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BY GEOFFREY BURBIDGE FRS

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BEGINNINGS

Fred Hoyle’s paternal grandfather was George Hoyle, who came from Lancashire to settle in
Gilstead, a village on the edge of the moors above the town of Bingley in West Yorkshire. He
was interested in mathematics and chess, and reportedly had a mass of grey curls. His son, Ben
Hoyle, originally emigrated to the USA but returned to Gilstead to help his widowed mother.
He started working in the wool trade in Bradford before World War I. Fred’s father, Ben
Hoyle, and his mother, née Mabel Pickard, were cousins, because their mothers were sisters.
The sisters were part of the large family of Ben Preston. He was much respected in the village
for the poems that he wrote and the regular articles that he contributed to the Bradford Argus.
Ben Preston’s mother was a member of the Hammond family, who were well-known brewers
in Bradford. Both Ben Preston’s house (Hammondale) and a pub that he built with his brother
John can still be found in Gilstead.

Mabel Pickard had considerable musical talent and she studied at the Royal College of
Music. However, her father died young from silicosis (he was foreman in a local stone quarry),
and she was forced to return home, where she qualified as a teacher.

After Fred was born, on 24 June 1915, his father enlisted in the army (1915–19) and spent
three years fighting in France (1915–18) until the Armistice in November 1918. Ben Hoyle
was the leader of a machine-gun unit. While her husband was away fighting in France, Mabel
could not manage on a soldier’s pay of a shilling a day, so she went to play the piano to accom-
pany silent films in a local cinema. After a time the cinema manager dismissed her, because,
he said, her choice of music was not suitable for the films or the audience. He had to retract
his words when customers insisted that they did not go to the cinema to see the films but to
hear Mrs Hoyle play Beethoven (music that Fred enjoyed to the end of his life.)
When he was a small boy, Fred went to school in Eldwick some distance from home. He went home to lunch and thus he walked back and forth four times a day, covering some six miles a day without sufficient warm clothing. This led to a middle-ear infection and deafness in one ear at an early age. He was the only child to pass his ‘11 plus’, which took him to Bingley Grammar School. He was given a clothing allowance by the school authorities, which his parents insisted he put in his own bank account and so manage his own financial affairs.

In the 1920s the Hoyles had very little money. His mother gave piano lessons and his father violin lessons. His parents gave him every encouragement with his schoolwork, which allowed him to perform chemical experiments in their very small kitchen. He first decided that he wanted to be a chemist and read for a chemistry degree at Leeds. But playing outside in the village in the early winter evenings in an era when there were no street lamps or traffic, he was captivated by the stars he saw in the night sky. He was so fascinated that he determined to find out about them one day.

At one stage, his father took him to see a man some distance away who had a telescope through which the heavens were revealed in a little more detail. When they returned home, his mother had hot soup waiting for them. Then they heard of another man who had a microscope, so they visited him to see what small objects looked like under magnification.

After attending Bingley Grammar School, Fred went to Emmanuel College, Cambridge, where he read mathematics. In 1936 he won the Tripos Part II Mayhew Prize. He then became a research student under Professor P.A.M. Dirac FRS. This was an arrangement acceptable to both of them because Dirac did not like to supervise students, and Hoyle really did not want supervision. Out of this, Hoyle wrote an essay on β-decay that won him the first Smith’s Prize. In 1939 his thesis on quantum electrodynamics won him a research fellowship at St John’s College. The other major event in his life in 1939 was his marriage to Barbara Clark.

But World War II then took him away from Cambridge and peacetime academia to radar and other technical projects for the British Admiralty. It was in this period that he met two of his most important collaborators, Hermann (later Sir Hermann) Bondi (FRS 1959) and Tommy Gold (FRS 1964). He had already met and worked with Ray Lyttleton (FRS 1955) in Cambridge. Lyttleton was a little older than Hoyle and was already an assistant lecturer in mathematics in Cambridge.

