Controversy

We may approach Dyson's views on climate change in this context. In his later years he was frequently branded as a climate science doubter—not as a compliment. Not one to back down, he responded sometimes immoderately, as in his summary of a Royal Society publication on the subject that he felt was directed against him: "In other words, if you disagree with the majority opinion about global warming, you are an enemy of science." It is worth separating Dyson's views into the scientific and the philosophical, as each played a role.

Dyson's first exposure to climate science was in JASON, in the mid-1970s. Early climate models were primitive. Were they so primitive as to be meaningless? Dyson, along with a few other JASONs, including Bill Nierenberg and Will Happer, thought so; and indeed some of the earliest models probably were. Even as the models became more credible over decades, Dyson stuck to this view. In 2005 he wrote of "enormous gaps in our knowledge, the sparseness of our observations, and the superficiality of our theories." Many of the basic processes of planetary ecology, he thought, were still poorly understood. He accused his critics of dogmatism, but he was dogmatic himself. His scientific views on this never changed and were only reinforced by his glee at being seen as heretical.

After making such scientific points (as he saw them), Dyson's argument tended to slide towards the philosophical. How could we be so certain that a hotter climate with increased carbon dioxide was bad? Yes, there might be significant transitional costs, but might not the final outcome be a more fertile, more hospitable Earth? A separate line of Dyson's argument criticized what he represented as the environmentalist "religion," that human-induced changes were intrinsically evil. Human settlement and wilderness were equally a part of nature, he argued. "Through human minds," he wrote, "the biosphere has acquired the capacity to steer its own evolution, and now we are in charge. Humans have the right and the duty to reconstruct nature so that humans and biosphere can both survive and prosper."

One may not agree with Dyson—in fact, we don't—but it is not hard to see how his inflexible positions on climate could be consistent with his self-described heresy and, interestingly, with his general theological thinking. The Universe was purposeful, and the human species was special. If there existed a teleological plan, then a human-remodeled biosphere might well be a part of it. Allowing or not allowing global warming should be a choice of mind, not a dogma. Where was the rational debate on this? He didn't see any such discourse taking place. Anyway, our biosphere was only one of the millions that would exist once we became a spacefaring species. In Dyson's view, global warming was not too big a problem for us to solve; it was too small a problem to get so worked up about.

Dyson wrote that family, friends, and work, in that order, were always the most important things in his life. We have chosen to say almost nothing about the first, in part because Freeman's feelings about family are documented so tenderly in his published letters. At the time of his death, Imme and Freeman Dyson had been married for more than sixty years.

After working with Freeman in JASON for many years, I (WHP) developed the habit of occasionally sending him well-posed mathematics problems that arose in my (largely computer-based) work that were beyond my abilities to solve analytically. Most of the time he thanked me, and that was the last that I heard about them. Once, in our joint work on the game-theoretical Iterated Prisoner's Dilemma problem, the seed took root and grew into a 2012 published paper that attracted some attention. Eight years later, on February 28, 2020, Dyson died of complications after a fall in the IAS dining room where he usually lunched with a group of young astrophysicists. Months after that, his son George Dyson sent me a photograph of the handwritten page of mathematics that was on top on his desk, seemingly the last thing he was working on. It clearly relates to a problem in the statistics of inheritance of polygenic traits that I had sent to him ten months earlier. He had worked on many things in the meantime. Why this particular problem was brought forward on that particular day we will never know. At age 96, as a mathematician, he was still having new ideas.

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