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Cosmology and High-Energy Astrophysics: A 50-Year Perspective on Personalities, Progress, and Prospects

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Abstract

In the 1960s, novel and increasingly powerful observational techniques opened up the field of high-energy astrophysics. Cosmology started to become an empirical science, and there was a resurgence in the study of general relativity. Martin Rees became a graduate student at the University of Cambridge during that period and subsequently held postdoc positions in the United States. He was therefore fortunate to have a close-up perspective on some of these developments and to interact with many senior figures who were spearheading these advances. He himself became a phenomenologist, contributing his own ideas to several topics in these fields and working with many collaborators. This article offers an assessment of some key subsequent developments and personal perspectives from a diverse career spanning more than 50 years.

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BEGINNINGS

My parents were both teachers—but not scientists. After the disruption of World War II, they made a rather bold move: They started their own small school. With finances limited to an unexpected inheritance, they rented a rural Victorian mansion in South Shropshire near the Welsh border. This is where I spent my formative years. The surroundings—with woods, a stream, and small lakes—were a wilderness, but it was wonderful for a small boy to explore. The mansion itself was, when we arrived, almost derelict. After renovations, the school opened its doors. I myself attended it until I was 13 years old. Despite the tension of being the teachers' son, it was a happy childhood, though a rather lonely one.

Unlike some who end up as scientists, I didn't form any firm ambitions in childhood. From an early age, however, I was interested in numbers—heights and sizes—and in mechanical things, especially cars. (I nerdishly studied motor magazines and catalogs.) And I was curious about natural phenomena. For example, on holidays to North Wales, I was fascinated by the tide tables. I had been told about the influence of the Sun and Moon on spring and neap tides. But I was puzzled by why the time of high tide varied from place to place along the coastline. Only much later did I understand this (the patterns of tides and currents in the Menai Straits—which separate the island of Anglesey from the mainland—are especially interesting). And I was perplexed by something even more mundane: When the water in a circular washing-up bowl is set spinning, why do the tea-leaves form a little pile at the center? I didn't understand this until my final undergraduate year, when a fluid dynamics course covered Ekman boundary layers, etc.

My parents rightly decided that I should leave home for my post-13 education. They enrolled me as a boarder at Shrewsbury School (about 30 miles away)—albeit at some financial sacrifice and despite ambivalence about traditional English "public schools" (actually private institutions). I was grateful to Shrewsbury for providing high-quality teaching, though my parents were right to be critical of the ethos of such establishments.

In English schools, specialization starts before age 16. My choice was motivated as much by being bad at foreign languages (and I've been embarrassingly lazy on that front throughout my life so far) as by enthusiasm for the sciences. I proved sufficiently good at mathematics to be accepted by Cambridge University to study that subject. But in retrospect, I wish I'd been less naive or had wider advice. I found myself among fellow students who clearly had an affinity and enthusiasm for mathematics that I couldn't share.

So I didn't find my undergraduate years intellectually fruitful; in retrospect, I would have done better to have chosen a much more varied menu of science courses. But I luckily did well enough in my undergraduate math courses to get a scholarship that allowed me to stay for a fourth year to undertake a masters-level course that offered a "shop window" into a range of applied topics. I had by then realized that I preferred to think in a more synoptic style about real phenomena. I attended courses in theoretical physics, fluid mechanics, and astrophysics; I even had a vague idea that I might become an economist and therefore went to lectures on statistics too.

Through sampling a disparate range of lectures, I learned a lot during that year. But this catholicity didn't serve me well in the final exam, where the best strategy would have been to focus on closely linked courses. So, though I passed, I didn't get a distinction (or star), which was the normal prerequisite for pursuing a Cambridge PhD. But here chance intervened. A man who had been allocated one of Cambridge's quota studentships decided instead to go to the University of Maryland. I was given the slot thereby released—and have felt grateful ever since to John Jackson (subsequently a lecturer at the University of Leicester and at Northumbria University) for his unwitting good deed.

RESEARCH STUDENT YEARS AND 1960s BREAKTHROUGHS IN ASTROPHYSICS AND RELATIVITY

I started as a research student in October 1964. There were several potential PhD advisors; it was my good luck to be allocated to Dennis Sciama (**Figure 1**). I was still unsure what I wanted to do

Figure 1

Dennis Sciama. Provided by the Royal Society.

and whether I'd made a sensible choice. Had I been allocated to another advisor, I might well have dropped out within the first year, but Dennis created a buzz of enthusiasm that swept me along. I already knew of him through his splendid lecture course on relativity and his book, *The Unity of the Universe* (Sciama 1959). He had charisma, he inspired his research group with his infectious enthusiasm, he followed developments in theory and observation along a broad front, and he was a fine judge of where the scientific opportunities lay.

Rather than struggling to make an original advance in a stagnant area where older scientists got stuck, aspiring researchers should choose a vibrant and fast-moving area, offering new observations or new techniques: They will then be on a level with their seniors, whose expertise and experience is then at a heavy discount. And astrophysics was certainly on a roll in the 1960s. The first quasar, 3C 273, had been discovered in 1963 (see Hazard et al. 2018 for a chronicle of how this happened). A year later, cosmic microwave background (CMB) radiation—the afterglow of creation—was identified. And there were theoretical advances too, especially in general relativity, where the pioneering figure was Roger Penrose (**Figure 2**). He was Sciama's friend and near-contemporary, who started out as a mathematician, but was persuaded by Sciama to transfer his interests to relativity: Penrose introduced new techniques that enabled the consequences of Einstein's equations to be explored in situations without special symmetry, where there were therefore no exact solutions. Dennis Sciama immediately recognized the importance of this breakthrough and encouraged his students to attend a lecture series that Penrose was giving in London (where he was then a professor).

My closest contemporaries in Sciama's group were Brandon Carter, Bill Saslaw, and John Stewart. George Ellis had just completed his PhD in relativity and was starting a postdoctoral fellowship. And there was another student two years ahead of me in his studies; he was unsteady on his feet and spoke with great difficulty. This was Stephen Hawking (**Figure 3**). I learned that he had been diagnosed with a degenerative disease and might not live long enough even to finish his PhD degree. But, amazingly, he lived on for over 50 years more—until 2018. Even mere survival would have been a medical marvel, but, of course, he didn't merely survive. He became perhaps the world's most famous scientific celebrity—acclaimed for his brilliant research endeavors; for his best-selling book about space, time, and the cosmos; and, above all, for his astonishing triumph over adversity. Astronomers are used to large numbers. But few numbers could be as large as the odds I'd have given, back in 1964 when Stephen received his "death sentence," against ever celebrating his uniquely inspiring and long-sustained crescendo of achievement.

I therefore knew Stephen through all the 25 years of his marriage to Jane, as depicted in the movie, *A Theory of Everything*. His portrayal by Eddie Redmayne was extraordinarily true to life. And despite the conflation of several events, and despite the caricature treatment of all the science, it is essentially faithful to reality (though it omits mention of Stephen's students and postdocs such as Bernard Carr and Don Page, who supported Jane as quasi-carers). And what is even more remarkable is that the events depicted surrounding the film's end (around 1990) were only a halfway point in his astonishing career.

I didn't go to Penrose's London lectures but attended a Cambridge colloquium that was one of the first presentations of his ideas on trapped surfaces and singularities (Penrose 1965). I didn't understand much of what he was saying, but he is the kind of person who, even if you don't know what he's on about, gives the impression that a very special brain is at work. His thinking is not merely deeper than most of us can manage—it is of a very special geometrical nature: He can visualize four dimensions as easily as we can visualize three. His new concept stimulated Stephen Hawking and George Ellis to begin their investigations of black holes and the singularity at the beginning of our expanding Universe.

Figure 2

(*Foreground*) John Conway, (*back row*, *left to right*) Roger Penrose, Jim Peebles, and Martin Rees at a Princeton University conference marking the 50th anniversary of the discovery of the cosmic microwave background. Photo provided by Sarah Jane Nelson.

Penrose later achieved public celebrity, but it was more for his work on consciousness. His 1989 book, *The Emperor's New Mind*, became a best seller (Penrose 1989). The sales pitch, "great scientist says the mind is more than a mere machine," was alluring, but many who opened the book and found lots of equations must have got a nasty surprise. And even those (and that's most of us) who don't "buy" his grand thesis that quantum gravity is the key to consciousness can admire this book "a la carte" as a brilliant exposition of his intellectual enthusiasm: relativity, Turing machines, quantum theory, etc. His later "popular" books were even more technical. Even his recreations have been creative. Penrose tiling—whereby tiles of just two different shapes can cover the plane with a never-repeating pattern—offered insights into the geometry underlying pentagonal quasicrystals. And a 1958 paper on "Impossible Objects: A Special Type of Visual Illusion," cowritten with his father (Penrose & Penrose 1958), the eminent geneticist L.S. Penrose, inspired Escher's engravings of endless staircases and distorted landscapes. Roger has remained a fertile and original intellect into his 90s.