The major problems of astrophysics 50 or 60 years ago were the same problems that we are trying to solve today: What was the origin of the Solar System, and how has it evolved? More generally, how do stars form, evolve and die? What is the composition, history and evolution of the extragalactic universe? Are there components other than stars and diffuse gas and dust in the universe?

Today we believe that we can answer all of these questions in much more detail and with much more certainty than was the case in 1939. The reasons for this are twofold. First and most important is that observational techniques involving telescopes and their auxiliary equipment have been tremendously improved. Telescopes are much larger, and there are far more of them. Detector efficiency has been vastly improved, and we now can observe over a very wide band of frequencies, from long-wavelength (metres) radio waves to X-rays and γ-rays. High-energy particles and neutrinos are also now seen as astronomically important. Second, we have far more astronomers and astrophysicists who can interpret and understand the observations and build a basic framework of theory so that we can ask more questions. What we do not have are so many more creative thinkers of the calibre of Fred Hoyle.

By 1939 the revolution in quantum physics had already led to great advances in our under-
standing of the spectra of the stars and the nebulae, and the energy sources of the stars had been shown to be due to nuclear fusion. At the age of 24, quite deliberately, Hoyle moved from quantum electrodynamics into astrophysics because he felt that this was an area of research with a plethora of observations and very little understanding.

Hoyle’s first research papers in astrophysics were written with Ray Lyttleton in 1939–40. They were on the accretion of interstellar matter by stars (3–5, 11)*. It was shown that the rate of addition of mass can be much greater than the earlier work by Harold (later Sir Harold) Jeffreys FRS and Sir Arthur Eddington FRS had suggested, because when collisions between interstellar particles are taken into account there is a focusing effect that takes place in the wake of a star moving through an interstellar cloud. The accretion rate is given by
\[
\frac{dn}{dt} = \frac{G M^2 \rho v^3}{c^5}
\]
where \(M\) is the mass of the star, \(\rho\) is the density of the interstellar medium and \(v\) is the velocity of the star relative to the interstellar gas. We shall return to this later.

Much of the later research on accretion after 1941 was done while Fred was working at the naval radar research establishment. It was in this period that he first got to know and work with two of his closest collaborators and friends, Hermann Bondi and Tommy Gold. We discuss this period next.

**WAR WORK AND INTERACTIONS WITH H. BONDI AND T. GOLD**

(Sir Hermann Bondi FRS and Professor T. Gold FRS have kindly given me information concerning the work of Fred Hoyle and his collaborations with them during the war period 1941–45 and in Cambridge until 1948.)

Fred Hoyle joined the naval radar research establishment based at Eastney Fort East, Southsea. An early achievement of his that is not widely known concerns the radar being used by the major ships of the Royal Navy. Although they had quite effective radars, they were using wavelengths of several metres, which meant that they were able to give the bearing of an attacking aircraft with fair accuracy, but not its height. This meant that directing our fighters to intercept it was largely ineffective. Fred saw that the interference lobes of our radars due to sea reflection meant that the ranges at which the aircraft echo faded gave away its height. He produced graphs that were rapidly distributed throughout the Royal Navy. They enabled radar operators to determine the height as well as the bearing of enemy planes, greatly improving the effectiveness of our fighter defences. This was of particular importance in the Mediterranean battles in the early years of the war. (Centimetric radar, which became available late in the war, could find heights directly.) It was a model of how a theorist can make rapid contributions.

Hermann Bondi returned to Trinity College, Cambridge, from internment in the summer of 1941 and started to work for a PhD with Harold Jeffreys as his supervisor. He was keen to do something relevant to the war. Through the efforts of Maurice Pryce (FRS 1951) he was sent to work on naval radars at Eastney Fort East, Southsea on 1 April 1942. This was the main establishment (where both Pryce and Bondi worked), but they had an outstation at Nutbourne for testing and improving the aerials of their sets. Soon Bondi heard that a remarkable man was based there, a very unconventional and original scientist, ‘a wild man’, Fred Hoyle. They

* Numbers in this form refer to the bibliography at the end of the text.
met several times and became friends. In June 1942 they were all moved to Witley, Surrey, and the establishment was reorganized as the Admiralty Signals Establishment, Witley, with a small theory division having Fred as its head and Bondi as his deputy. It might be worth mentioning that five of the members of this small unit later became Fellows of The Royal Society (Hoyle, Bondi, Tommy Gold, Cyril Domb and the late Jerry Pumphrey).

Tom Gold got his Cambridge degree in 1942 and joined the group in October 1942. His initial reaction when he joined the group was as follows:

It was quite an experience coming into the theory section at the Admiralty’s radar establishment (radio location, as it was then called, at Witley in Surrey). There were six people in the section, all of them intelligent, likable, and witty. I felt I could not possibly have done better for myself than to join this group. The great enigma to start with was the director, Fred Hoyle. He seemed so strange: he seemed never to listen when people were talking to him, and his broad North Country accent seemed quite out of place. I soon learned, however, that he had already made a very significant contribution in showing how the existing radar sets on British ships could be used to obtain, already at a hundred-mile range, a reasonably good estimate of the altitude of an incoming plane. He did this without requiring any new equipment, just requiring the radar officer on the ship to plot the range of first appearance of the plane and the range of subsequent disappearances from the screen. What this was doing was identifying the position of the plane in the interference pattern between the direct wave of the radar set from the antenna to the plane, and the indirect wave, due to the reflection at the sea surface. A very simple method, but, of course, it was of the greatest value.

Before long I also discovered that I had misinterpreted Hoyle’s attitude of apparently not listening. In fact, he listened very carefully and had an extremely good memory, as I would find out later when he frequently remembered what I had said much better than myself. I think he put on this air not to say, ‘I am not listening’, but instead ‘don’t try to influence me, I am going to make up my own mind’. This rugged independence he maintained all of his life, and I have certainly come to admire it greatly.

On 1 January 1943, Hoyle, Bondi and Gold rented a house at Dunsfold, although Hoyle spent some of his time with his wife and son at the house that he rented at Funtington near Nutbourne. Professor Gold tells one interesting story concerning their life there:

As the preparations for the invasion of France were proceeding, the French Channel coast was of course under almost constant bombardment by our airplanes. One such striking force was a Canadian contingent who flew these bombing missions early every morning, mostly with chemically timed bombs that could not be disarmed in any way. An acid inside was just going to eat its way through a diaphragm and when it did, the bomb would explode. Nothing you could do from the outside would stop it; the most sophisticated bomb disposal squad could do nothing with it, even if it knew all the details of its design.

The trouble for us was that this Canadian contingent was operating from an airfield adjacent to the house in which Bondi and I, and Hoyle some of the time, were living. In fact, it was our house that was the first object the heavily laden planes had to clear on takeoff. When we had rented the house, we did not know of this particular drawback, but now we were stuck with it. After a while of being awakened by twenty planes in succession just clearing the rooftop at 4:30 a.m., we got quite used to this, and could sleep through it.

But then one morning I woke up in a state of shock—there had evidently been a very nearby and very violent explosion. I must have been sleeping with my mouth wide open, for a large chunk of the plaster from the ceiling had fallen into it. As I was spitting it out, my bedroom door opened, and Fred Hoyle, who was staying there at the time, stuck his head in and said ‘Did you hear that?’ I said, ‘what do you mean, did I hear that? The house nearly collapsed!’ He said, ‘I know, but I heard, about twenty minutes ago, all the planes taking off except for one, where I heard the take off noise just suddenly stop, and then nothing more. So,’ he said, ‘I went back to sleep, and then came this noise, which of course, woke me up.’ I said to him, ‘How can you be so stupid, to go back to sleep, when clearly what must have happened was that the plane failed to take off, caught fire, and its bombs exploded?’ He said, ‘Well, of course, I know that now, but I couldn’t have done anything about it anyway’.