Figure 3

(*Foreground*, *left to right*) Martin Rees, Stephen Hawking, Don Page; (*back row*, *left to right*) Bernard Carr and Gary Gibbins. Photo provided by Anna Zytkow.

Back in 1948, Fred Hoyle (1948) and Thomas Gold and Herman Bondi (Bondi & Gold 1948)—then all in Cambridge—had proposed the steady state theory, according to which the Universe had existed in the same average state "from everlasting to everlasting." They conjectured that, as galaxies moved away from each other owing to cosmic expansion, new atoms were continually created, and new galaxies formed "in the gaps." This theory never acquired much traction in the United States (and still less in the Soviet Union), but its three advocates (all of whom I knew in their later years) were vocal and articulate: In the United Kingdom, the theory was widely publicized and discussed. And it was indeed a beautiful concept. Sciama himself espoused it, describing himself as its most fervent advocate apart from its three inventors.

The steady state theory was (rightly) touted as being a "good" theory because it was vulnerable to disproof. It predicted that everything was (statistically) the same, everywhere and at all times (i.e., at all redshifts): If things were, on average, different in the past, that would be evidence against it. But optical astronomers in the 1950s were unable to detect objects at sufficiently large redshifts (and look-back times) for such changes (even if they occurred) to be evident. However, the first clues came from radio astronomers: Some realized that some of the discrete sources detected in their surveys were "exploding galaxies" too far away to be detected optically. Although the redshifts of individual sources were unknown, it was possible to draw inferences from the relative numbers of apparently strong and apparently weak sources (because the latter would, statistically at least, be at greater distances).

The first credible evidence came from Martin Ryle's radio astronomy group (based in Cambridge's Cavendish Laboratory) and from the Australian group headed by Bernie Mills. Their surveys revealed too many distant sources relative to nearby ones to be compatible with a steady state. Ryle interpreted his results (correctly, as we now recognize) by postulating that we lived in an evolving Universe where galaxies in the past (when young) were more prone to indulge in the explosive behavior that rendered them strong radio emitters.

For me, coming fresh to the subject, the skepticism that greeted this evidence was perplexing. Ryle's claims—indeed everything he had claimed from 1958 onward—was compelling to me (and was vindicated by later developments). But I later realized that the skepticism of the "steady statesmen" was not just irrational obstinacy. Some of Ryle's previous data, in particular the earlier 2C survey, turned out to be unreliable, owing to "confusion" caused by inadequate angular resolution. Furthermore, he had initially vehemently opposed the suggestion that the so-called radio stars—discrete radio sources with no obvious optical counterpart—were actually distant galaxies. To add even more irony, it was actually Thomas Gold who first made that suggestion, which became the cornerstone of Ryle's later argument in favor of an evolving Universe. This "baggage" dating back to the early 1950s perhaps helps to explain why the steady statesmen held out against the evidence of the source counts.

There was also personal antipathy between Hoyle and Ryle—two outstanding scientists of very different styles. Hoyle enjoyed robust controversy; in contrast, Ryle shunned it. But, to be fair to Ryle, an experimenter who spends years designing and constructing an instrument (and Ryle was very much a hands-on instrument builder) understandably develops an exaggerated perception of its importance. In contrast, theorists can be relaxed about jettisoning theories: Those with fertile minds can quickly devise new ones.

Sciama took Ryle's data seriously, but when I joined his group, he was clinging to the steady state theory. He conjectured that many of the unidentified sources were nearby: The source brightness statistics could then (he argued) reflect nothing more fundamental than a local deficit in the Solar Neighborhood. But when the sources were revealed to have high redshifts, he abandoned this model (and never followed the route of saying that redshifts were noncosmological). The clinching evidence that led him to abandon the steady state was a very simple analysis we conducted (Sciama & Rees 1966) on the redshift distribution of quasars. By 1966, more than twenty radio sources in the 3C catalog had been identified with quasars with known redshifts (extending up to $z = 2.01$ for 3C9). When we split the quasars into redshift bins, each corresponding to a shell containing the same comoving volume as the others, the quasars were concentrated in the high-redshift bins. This evidence suggested that quasars were more common (or more luminous) in the past—just as Ryle had argued was the case from radio data alone. This was a crude version of the luminosity/volume or V/V_{m} test (Rowan-Robinson 1968, Schmidt 1968).

In a Big Bang model, the redshift distribution of quasars tells us little about the geometry of the Universe, but it tells us something about the astrophysical evolution of galaxies—indeed, I've continued throughout my career to study the implications of such data for galaxy and black hole formation, reionization of the intergalactic medium, and evolution of cosmic structure. The attraction of the steady state model was that every process of cosmic importance must be happening somewhere now and, therefore, must in principle be accessible to observations. The theory's advocates believed—as was reasonable in the 1950s—that in a Big Bang model crucial processes would be inaccessible. But it later turned out that we can indeed observe "fossils" of the formative early eras of cosmic history—the CMB itself (and its angular fluctuations), and cosmic helium and deuterium. So Sciama's disappointment was short-lived, and he became quickly reconciled to the Big Bang—indeed, he espoused it with the enthusiasm of the newly converted.

There was, at that time, a substantial research effort (spearheaded by George Ellis and a series of collaborators) aimed at investigating the various classes of homogeneous but anisotropic cosmological models. This was an interesting exercise in its own right. However, a special motivation came from Charlie Misner, who spent the academic year 1966–67 on sabbatical in Cambridge. It was from Misner that we learned about the so-called horizon problem—that causal contact had been worse in the early phases of a Friedmann (decelerating) Universe, rendering it a mystery that the present Universe seemed so uniform and synchronized. Misner noted that causal contact would have been better if the early expansion had been anisotropic and best of all in the "mixmaster" model, in which there was an alternation in the axes of fast and slow expansion. The aim of the "Misner program" was to show that a Universe could have started off (and homogenized) via a mixmaster phase but that the initial anisotropies would later be erased, either dynamically or via neutrino viscosity (Misner 1968). This program failed—and until the invention of the "inflationary" Universe, more than a decade later, most of us probably thought that an explanation of global homogeneity would have to await a quantum-level understanding of the singularity.

It was coincidental that the theoretical advances in relativity, instigated by the new global methods that Penrose pioneered, happened concurrently with the discovery of the CMB. It was a further coincidence that, during the 1960s, objects were discovered where general relativity was crucial rather than a trivial refinement of Newtonian gravity—discoveries that stimulated the new research area of relativistic astrophysics.

Black holes, of course, are the most remarkable prediction of Einstein's theory. The Schwarzschild solution, discovered in 1916, represents the simplest black hole. But a more general solution, discovered in 1963 by the New Zealander Roy Kerr, was believed to describe a spinning black hole. A crucial breakthrough came from the work of Penrose, Hawking, Carter, David Robinson, and others. They showed that Kerr's solution was actually generic, in the sense that any (electrically neutral) black hole would end up being described by this particular solution of Einstein's equations. That was a very surprising result, for which John Wheeler later coined the phrase, "black holes have no hair." Something that was exactly spherical would collapse to Schwarzchild's solution—that seems natural. But it was thought that the generic case would be intractable. The "no hair" theoretical results revealed, contrariwise, that any gravitational collapse leads, after the emission of gravitational waves, to a black hole described exactly by two numbers: its mass and its spin.

The relativists who pushed general relativity forward in the 1960s and 1970s (in retrospect, a "golden age" for that subject) were mainly associated with one of three "schools": those centered in Princeton, in Cambridge, and in Moscow. Communications were far less immediate than today (especially, of course, between East and West in the Cold War era). However, the interactions that occurred were almost invariably cooperative and friendly, to an extent that isn't always the case in fast-moving fields.

The emergence of relativistic astrophysics was driven by the first high-redshift quasars, the discovery of neutron stars, and the first results from space astronomy (especially X-ray astronomy). Cosmic objects plainly existed where explanations from general relativity could be crucial, rather than just needing a tiny correction to Newtonian dynamics. Although I followed the exciting advances in mathematical relativity at close range, my own research was guided toward its observational consequences, involving more physics but less mathematics.

My early focus was on the physics of quasars—how an object small enough to be variable could pack a punch equivalent to a hundred entire galaxies. A byproduct of this was a paper (Rees 1966) pointing out that objects expanding at close to the speed of light could appear to move superluminally—with apparent transverse speeds much faster than light. This was a very elementary point, but it attracted renewed interest from the 1970s onward, when radio interferometers with milliarcsecond resolution actually detected such superluminal motions in jets emerging from the centers of galaxies.

The social and interactive aspects of research appealed to me—and gradually broadened my still-limited horizons. My first foreign trip was to a 1965 conference in Dublin [memorable for the presence of the ancient President Éamon de Valera, a mathematician in his younger days, led (blind) into the front row]. It was followed, while still a student, by other European meetings, and two trips to the United States. One of the latter meetings was to a relativistic astrophysics conference in Miami. It was held in the Fontainebleau Hotel, which featured in the James Bond movie *Goldfinger*, then recently released. This accorded well with my preconceptions of what the United States was like—and it took several later visits to realize that this grotesquely lavish location was atypical even of America.