We later learned, of course, that this is exactly what happened. The crew had been able to save themselves, but the burning wreck eventually exploded its bomb load. It was only a hundred yards from our house. Hoyle’s
attitude, to go back to sleep after hearing the obviously failed takeoff, may seem incomprehensible if one doesn’t know him. Had I heard it, I am sure I would have rushed out of the house as fast as I could in the direction away from the airport. Hoyle was more relaxed about it.

The first collaborative research in astrophysics between Hoyle and Bondi was on stellar accretion which Hoyle was already working on with R.A. Lyttleton. Bondi brought to the accretion problem his knowledge of two-stream states that he had defined and used in his war work on magnetron transmitter valves. For this analysis Bondi was awarded a Research Fellowship at Trinity College, and the joint work with Hoyle was published in 1944.

The joint scientific efforts in this period were this paper entitled ‘On the mechanism of accretion by stars’ (6), and then a paper ‘On the structure of the solar corona and chromosphere’ (14).

In their accretion paper Bondi and Hoyle took the broad sweep analysis of Hoyle and Lyttleton (6) and subjected it to a detailed investigation. The gravitational pull of a star moving through a cloud of gas creates motions in the cloud. If, as they assumed, there is enough molecular gas and dust in the cloud to radiate away the heat generated, the orbits of the gas atoms will be hyperbolic, focused on the ‘accretion column’ behind the star. In the column the motion is purely radial, towards the star near it, away from it beyond a ‘critical point’. In the skin of the column there is a two-stream region, which Bondi had analysed in his Fellowship thesis. The location of the critical point was not determined by the steady-state equations and they had to study the time-dependent situation. They also derived the braking force on the star.

Two comments on this paper are relevant. First, interstellar matter (especially hydrogen) was not observable with prewar instruments and its density was unknown. The solidity of the paper made quite a difference to the perception of its possible importance. Second, although motion near a star is now thought to be mainly outwards, the notion of accretion is still relevant in cosmogony, where, however, angular momentum (not considered by them) is crucial. The application of this mechanism to stellar evolution and to the problem of the solar corona followed naturally.

In the paper on the solar corona (14), Hoyle, Bondi and Lyttleton used the accretion concept to account for the high temperature of the corona (heating from outside). Even so, complex thermodynamic considerations had to be employed to explain the very high temperatures observed.

In 1945, Hoyle, Bondi and Gold all returned to Cambridge. Hoyle rented a house some 20 miles from Cambridge, but Bondi had rooms at Trinity and, after he got married, a flat adjacent to Trinity. Thus until 1949, when Bondi moved further away, Hoyle, Bondi and Gold used Bondi’s rooms as a place where they could spend several hours together every day. It was in this period, starting in about 1947, that they began their work on cosmology.

Hubble’s tremendous prestige as an observational astronomer meant that his estimate of 1.8 billion years for the inverse Hubble constant, and thus an approximate age of the Universe, was rarely doubted. Therefore there was a serious time-scale difficulty associated with the conflict between the age of the Universe derived from its expansion and the age of the Earth and the Solar System, which were clearly known to be much older. This had led Eddington, G. Lemaitre, E.A. Milne FRS and Dirac each to propose their particular theories in the 1930s. Although this problem was much discussed by Hoyle, Bondi and Gold, nothing concrete emerged, before Tommy Gold had the idea of the steady state, probably late in 1947. Fred was also thinking much about nucleogenesis, but the others could not make a serious contribution there, because neither of them was as well versed in nuclear physics. However, the steady-
state idea, with its attendant notion of continual creation as an alternative to an evolving Universe, greatly appealed to the three of them. Fred Hoyle immediately felt the need for a field theory of creation, whereas Bondi and Gold regarded this as premature, and took the philosophical foundations much more seriously than Fred did. Thus two separate papers each proposing the steady-state model were published by Bondi & Gold (1948) and by Hoyle (15).

**Stellar astronomy**

In the 1940s Hoyle, Lyttleton and Bondi applied the accretion hypothesis to various problems of stellar evolution, including the idea that if enough hydrogen were accreted onto massive stars, they could increase their lifetimes on the Main Sequence, and also the suggestion that accretion is responsible for the existence of the solar corona and chromosphere. We know now that neither of these hypotheses is correct, in large part because the density of interstellar matter in our galaxy and in the vicinity of the Sun is far lower than the values required \( (\rho = 10^{-21} \text{ g cm}^{-3}) \) for accretion to be an important mechanism in stellar evolution, or in determining the outer structure of the Sun. However, many years later, astronomers have realized how important the accretion mechanism is in understanding the evolution of close binary stars, both for optical studies and particularly in situations in which one component is either a neutron star or a black hole. It is generally believed that the powerful X-ray sources in the disk of our own galaxy and other external galaxies are such binary systems powered by this accretion mechanism. In addition, accretion disks, which form around the central nuclei of external galaxies, are thought to be responsible for a wide variety of phenomena observed in active galactic nuclei. A line may be traced from the earliest accretion work and the classical ideas of Hoyle, Lyttleton and Bondi to the widespread use and importance of accretion in many areas of modern astrophysics.

Hoyle also performed many important investigations on stellar structure and evolution. Together with Lyttleton he reformulated the earlier work of Eddington on homogeneous stars, because now the energy-generating cycles were understood (2–4, 11). They also showed that stellar models with inhomogeneous structures were required to explain red giant stars (5). This had already been realized by E. Opik, but his publication in the *Tartu Observatory Proceedings* (Opik 1938) was not known to them.

Hoyle argued that stars must, in general, contain a very high ratio of hydrogen to helium. With Martin Schwarzschild (ForMemRS 1996) in Princeton, he gave a complete discussion of the evolution of a low-mass star from the main sequence to the top of the giant branch. With C.B. Haselgrove he estimated from the theory of red giants that the age of the oldest stars in our galaxy is ca. \( 10^{10} \) years. This was done in the period in the 1950s, when A.R. Sandage and others first showed how one could age date the globular clusters and the galactic clusters from the forms of their Hertzsprung–Russell diagrams. In this period, Hoyle and Schwarzschild were generally seen to be the leaders in our theoretical understanding of stellar evolution.

I am indebted to Professor L. Mestel FRS and Professor J. Faulkner for providing me with the following detailed discussion of these works.

‘On the internal constitution of the stars’ (4). This was probably the first paper to incorporate the results of the Bethe–Weizsacker and Gamow–Teller work on thermonuclear energy liberation at the rate \( E_T \) to calculate directly both the luminosity and the radius of homo-
geneous, main sequence stars. The Kramers opacity law \( \kappa = (\rho/\tau^{3.5})(1/\tau) \) is adopted, with Stromgren's calculations of the dependence on \( \rho \) and \( T \) of the 'guillotine factor' \( \tau \), yielding \( \kappa \propto \rho^{1/2}/T^{11/4} \). The homology relations derived for the luminosity and radius show the strong dependence of the luminosity \( L \) on \( M \) and its weak dependence on the coefficient \( E \), as is implicit in Eddington's 'mass–luminosity relation'. The paper confirms the existence of a convective core, so effectively rediscovering Cowling's point-convective model. Some account is taken of radiation pressure, important for the high masses through its effect on the adiabatic exponent \( \gamma \) and so on the extent of the convective core.