But the most influential conference I attended (at a time when I'd just completed my PhD) was the 1967 International Astronomical Union General Assembly. This was held in Prague—just a year before the Prague Spring. It offered many of us in the West our first chance to meet many scientists from the Soviet Union. Some of these people were allowed to travel to the West—and indeed one of the greatest of them, V.L. Ginzburg, spent several months in Cambridge in the 1960s. But things were less flexible for most of the outstanding Russian theorists. Furthermore, these scientists had to struggle even for permission to send their papers to the West; they received Western journals only after a delay and after censorship of "sensitive" content. They were, however, allowed to travel to Prague. So this was an opportunity to meet the great and hyperenergetic Y.B. Zel'dovich and his younger colleagues. Among the latter was Rashid Sunyaev. Sunyaev and I were more or less the same age and had each read some of the other's papers. When we met, we each expected, I think, to encounter someone older.

POSTDOCTORAL YEARS (1967–72)

I had done enough research by mid-1967 (mainly on the physics of quasars) to complete a thesis. I typed it myself on a portable typewriter, but was spared the need for carbon paper or, even worse, the inky stencils of the Gestetner duplicating machine, as xeroxes had by then become available.

It was necessary to think about an "afterlife." I was happy in Cambridge (more so than I had been as an undergraduate) and also enthused by my research. So, I followed the path of least resistance. The individual colleges in Cambridge offered junior research fellowships—attractive posts that allow greater freedom than a typical postdoc position tied to a specific principal investigator or grant. I applied to eight colleges and was relieved when one of them, Jesus College, accepted me. But I felt then that such a fellowship should be a springboard and not a cushion (and have ventured that advice to many younger colleagues over the years). In particular, a big plus was the flexibility to intermit, and spend periods of a few months abroad, without having to worry about having a job to return to.

Dennis Sciama's recommendation gained me an invitation to spend a few months at the California Institute of Technology (Caltech) in 1968. This proved a productive period during which I met many scholars—some of them also postdocs, others more senior—who became friends and collaborators for decades to come. Kip Thorne and Peter Goldreich, both young and precocious professors, offered mentorship, and it was a privilege to interact with Maarten Schmidt and other senior observers. I especially appreciated the hospitality of Wallace and Anneila Sargent—both from the United Kingdom but who were then recently settled at Caltech, where they would spend their entire distinguished careers.

In February 1968, while at Caltech, I received a handwritten airmail letter from Dennis Sciama telling of a remarkable seminar that he'd just attended in Cambridge. It was given by Anthony Hewish, who announced the discovery of mysterious radio sources pulsing on and off with a very steady period of a second or less. These were pulsars—recognized a year later to be spinning neutron stars but still a mystery at the time of the announcement. (Of course, news of any such breakthrough would today spread instantaneously via social media.) The Caltech astronomers held a weekly "journal club," where interesting new papers could be discussed. I was asked to speak about what Sciama said in the letter. I don't recall that anyone offered any immediate new insights—this was a disappointment, as the great Richard Feynman (who I'd not previously encountered) turned up to listen.

My work in the late 1960s was mainly on the physics of extreme objects—those involving black holes or neutron stars or which emitted powerfully in the radio band. I followed developments in cosmology and can lay claim to two minor contributions directly related to the CMB. One concerned what is now sometimes called the Rees–Sciama effect (Rees & Sciama 1968)—the perturbation in the CMB due to a transparent gravitational potential well (e.g., a supercluster of galaxies) along the line of sight. This was actually a special nonlinear extension of the important general paper by Sachs & Wolfe (1967). Had Sciama and I known then the actual amplitude and scale of clustering, we would not have felt it worthwhile to explore these higher-order effects. But at that time, there was no way of ruling out large-amplitude density fluctuations on gigaparsec scales (indeed there were early—and, in retrospect, misleading—indications of such clustering from the distribution of quasars over the sky).

My second CMB contribution (Rees 1968) addressed its possible polarization. The simplest illustrative examples of this effect arose in anisotropic but homogeneous models (though the effect was obviously present in more general models). This realization stimulated a search for quadrupole polarization by Pete Nanos (then a Princeton student who published his lower limits later; Nanos 1979), who subsequently achieved greater fame as the Director of Los Alamos at a time of turmoil in the lab. That was more than 35 years before CMB polarization was actually detected, and the polarization pattern is now recognized as a crucial diagnostic of the nature of the fluctuations (scalar versus tensor) and the reionization era of the intergalactic medium.

In the mid-1960s, there had been an organizational upheaval in Cambridge astronomy that, though perceived as disruptive at the time, had important long-term benefits for the subject in the university and would have a big impact on my own later opportunities. The central figure here was Fred Hoyle. I've already mentioned his standoff with Ryle over the steady state theory. But he was also involved in another spirited vendetta—one where the issues were parochial rather than cosmic.

Ryle's group was part of the Cavendish Laboratory—a consequence of the fact that radio astronomers were drawn from physics and engineering. But theoretical astronomers were in the faculty of mathematics. This also was a legacy of history: The mathematical curriculum had always included subjects that would in most universities be classified as physics; the most distinguished chair in mathematics, the Lucasian, had been held by Isaac Newton and a series of distinguished physicists. (Paul Dirac was the holder in the 1960s. I attended his lectures on quantum theory: He read, and copied onto the blackboard, from his classic monograph published 30 years earlier. He'd tried to express himself optimally then and saw no reason to change his exposition.)

The dominant applied mathematics figure at that time was George Batchelor, an expert in fluid mechanics. Batchelor was an efficient administrator and effective operator. But in style and personality he was orthogonal to Hoyle. Rather than remain in a department headed by Batchelor, Hoyle obtained funds to set up a separate Institute of Theoretical Astronomy (IOTA) (**Figure 4**). He took a close interest in the design of his new building, which was modeled on the geophysics institute in La Jolla, California, where his long-time collaborators Geoffrey and Margaret Burbidge worked. (Indeed, I was interested to unearth much later a file of Hoyle's business-like correspondence with the architect and quantity surveyor. Hoyle argued, for instance, that it would be economical for all the floors to be carpeted, because otherwise there would be a need for costly soundproofing on the ceilings—and they were.)

Figure 4

The original 1967 Hoyle building for the Institute of Theoretical Astronomy at the University of Cambridge, which has undergone subsequent enlargement.

IOTA had funds to appoint 12 research fellows, and I was lucky to be one of the first tranche. (Others included Joe Silk, Stephen Hawking, Brandon Carter, Gary Steigman, and Michael Werner.) Appointment procedures were far more informal in those days—I recall getting my offer after a chat with Hoyle and Geoffrey Burbidge (who was on his regular summer visit) in the not-quite-finished IOTA building and, walking back into town in especially high spirits, realizing that this was a genuine lucky break. This post could be held in conjunction with a college fellowship. I therefore continued my affiliation with Jesus College for two years, before moving to King's College.

Hoyle himself was a habitual absentee from IOTA and was relaxed about other staff members taking periods of leave. So, a year after my stay at Caltech, I arranged a similar four-month period at the Institute for Advanced Study (IAS) in Princeton. This institution is (contrary to a common misperception) formally quite separate from Princeton University. However, there is clearly a symbiosis—indeed, I doubt that the IAS would be a magnet for astrophysicists were it not for the proximity of the much larger group at the university. My visit was the precursor to a regular association with both these institutions.

Among the distinguished professors at the IAS, Freeman Dyson was especially engaging. Although most famous as a mathematical physicist, his eclectic interests spanned astronomy and space exploration. Knowing that I was a solitary bachelor, and that the cafeteria was closed on weekends, he often invited me to Sunday morning breakfast with his wife Imme and the four youngest of his six children (then aged 3–16). It was Freeman, more than anyone, who convinced me that the IAS was a place I wanted to maintain contact with (I later became a Trustee)—and on every visit, the chance to chat with him was a bonus.

He exemplified the distinctive value of the IAS—no other institution could have enabled someone with his intellectual range to flourish and to stimulate so many for so long. His achievements in quantum electrodynamics secured his reputation while he was in his 20s. But he spent the remaining 65 years indulging his curiosity and deploying his talent across many fields. During my visits, we discussed, for instance, the far future of our Universe. Freeman didn't like an early paper I'd written (Rees 1969) on a closed Universe that would recollapse to a big crunch: He said the concept "gave him claustrophobia." He later wrote a classic (and very long) article on the future of an ever-expanding Universe (Dyson 1979). One of his talents was for clear writing and exposition. He thought that young scientists should write papers and old ones should write books. He was already in his 50s when he published his first book (Dyson 1981), *Disturbing the Universe*; his books and general writings (especially those in the *New York Review of Books*) are an important part of his legacy.

I resonated with his attitude that the diversity of the living world should stimulate our sense of wonder—as much as the austere beauty of math and physics—and with his nondogmatic engagement with religion. (In the latter context, when, in 2011, I received the Templeton Prize, he told me that this made him feel better about having won it himself, as I'd "done just as little to deserve it as he had!" Of course, I'd actually done far less.)

It was a privilege to speak at the conference held in 2013 to celebrate his 90th birthday—a meeting attended by several hundred people, along with his 6 children and 16 grandchildren. He survived to be 96, engaging in email correspondence with friends around the world and hosting guests in the IAS's cafeteria—indeed, he died a few days after falling there while carrying his lunch-tray. His was a unique intellect, and he was a great and kind man.

I owed my initial invitation to the IAS to John Bahcall—a nuclear physicist who had shifted into astrophysics. He was a junior faculty member at Caltech, but in 1969 he was offered one of the grand permanent professorships at the IAS. I was one of his first IAS postdocs. Over the next 35 years (until his sadly premature death in 2005), he was the pivotal mentor who rendered the IAS an exceptional training ground for young astrophysicists. He was also a major influence in scientific politics and probably deserves as much credit as any other individual for the fact that the *Hubble Space Telescope* (HST) got built, survived initial setbacks, and sent back a stream of fascinating data and images for 30 years. But he will be remembered also as a pioneer of neutrino astrophysics. Ray Davies's 20-year experiment to study solar neutrinos by detecting their interactions—only about one a day, with chlorine atoms in a 35-ton tank of cleaning fluid deployed deep underground (in the Homestake Mine in Ohio)—was an astonishing achievement. Throughout this period, it was John Bahcall who provided the theoretical underpinning and interpretations. It was unfair that John didn't eventually share the Nobel Prize awarded to Davies (and doubly sad that this accolade didn't happen until Davies had such advanced dementia that he couldn't properly appreciate it).

John was a systematic worker. He would say, I'm going to do this problem; it will take about a month. And that's what he would do. Each morning he would write the date at the top of a sheet of paper and start calculating. In this respect, he modeled himself on the great Hans Bethe who would famously work steadily all day, covering sheet after sheet of paper with equations. Bethe had a long-continuing collaboration with Gerry Brown on supernova theory. This was before the Internet era, and he numbered the pages consecutively, ending up with more than 2,000 pages and 5,000 equations. I knew I could never be as systematic as that, and fortunately I found that there were other role models who worked in a more open-ended and disorganized fashion.

A highlight of early Princeton visits was the chance to meet John Wheeler—surely one of the visionary figures in physics—with a record dating back to classic work on the physics of nuclei with Niels Bohr. Wheeler was atypical of physicists in his attitudes and style: conservative in his politics (and one of the few who retained friendship with Edward Teller after the Oppenheimer security hearing controversy); always dressed in a suit and tie; and always formal and courteous. These characteristics were in seeming disjunction to his speculative research on what he called the "flaming ramparts" of the cosmos. Back in the 1950s, he wrote a book, *Geometrodynamics* (Wheeler 1962), exploring the concept of space–time foam, the idea that on a tiny scale space and time had a complex entanglement. In the 1960s, his focus was on the physics of ultradense matter and the nature of gravitational collapse. Wheeler was a workaholic, highly organized, and dedicated. One of his habits, copied by his students at Princeton, was to keep large leather-bound notebooks in which he would neatly write whatever he learned or was told. One always felt the need to think before speaking because everything was being inscribed in one of these notebooks. His lectures were memorable. When he came to give us a special lecture in Cambridge, he spent two hours beforehand covering a wall-to-wall blackboard with equations, diagrams, and cartoons, using multicolored chalk. During the lecture, he transited slowly across the room, annotating and explaining what he'd prepared—no modern PowerPoint lecture could be so pedagogically effective.

I also had contact with Subramanian Chandrasekhar, another venerated figure in the subject—and, like Wheeler, a renowned workaholic. He had the ability to do very detailed calculations and algebra, which would have intimidated younger people, without making mistakes. Chandra (as he was universally called) worked, over his long career, on a succession of different subjects—for each phase, he wrote dozens of papers and codified them in a monograph before moving on to the next subject. He took up general relativity when already in his 50s, aiming to use his distinctive skills to do things differently from what the younger people were doing at that time. I recall my first contact with him at a summer school held at Varenna on Lake Como. I turned up late, when most participants were away on an excursion. But not all were gone. Standing silently in the middle of a formal garden, impeccably dressed in a dark suit and white shirt, reading a novel by Thomas Hardy, was an austere-looking Indian. This was Chandra. He had been a student in Cambridge in the 1930s, where he had famously clashed with Arthur Eddington. But on the occasion of Eddington's centenary in 1982, Chandra returned to give a gracious memorial lecture. Some of us made vain efforts to persuade him to spend his retirement in Cambridge, but he stayed loyal to the University of Chicago, where he had been since 1936. He said that the only reason he might consider a move was through concern at leaving his wife, Lalitha, in the harsh environment of Chicago. He never really retired. My last meeting with him was when he came to Oxford to check proofs of his final book (Chandrasekhar 1995), an analysis of Newton's *Principia* from a modern perspective. I spoke at a meeting in Chicago in 2010 to mark his own centenary—Lalitha, then aged 100, was still alive and living there.

My postdoc-era visits to Caltech and Princeton were followed by a shorter but equally stimulating stay at Harvard University, and by brief excursions to meetings, summer schools, and suchlike, which meant that within a few years I had met many leaders in my subject, as well as many of my contemporaries, some of whom became collaborators. Although I visited centers in mainland Europe, I generally encountered my European contemporaries in the United States, because we nearly all spent some postdoctoral time there. In this regard, the situation later became gratifyingly different: There is a far stronger and more integrated research community across Europe, and it's more common for young researchers to move between different countries on our side of the Atlantic. (We Brits, however, now have to campaign hard to ensure that the fall-out from Brexit doesn't jeopardize such links for our next generation of young researchers.)

Though based at IOTA for five years, I had little scientific engagement with Fred Hoyle: He was already moving out of the mainstream (he never reconciled himself to the Big Bang—though he developed a kind of "steady bang" compromise in his later years). Although Hawking and Carter were based at IOTA, doing what in retrospect was clearly exceptional work on black holes, Hoyle took minimal interest in them. And he delegated responsibility for graduate students. I consequently found myself, by default, supervising several—which was a real plus for me, as these included Roger Blandford and Jim Pringle, from whom I learned a great deal both then and during their later careers.

But my personal relations with Hoyle were genial. At a 1970 "study week" on galactic nuclei at the Pontifical Academy of Sciences—attended by about 20 mainly senior figures (I was much the youngest participant)—he gave a talk attacking the evidence for the Big Bang. I spoke straight afterward, offering an opposite view. Not all senior professors would have been relaxed about being contradicted in front of scientific grandees by a whippersnapper on their staff. But Fred was generous-minded, and these differences didn't weaken his support for me.

MY FIRST "REAL JOB," THEN BACK TO CAMBRIDGE

After five years in research posts, I thought I should seek a more established position in academia, even though this would entail leaving a junior perch in Cambridge offering enviable freedom. I had by then a substantial publication record and experience as a lecturer at conferences and summer schools. I applied for a mathematics chair in London that I didn't get: This was a lucky escape as it would have involved duties poorly matched to my style or experience and foreclosed future opportunities. My good fortune was to be appointed instead at the University of Sussex. This university, founded in 1961, had established a special Astronomy Centre because of its closeness to the Royal Greenwich Observatory (RGO) at Herstmonceux Castle. Roger Tayler (who had taught me at Cambridge) moved to Sussex as director of the center.

I rented a bed-sit in Brighton for a pound a day (the landlady regarded me as an unusually well-behaved student, and I didn't disillusion her); I enjoyed the daily commute to the spacious and well-designed campus in Falmer (4 miles away). At the time, the university had the huge benefit (compared to a tradition-bound institution like Cambridge) of having nobody who had been there for more than 10 years. Two faculty members then in their 30s (Harry Kroto and Tony Leggett) went on to win Nobel Prizes. I was able in my first year to attract excellent postdocs (e.g., Vincent Icke) and visitors (e.g., E.R. Harrison) and to benefit from contact with researchers at RGO. And there was a lively group of students doing PhDs and taking an MSc course. But though I would have been content to settle in Sussex, my first year turned out, unexpectedly, to also be my last. This was a consequence of further ructions in Cambridge.

When Fred Hoyle set up IOTA, he had secured funding for 5 years—it would expire in 1972. But he'd given little forethought to the longer-term future. Indeed, he became disengaged from what went on at the institute, working mainly at home with ex-students such as Jayant Narlikar and Chandra Wickramasinghe and with his long-standing collaborators such as Willy Fowler and the Burbidges, who visited every summer. Others in the university, however, did address IOTA's future. They proposed a merger with the adjacent old Observatories (which had a funding stream and several staff positions) to set up a combined Institute of Astronomy (IOA) that was intended to embrace optical astronomy and instrumentation, as well as theory. The Observatory Director, R.O. Redman, happened to be retiring that year, and it was proposed to replace him with someone who could direct the new IOA. The appointee was Donald Lynden-Bell, a versatile theorist, previously at Cambridge and Caltech, who then held a senior post at the RGO. I already knew his classic work on stellar dynamics and on black holes, and we'd collaborated on a study of the Galactic Center (Lynden Bell & Rees 1971).

Hoyle felt aggrieved that maneuvers crucial to IOTA's future took place behind his back. It is true that he had little goodwill from some senior Cambridge physicists and mathematicians, but they could fairly say that his back was almost constantly turned (through his travels and disengagement). His grievance led him to resign as Plumian Professor of Astronomy and Experimental Philosophy (the University's oldest astronomy chair, dating from 1704). Hoyle actually forewarned me of his decision—and I was gratified when he added that he'd be happy if I were his replacement. I was only 30 and diffident about applying, but I did so. To my surprise, I was appointed and, in consequence, returned to Cambridge in 1973. One long-term bonus was becoming a colleague of Donald Lynden-Bell, who retained his enthusiastic engagement with IOA's staff and students right until his death in 2018.

IOA's staff was strengthened by the arrival of Andrew Fabian, Gerry Gilmore, George Efstathiou, and others. We attracted excellent students as successors to the Blandford–Pringle generation. Theorists who graduated in the 1970s and 1980s included many who have achieved distinction (and remained friends and collaborators)—among them Mitch Begelman, Cathy Clarke, Carlos Frenk, Martin Haenhelt, Craig Hogan, Nick Kaiser, Ofer Lahav, Sterl Phinney, and Simon White—and these were followed by another generation in the 1990s, including (among those I advised) Douglas Scott, Priya Natarajan, and Enrico Ramirez-Ruiz (**Figure 5**).

Another bonus of my return to Cambridge was election as a professorial fellow at King's College. Involvement in a college gives an extra dimension to academic life: It offers even the youngest academics informal day-to-day contact with those from other disciplines. This is a benefit limited in most universities to only the most senior—department chairs and suchlike. The King's Fellows

Figure 5

A group of former students and collaborators—taken at home during my 75th birthday conference. (*Sitting, left to right*) Martin Rees, Carlos Frenk, Pria Natarajan; (*standing, left to right*) Craig Hogan, Roman Znajek, Roger Blandford, Mitch Begelman. Simon White, Enrico Ramirez-Ruiz, and Martin Haehnelt. Photo provided by Anny Zytkow.

included molecular biologist Sydney Brenner, geneticist Anne MacLaren, and plate-tectonics pioneer Dan McKenzie, along with eminent humanists including Geoffrey Lloyd, Bernard Williams, and Frank Kermode. It was a privilege to serve alongside such people on college committees. Another bonus was the beauty of the surroundings, I felt this especially whenever I walked through the college at night, hearing someone practicing the organ in King's College's famous Chapel, all lit up like some vast musical instrument.

For my first ten years as Plumian Professor, I lived in King's College in rooms that had once been occupied by the great economist Maynard Keynes. The main room was decorated with a mural, painted by Duncan Grant and Vanessa Bell, depicting (on alternate panels) clothed women and nude men—a work of minimal artistic merit that was preserved as an interesting relic of the Bloomsbury Group. The room was an excellent place for entertaining students and colleagues. Some who were at IOA during that time may recall drink parties there. In 1986, when I got married, my wife Caroline (a social anthropology professor) and I moved to a house in a neighboring village; but we both retained offices in King's.

When IOA was fully established, Donald Lynden-Bell became the first Director. But we agreed that, if others were content with the plan, we would like to alternate, doing five-year stints—and this is more or less what happened for almost 20 years, key long-term issues being discussed jointly. Although IOA was technically a department of the university, it was small and had below-average teaching obligations. But it had an above-average proportion of research students, postdocs, and academic visitors.

I spent a lot of time as a facilitator—attracting and engaging with visitors, arranging conferences, and so forth. Donald Lynden-Bell and I tried to foster a year-round informal and interactive environment—where everyone could gather for morning coffee, as well as for refreshments before and after colloquia, and where new arrivals could quickly be introduced to others. We attracted a flow of regular visitors including many from Eastern Europe, who during the Cold War were often isolated. I'd mention in particular Jaan Einasto and his Estonian colleagues; Igor Novikov and others from Moscow; Jiří Bičák and Prague colleagues; and several from Poland. The annual summer conferences had been a distinctive feature of IOTA. Especially memorable and well-attended was the conference held in 1971 to celebrate Willy Fowler's 60th birthday. I tried to continue this tradition. I recall, in particular, the conference in 1981 on supernovae, during which Hans Bethe celebrated his 75th birthday. Another especially significant meeting was the three-week Nuffield workshop in 1982, during which the then new ideas of cosmic inflation (and, in particular, how fluctuations might be generated) were thrashed out.

I've come to realize that this atmosphere owes a lot to the design of our buildings. Hoyle's original IOTA was on one floor, with a wide corridor (along which the Burbidges, Fowler, and Hoyle could walk side by side!) and spacious open areas; all offices had French windows opening onto a lawn. It was also a plus that many staff members chose to work with their office door open. This architectural concept has, in essence, survived despite subsequent enlargements. It would be harder to replicate an informal interactive atmosphere in an urban tower block where people were spread over several floors, especially if there were no cafeteria.

My own research progressed steadily on a broad front—though this was substantially because of the colleagues and students with whom I could collaborate. Thanks to the excellent support staff, formal administration wasn't too time consuming—this is certainly a huge contrast to the present, when IOA is (gratifyingly) much larger, but the overall bureaucratic burden weighs more heavily on everyone.

I've myself preferred to work on several topics in parallel. But for clarity I'll describe them in two categories: high-energy astrophysics and cosmology.

SCIENCE: HIGH-ENERGY PHENOMENA

The 1960s saw the advent of space science—including X-ray astronomy. This started with sounding rockets, each yielding just a few minutes of data before crashing down again: The *Uhuru* satellite was launched in 1971 and revealed compact binary systems where an ordinary-seeming star was orbited by a companion emitting intense and rapidly variable X-rays. The companions were quickly interpreted as neutron stars or black holes, capturing gas from their more normal partner. These discoveries created great interest in modeling the structure of accretion discs—and the more complicated pattern of accretion onto highly magnetized neutron stars. I explored this topic partly with Jim Pringle (Pringle & Rees 1972) and other colleagues over the next decades—especially Andrew Fabian, who came as a postdoc and remained at IOA for the rest of his career and became an international leader in X-ray astronomy. He has been well plugged in to the data from the succession of increasingly powerful X-ray telescopes—especially those from the ESA (European Space Agency), NASA, and Japan.

Later, X-ray telescopes offered sufficient spectral resolution to reveal broad emission lines in the stronger sources. Fabian and others attributed the broadening and asymmetry of the observed lines to Doppler and gravitational redshifts. These lines not only provided clear evidence that the central object was indeed a black hole but in some cases set a lower limit to the hole's spin (e.g., Fabian et al. 1989). The study of disk polarization, about which I wrote an early paper (Rees 1975), is only now opening up, with the launch in 2021 of the *Imaging X-ray Polarimetry Explorer* spacecraft.

The physics of X-ray binaries, containing a black hole of about 10 $M_\odot$, is in many respects a miniature and speeded-up version of what happens in the engine that powers quasars and radio sources—where the hole has a mass of millions, or even billions, of suns. But whereas it was quickly accepted that X-ray binaries like Cygnus X1 involved black holes, it took longer—until the late 1970s—before a well-justified consensus emerged that active galactic nuclei (AGN) were powered by inflow onto black holes [and perhaps by the more exotic Blandford & Znajek (1977) process, whereby energy is extracted from a black hole's spin rather than from accretion].

Reaching that consensus was a muddled and convoluted process—in contrast, for instance, to the speed with which, in the 1960s, pulsars were accepted to be spinning neutron stars and the CMB was attributed to a hot big bang. This is partly because quasars were in a sense discovered too early. Had there already been a better understanding of less extreme phenomena in the centers of galaxies—radio sources and Seyfert Galaxies—it would have been natural to have interpreted quasars as similar objects but with the wick turned up so that they outshone their host galaxy. And the undue persistence of Hoyle, Burbidge, and others in advocating noncosmological redshifts muddied the waters still more.

Furthermore, compelling arguments had been given by Donald Lynden-Bell (1969). I took his ideas seriously and indeed coauthored a paper arguing that the observed activity at our Galactic Center was energized by a black hole of a few million solar masses (Lynden-Bell & Rees 1971; see also Rees 1982). But I continued to hedge my bets, to the extent that I thought it was worth considering alternatives such as runaway evolution in a compact massive star cluster (e.g., Begelman & Rees 1978).

Indeed, I was agnostic on the nature of the central engine when I wrote my first serious paper attempting to interpret radio galaxies (Rees 1971). In that paper, I conjectured that the giant lobes weren't ejected via a single mega-explosion but were gradually inflated by relativistic jets. A more general version of this model was later described by Blandford & Rees (1974). But the assumption that the engine was a supermassive hole enabled Blandford to carry the idea further and to develop specific ideas on energy production and jet formation that have been the basis for almost

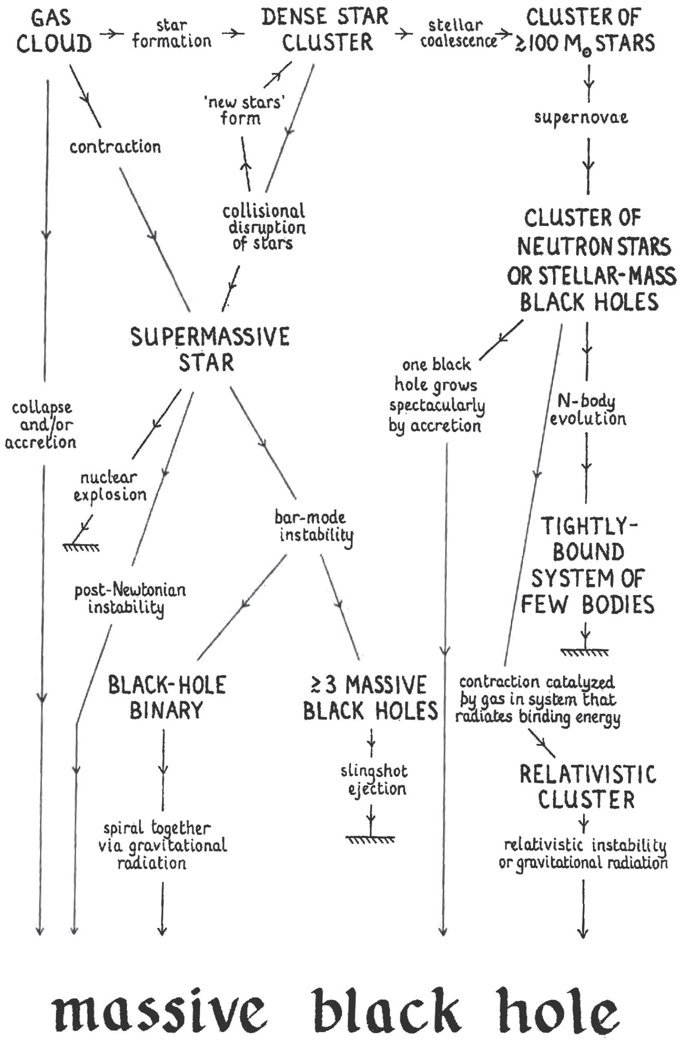

Figure 6

Flow diagram of routes to the formation of a supermassive black hole. This first appeared in my Halley Lecture. Figure reproduced from Rees (1978).

all subsequent interpretations (reviewed in Begelman et al. 1984)—and this has continued right up to the present (now of course aided by powerful computer simulations).

For my Halley Lecture in Oxford (Rees 1978), I prepared a flow diagram (**Figure 6**) that displayed the various routes whereby a massive black hole could form. The precursor stages could generate powerful emission, but the main message—the bottom line—was that a supermassive hole would be the inevitable outcome and the likely origin of the bulk of the energy emitted.

One interesting and predictable diagnostic of supermassive holes would be the tidal disruption of stars that pass close to them (Rees 1988). Tidal disruption events (TDEs) have attracted increasing interest because they seem to account for some characteristic flaring in AGNs, and because they create a fascinating variety of phenomena dependent on the type of star, its orbit, etc. (Indeed, TDEs were the theme of an entire conference held on my 75th birthday nearly 30 years later.)

Figure 7

Two high-energy astrophysicists, Rashid Sunyaev (*left*) and Peter Mészáros (*right*). Photo provided by Anna Zytkow.

From the 1990s onward, I became interested in a new phenomenon: gamma ray bursts. These were first reported by Klebesadel et al. (1973), but it wasn't until the 1990s that they were generally accepted as hyperpowerful events from cosmological distances. All-sky surveys showed they had an isotropic distribution around us (Fishman & Meegan 1995), and some were identified with optical afterglows that allowed them to be associated with distant galaxies (van Paradijs 1999).

The consensus now is that there are two classes, both of which are extragalactic. Some are a rare type of supernova in which energy squirts out along a jet rather than diffusing through the stellar envelope. Others (the shorter ones) are caused by the coalescence of a neutron star binary. Both scenarios are likely to create a black hole remnant.

I have had the benefit of extensive collaboration with the versatile theorist Peter Mészáros (**Figure 7**): Our papers include Mészáros & Rees (1992, 1997, 2015) and Rees & Mészáros (1992, 1994). We developed a model whereby jets with high Lorentz factors are unsteadily generated for typically a few seconds from a newly formed black hole; as they move out, they experience internal shocks and sweep up an external medium that generates a slowly declining afterglow. This has become an extensively studied topic by several groups (see, for instance, Piran 1999).

It was especially exciting when LIGO (Laser Interferometer Gravitational-Wave Observatory) and VIRGO, in 2017, detected an event, GB 170817, whose gravitational radiation was consistent with it being a merging neutron star binary. It emitted electromagnetic radiation in all wavebands—though weaker than expected for directly detected bursts at that distance, probably because our line of sight is misaligned with the beam and we only see sidelobes. This discovery was a paradigm of multimessenger astronomy, requiring global collaboration involving instruments of many kinds.

SCIENCE: GALAXY FORMATION AND COSMOLOGY

The other ongoing theme of my research has been galaxy formation and cosmology. The CMB maps reveal that the early Universe was homogeneous, except for small-amplitude fluctuations, but the associated density contrasts would amplify during the expansion until bound systems condensed out. Ideas of how galaxies and large-scale structure might have formed—top-down fragmentation or bottom-up hierarchical build-up?—were gradually settled in favor of the latter option. Independent of this key question, an important mystery was what singled out these objects called galaxies as the most conspicuous and distinctive large-scale entities in the cosmos. Why is there some kind of upper limit that prevents a cluster of galaxies from instead being a single supergalaxy? With Jerry Ostriker (Rees & Ostriker 1977), I advanced a physical reason for this—as did Silk (1977) and Binney (1977) at around the same time. Clouds that were small and dense would cool so rapidly that they could never be supported by gas pressure; they would go into free fall and fragment into stars. But above a critical mass, the cooling is less efficient, and gas could remain supported against gravity by pressure. This critical mass might relate to the mass of the biggest and brightest galaxies.

These papers were written in 1977. But by that time there was a consensus that a key ingredient must be added to our theoretical models of galaxies: dark matter. A whole raft of observations accumulated during the 1970s that indicated that the internal motions in galaxies would cause them to fly apart unless they were restrained by a gravitational force several times stronger than the stars and gas in the galaxies could supply. Galaxies must possess a dark halo, probably consisting of a swarm of particles with no electric charge. In a paper the following year with Simon White (who was then just completing his PhD and deserves 90% of the credit for what's currently my most highly cited paper), we generalized the 1977 work to the case when the gas was embedded in a halo of dark matter, whose nature we didn't specify (White & Rees 1978).

A transformative development of the 1980s was the delineation of the cold dark matter (CDM) theory for the emergence of galaxies during cosmic expansion. The dark matter, assumed to be a relic of the early Universe, is inferred to contribute five times more gravitating material than ordinary atoms. Not only did its inclusion allow a better fit to the data on galaxies and clusters but it also made a crucial difference to the formation process. Of special importance was a conference at Santa Barbara in 1984, of which I was a co-organizer, during which several of the key ideas jelled (reviewed in Blumenthal et al. 1984).

Of interest then was another issue: the end of the cosmic dark age. The Universe became literally dark after about half a million years, when the primordial radiation shifted into the infrared. It remained dark until the first stars (probably grouped in structures of subgalactic scale) formed and heated it up again. How and when this happened, and the nature of the so-called Population III stars, formed the theme of some of my papers. I also showed (Hogan & Rees 1979, Scott & Rees 1990, Madau et al. 1997) how one might use 21-cm line tomography to probe the dark age—a topic of burgeoning interest now that the massive Square Kilometer Array project, well suited to this task, is likely to become a reality. This instrument will also probe the era of reionization by early stars and quasars (Haehnelt et al 1998, Miralda-Escudé & Rees 1994, Miralda-Escudé et al. 2000).

Later developments in modeling galaxies and structure formation have depended on computer modeling—for which I've been always an enthusiast but never a practitioner. However, I'm glad to have had George Efstathiou, a pioneer of these techniques, as a long-time Cambridge colleague (now joined by Debora Sijacki) and to have remained in close contact with Carlos Frenk and Simon White, who built up world-class groups at Durham University and the Max Planck Institute for Astrophysics in Munich, respectively.

The great triumph of elaborate simulations by these (and other) groups in the past two decades is the seeming success of the lambda CDM model, a compelling realization that primordial fluctuations with the properties inferred from the CMB (which offer direct evidence on conditions 300,000 years after the Big Bang) evolve under the actions of gravity, gas dynamics, and dark energy into galaxies with their observed present-day morphology and clustering properties. The main uncertainty in the simulations concerns the rate of star formation and gas accumulation.

The two main threads of my research—relativistic astrophysics and galaxy formation—have only a limited overlap, but this overlap is crucial in answering a very important set of questions: What are the seeds of the black holes that eventually grow enough to power quasars, and how quickly do they grow? (See Begelman et al. 2006 and Volonteri & Rees 2006.) How quickly do black holes grow through galactic mergers? (See Begelman et al. 1980.) And does the energy and momentum from quasar activity blow uncondensed gas out of the host galaxy and, thereby, quench star formation? With regard to this latter issue, Joe Silk and I suggested that the observed correlation between galaxy masses and the masses of the holes at their centers could be due to feedback: In a big galaxy with a deeper potential well, the hole would have to get more massive and more powerful before this quenching was effective (Silk & Rees 1998; see also Fabian 1999). It's gratifying that these issues have gradually clarified, owing to better observations and more powerful computer simulations.

ORGANIZATIONAL ISSUES

From the 1970s onward, I served on several ESA panels—and, indeed, chaired ESA's Science Advisory Committee during a period when the European participation in the HST was being discussed, along with other NASA/ESA collaborations. It's gratifying that ESA has, in later decades, launched a series of scientific missions that match those of NASA, which spends so much of its much larger budget on human spaceflight. Also, because I participated in international conferences on an unusually wide range of themes, I became involved with several different specialist networks, which led to participation in visiting committees, visiting professorships, and so forth. Although these were predominantly in Europe and North America, I've especially appreciated the chance to forge lasting contacts with India, Israel, Russia, Japan, Australia, and South Africa.

Within the United Kingdom, I did stints on panels for assessing grants and projects—an obligation, like refereeing of papers, that we all share but which is actually helpful in broadening one's knowledge. It is all too easy to grumble about the staff of funding agencies when actually the frustrations we encounter are mainly caused by our peers and colleagues, who decide the fate of our grants and the priorities among new projects. The administrators I've dealt with over the years have generally been efficient and agreeable. But (as in so many hierarchical organizations) competence and commitment seem poorly correlated with status and seniority.

But in the mid-1980s, an issue emerged that absorbed disproportionate time and trouble: the fate of the RGO. In the 1950s, it was moved from its original site (which became a museum) to Herstmonceux Castle, Sussex. But when it became logistically straightforward to operate telescopes on better sites overseas, the UK Research Council decided that the remaining rump of the RGO could be relocated (more cheaply) onto the site of a university, with a remit to develop instrumentation for the UK astronomical community. A merger with the Royal Observatory of Edinburgh (ROE) was the most obvious choice. Cambridge was a contender, but there was ambivalence even in Cambridge about how much of an asset it would actually be. I was among those who were genuinely positive: I thought that there could be real synergy with IOA. Furthermore, it would be good for the United Kingdom because it would offer the only real chance of creating a broad-based national center with international standing and range similar to Munich (the number-one such center on mainland Europe).

After prolonged debate, Cambridge was chosen as the favored site, and by 1990 the RGO label was firmly attached to an undistinguished but functional new building adjoining IOA. I felt that things were looking bright for Cambridge astronomy (especially as Malcolm Longair, previously director of the ROE, had moved to the Cavendish Laboratory, giving us another heavyweight).

By 1991, I had been in my post for 18 years. I took the opportunity to shift sideways into one of the research professorships funded by the Royal Society (Andrew Fabian at IOA already had such a post). The IOA leadership could then be strengthened by refilling the Plumian Chair—with someone who I hoped could engage positively with the new RGO. The selected candidate was Richard Ellis, an optical astronomer who had built up a highly successful group at Durham. This choice (in which, of course, I had no say) seemed good news for a potential Cambridge-based RGO: I thought Richard had the energy and standing to galvanize it. But he instead chose to build up a small instrumental group of his own. The RGO didn't thrive in Cambridge; perhaps I was wrong to think it ever could. By 1998, it lost its independent funding and its work moved to Edinburgh, though several of its best researchers chose to stay and were absorbed by IOA. And Richard Ellis himself soon afterward moved to Caltech.

Despite these diversions, the 1990s saw substantial expansion and strengthening of IOA. Furthermore, optical astronomy in the United Kingdom was boosted by the (albeit overdue) decision to join the ESO. Cambridge has benefited from that, and from involvement in ESA projects such as *Gaia* and *Planck*.

It has, more generally, been gratifying to see the expansion of the United Kingdom's astronomy and space community, as an increasing number of universities have converted their physics departments to include physics and astronomy. I enjoyed a two-year stint as President of the Royal Astronomical Society (though I regret, in retrospect, that I was less of an activist than some of my successors such as Michael Rowan-Robinson and Andrew Fabian have been). At around this time, I was given the title of Astronomer Royal (an honorary one, no longer linked to the RGO), which instilled a continuing obligation to promote the subject.

DIVERSIONS POST 60

In 2002, my colleagues and former students organized a conference to mark my 60th birthday. I was unenthusiastic when the idea was mooted. Academic demographics have led to a proliferation of such meetings—it's clear that some feel pressured to attend them by guilt and obligation, rather than enthusiasm, so I worried about being myself the pretext for such an event. However, I was immensely grateful to those who had organized the meeting. It turned out to be a genuinely stimulating gathering scientifically, as well as a convivial one (**Figure 8**).

But reaching 60 is a time to take stock. I didn't feel I'd made any really major and lasting individual scientific contributions, but I felt fortunate to have been an active participant in debates and to have closely followed (and sometimes influenced) the progress of projects and concepts that will surely be recognized, when the history of science is written, as a collective effort providing one of its most exciting chapters. And I am happy to have done this while based in a near-ideal academic environment, with colleagues (both in Cambridge and spread around the world) who, with hardly any exceptions, I respected and enjoyed interacting with. I've been part of an unusually fortunate generation. Younger colleagues are pressured to have a narrower scientific focus, to have a more competitive attitude, and to be enmeshed in a more vexatious bureaucracy.

I'd noticed three ways in which scientists grow old. Some lose interest in research, turning to other activities—or just lapsing into torpor. Others continue on a plateau, doing what they're good

Figure 8
A group photo from my 60th birthday conference. Photo provided by Lafayette Photography.

at. They generally realize, however, that their last works are unlikely to be their best. That's because most of us get worse, as we age, at assimilating new influences and mastering new techniques: Science is a collective enterprise and its practitioners have to do this to remain cutting edge. There's a contrast with (for instance) composers, who can deepen their work through internal development alone. But there is a third category, which includes some major figures (including, ominously, two especially distinguished former holders of my Cambridge chair: Arthur Eddington and Fred Hoyle). These people retain their research motivation: They are still trying to understand the world. But they no longer get satisfaction from routine work. They instead make incursions into other fields of knowledge where they lack expertise; they over-reach themselves and embarrass their admirers, if not themselves.

Furthermore, the style of research on most of the phenomena I'd worked on has now been transformatively enhanced by computer simulations. This has long been the case for *N*-body gravitating systems and for the evolution of spherical stars. However, until the past two decades, computers weren't powerful enough to generate results that were believable and generic in most of the subjects I worked on; simple analytical models were the best one could do. But relativistic magnetohydrodynamics, dynamical relativity, nonsymmetrical explosions, galactic mergers, and plasma processes can now be analyzed in the virtual world of computers. I realized that, however hard I applied myself, I could never become an adept solo performer in these techniques; I leave such things to younger collaborators and advise students to work with them rather than me; I try to remain a cheerleader and advisor.

I therefore had reasons for wanting to take on something additional to research. Here, I was lucky—in a sense I was too lucky, in that by seizing several opportunities I overshot, leaving less time for research during the decade that followed than I'd have wished if I could have apportioned my effort optimally.

Despite reservations, I didn't give a straight "no" when I was asked if I was willing to be considered as Master of Trinity (Cambridge's largest college). I valued the collegiate system, which renders Oxford and Cambridge unique in combining the qualities of a world-class research university with those of the best American liberal arts colleges. Some earlier Masters had found the College difficult and contentious—I worried I might be joining the "Unholy and Divided Trinity." But I was friendly with the retiring Master, economist Amartya Sen, and his predecessor, mathematician Michael Atiyah. These men had both achieved levels of distinction that I couldn't aspire to; they were both entirely unstuffy and progressive in their views—and they were encouraging. So I took on the role in 2004; I continued in that Mastership for 8 years and was fortunate that my "watch" proved generally uncontentious.

A more substantial commitment, with a lower ratio of flummery to substance, was a five-year stint as President of the Royal Society (the main scientific academy for the United Kingdom and the Commonwealth). I was also appointed a crossbencher in the House of Lords (**Figure 9**).

These new roles were a serious distraction from astrophysics throughout my 60s, but I certainly don't regret this spell of involvement in "public service." I feel in retrospect that—despite limited relevant experience, and an aversion to formality—I coped reasonably well with the routine and formal parts of these roles, as well as introduced various reforms and innovations that were worthwhile.

After leaving the Royal Society in 2010 and Trinity in 2012, I became "officially" retired. I was able to revert to part-time research and again spend time at IOA. But my decade as a small-time public figure had opened up other opportunities. I'm realizing, however, that it might have been better if I had kept a narrower focus rather than spreading my efforts over too many (often ephemeral) activities, especially as some proved unexpectedly time-consuming without the excellent secretarial support that I could draw on preretirement.

Figure 9

Admitting Prince William as an Honorary Fellow at the Royal Society's 350th anniversary celebration in Royal Festival Hall. Also pictured are (*forward sitting row from right)* Prince Philip and Queen Elizabeth as well as (*back row from right*) Aaron Klug and Bob May, my two immediate predecessors as President of the Royal Society. Photo provided by Frank Noon.

POST 2012: SPECULATIVE SCIENCE, OUTREACH, AND POLICY

In his own contribution to this autobiographical "slot" in the *Annual Review of Astronomy and Astrophysics* (Lynden-Bell 2010, p. 6), Donald opined that we shouldn't spend all our time groping at fundamental problems. We should mainly do "bread and butter science"—straightforward extensions of what is known. That's the modest way he would have described much of his work. I've tried to follow his recipe, but have over the years—especially more recently—indulged in two speculative areas: the multiverse and exobiology. I've combined this with an expanded involvement in science and technological policy and with more general writing.

My first solo book was *Before the Beginning* (Rees 1995). It was distinctive in presenting a popular exposition of the multiverse concept, on which I'd written articles dating back to the 1970s (e.g., Carr & Rees 1979). This book's arguments were partly motivated by the seemingly biophilic and fine-tuned character of our Universe, which would occasion no surprise if physical reality embraced a whole ensemble of universes that "ring the changes" on the basic constants and laws. Most would be stillborn or sterile, but we would find ourselves in one of those where the laws permitted emergent complexity. This idea had been bolstered by the cosmic inflation theory of the 1980s, which offered new insights into how our entire observable Universe could have sprouted from an event of microscopic size. It gained further serious attention when string theorists began to favor the possibility of many different vacua—each an arena for microphysics governed by different laws.

I've ever since had a close-up view of the emergence of these (admittedly speculative) ideas. In 2001, I organized (with Bernard Carr and Neil Turok) a conference on this theme. We held it at my home, then a farmhouse, in a converted barn. Some years later, we had a follow-up conference. This time the location was very different: a grand room in Trinity College's Master's Lodge, with a portrait of Newton (the college's most famous alumnus) behind the podium.

The eminent theorist Frank Wilczek attended both meetings. When he summarized the second one, he contrasted the atmosphere at the two gatherings. He described physicists at the first as "voices in the wilderness who had for many years promoted strange arguments . . . . alien to the consensus vanguard of theoretical physics, which was busy successfully constructing a unique and mathematically perfect universe." But at the second meeting, he noted that "the vanguard had marched off to join the prophets in the wilderness."

Some years later, I was on a panel at Stanford University, where we were asked by the chairman, Bob Kirshner, "On the scale, 'would you bet your goldfish, your dog, or your life,' how confident are you about the multiverse concept?" I said that I was nearly at the dog level. Andrei Linde, who had spent twenty-five years promoting a theory of "eternal inflation" said he'd almost bet his life. Later, on being told this, another eminent theorist, Steven Weinberg, said he'd happily bet Martin Rees's dog and Andrei Linde's life (**Figure 10**).

Andrei Linde, my dog, and I will all be dead before this is settled. The concept will remain speculative until physicists can supply a theory that applies under the extreme conditions at the inflationary era and which—crucially—has gained credibility by making correct predictions about

Figure 10

Two speculators about multiverses, Andrei Linde (*left*) and John Barrow (*right*). Photo provided by Anna Zytkow.

physics at low energies where it's testable. But the multiverse concept is science, not metaphysics. And it may be true (Tegmark et al. 2006, Livio & Rees 2020).

Another fringe topic I've become involved with is the Search for Extraterrestrial Intelligence (SETI). Throughout my career, I had followed the arguments about the likelihood of life elsewhere (it's probably the question astronomers are most often asked). The subject has become much more mainstream for two reasons: first, the detection of exoplanets, and the realization that there might be a billion Earth-like planets in the Galaxy; second, the genuine progress by serious biochemists in tackling the conundrum of life's origins. But of course a habitable planet doesn't mean an inhabited planet. And even if simple life were common, advanced life could still be rare (or even, conceivably, unique to our Earth). The subject was boosted in 2015 by the commitment from a US–Russian investor, Yuri Milner, to donate 100 million dollars over a ten-year period toward a deeper and more elaborate SETI program. This initiative is especially welcome, as the subject has traditionally depended on private benefactors. I've been glad to chair an international advisory group for Milner's Breakthrough Listen project.

I've felt no guilt in exploring these topics, as well as following mainstream areas—at the very least through scanning each morning the new postings on the ArXiv and through seminars and coffee-time discussions with IOA colleagues. Cambridge astronomy has greatly expanded in the past decade (with two major new buildings and via more diverse funding). And our university has recently established, under Didier Queloz's leadership, a cross-disciplinary group on exoplanets and exobiology.

I've devoted more time to outreach—both speaking and writing. Astronomers are fortunate that their subject attracts widespread interest. Furthermore, it has—along with evolutionary biology, but unlike nuclear physics, artificial intelligence (AI), or genomics—an unambiguously positive and nonthreatening public image. I would derive less satisfaction if my work could be shared only with fellow researchers.

Before the Beginning was soon followed by another book (Rees 1999) on an anthropic theme. Its synopsis had stated, Just six numbers determine the key features that render our universe an abode for life, and it was an editor, Sarah Lippencott, who suggested "Just Six Numbers" as the title. Slightly different in approach was a book on black holes entitled *Gravity's Fatal Attraction*, coauthored with Mitch Begelman (Begelman & Rees 1996); an updated edition appeared in 2010 and the third in 2020. Another book, *Our Cosmic Habitat* (Rees 2001; again recently updated) was based on a series of Scribner lectures at Princeton University. And *The End of Astronauts* (Goldsmith & Rees 2022)—an assessment of the case for (and against) human spaceflight coauthored with Don Goldsmith—appeared in 2022.

But I've spent half my time in the past decade on more general scientific issues and science's impact on policy. My years at the Royal Society gave me an opportunity—and indeed an obligation—to engage with issues where science impinges on our lives, especially the challenges of energy, health, and the environment. During my tenure, I forged links with other national academies and nongovernmental organizations; I felt it would be a shame to let these advantages lapse.

In particular, I followed up long-term concerns about the fragility of our civilization. The Earth has existed for 45 million centuries, but this century is the first in which one species, ours, has the planet's future in its hands. The concerns are of two kinds: disruption of climate and biosphere by a growing and more demanding human population; and the threats that biotech, cybertech, and AI—which can empower small groups with potentially global influence—could lead to societal breakdowns. Two general books—*Our Final Century* (Rees 2003) and *On the Future: Prospects for Humanity* (Rees 2018)–outlined these concerns. Much of my time has been spent addressing policies on these issues—with, for instance, the National Academy of Sciences, the

Pontifical Academy of Sciences, and the International Scientific Council. Also, membership in the House of Lords offers a platform from which to engage with the UK apparat.

Does being an astronomer offer a distinctive perspective ? I think it does. It plainly fosters an international view. But, more than that, it extends our time horizons. Most people are aware that we're the outcome of billions of years of Darwinian evolution. But many think that we humans are the culmination—the "top of the tree." It's hard for astronomers to believe this. We know that our Sun is less than halfway through its life, and that cosmic evolution could have far further to run. This realization offers an extra motive to ensure that human-induced catastrophes don't foreclose a future of such potential immensity.

So my recent years have been irresponsible, in the sense that I don't direct or chair any substantial organization or group. But I've drawn satisfaction (and I hope made positive contributions) via various commitments and interests—a mixture of astrophysics, outreach, and policy. I shall always be mindful of my good fortune in having such a range of stimulating opportunities and a supportive home and in being embedded in a university—and in a global network of academic colleagues—where the "oldies" are treated humanely. Despite substantial further expansion, IOA has sustained an interactive atmosphere. But I worry that coffee-time conversations are increasingly about grants, job security, and suchlike. Prospects of breakthroughs will plummet if such concerns prey unduly on the minds of even the very best young researchers. I feel my generation of academics (in the United Kingdom and the United States, at least) was specially fortunate. Owing to rapid university expansion in the 1960s, the young outnumbered the old in academia. For this reason, and also because it was routine for senior staff to retire in their 60s, promotion prospects were brighter.

But for the science itself, prospects are surely bright. Many issues that perplexed astronomers in the 1960s have now been settled, and we're now tackling questions that couldn't even have been posed back then. So the coming decades promise to be as exciting as the past few have been. But I conclude on a modest note: Progress will, as in the past, be owed primarily to better technology and instrumentation, not to armchair theorists like me.

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