‘On the nature of red giant stars’ (5). This paper was the first suggestion in the UK and the USA that inhomogeneity in the mean molecular weight \( \mu \) is responsible for the increase in stellar radii towards the red giant state. The models in this paper and in the later one (16) by the same authors, and in parallel work by C.M. and H. Bondi and by Li Hen and M. Schwarzschild, consist essentially of a homogeneous main sequence star with \( \mu = \mu_c \), surrounded by a homogeneous envelope with \( \mu_e < \mu_c \). It was assumed implicitly that meridian circulation currents within an isolated rotating star would mix the processed, higher \( \mu \) material from the core throughout the envelope, so keeping the star effectively homogeneous, but that a potential giant star would arise if there were very efficient accretion of interstellar gas of low \( \mu \). The relative proportion of low- and high-\( \mu \) material is effectively an extra parameter, along with the mass and the age.

‘The chemical composition of the stars’ (13). Following the work of Stromgren, it was accepted that to get agreement between the physical and the astronomical opacities, stars would need to have a substantial hydrogen content. (Eddington finally conceded this in private but with reluctance, as it went against his rather mystical belief that the stars occur in the mass range where the radiation and gas pressures are comparable.) Subrahmanyan Chandrasekhar FRS and others noted that in addition to the Stromgren solution—in standard notation, \( X = 0.35, Y = 0, Z = 0.65, \mu = 1 \)—there is the one with \( X = 0.5, (1 - X)^{1/5} \), the much smaller opacity resulting from the low metal content, proportional to \( (1 - X)^{1/5} \), being compensated for by the small factor \( \mu^7.5 \) in the mass–luminosity–radius relation. Hoyle noted that the work of Dunham on interstellar matter and that of Russell and Stromgren respectively on stellar atmospheres and the solar atmosphere provide strong arguments in favour of the ‘implausible’ solution with \( X = 1 \). He pointed out that the transition from electron scattering to photoelectric opacity would then occur at masses near 0.5\( M_\odot \). He argued that his calculated \( M - L \) curve gave a somewhat better fit with observation. Nowadays, the argument is modified by the universal primeval helium, so that one has \( (X + Y) = 1 \).

‘The evolution of type II stars’ (20). This is probably the outstanding paper by Fred on stellar evolution. Theoretical work had shown that the efficiency of rotational mixing had been grossly exaggerated, while photoelectric observations of both globular and galactic star clusters, especially by Sandage and colleagues, were showing that the Hertzsprung–Russell (H–R) diagrams of coevally forming stars had giant branches with a modest spread, suggesting strongly that stellar evolution modelling should depend essentially on just one parameter—the mass—rather than on adventitious factors such as stellar rotation and the amount of accreted matter. Hoyle and Schwarzschild follow the evolution of stars in globular clusters from the main sequence to the top of the red-giant branch, assuming zero accretion and zero mixing between convective and radiative zones. In the initial phase, each star develops a burnt-out, isothermal, partly degenerate helium core surrounded by a hydrogen-rich radiative envelope. As the core increases in mass through further hydrogen burning, the envelope begins by
expanding at nearly constant luminosity and so with the surface temperature $T_s$ decreasing. A crucial new feature is the necessity of replacing the previously used boundary condition $\rho c_\text{539} T / \text{unif0ae} 0$, when $R = R_s$, by the directly observed boundary condition that the photospherical optical depth $= 3$. This puts a lower limit on the photospheric temperature, due essentially to the negative hydrogen ion, which yields an opacity that decreases with decreasing temperature, as discussed originally by Rupert Wildt. The predicted red-giant branch in the H–R diagram is a locus of models in thermal equilibrium, steadily increasing luminosities, but only gradually reducing surface temperatures, and so steadily increasing radii. Each star derives its luminosity from hydrogen fusion in a shell surrounding the core, and has an extensive domain in which convection is the dominant energy-transport process to the surface. The luminosity is fixed by conditions deep down. Once the star has expanded sufficiently, the boundary condition keeps $T_s$ nearly constant and the outer radius becomes determined by the requirement that the star be large enough to radiate that luminosity at such an effective temperature. At the top of the initial giant branch, the release of gravitational energy heats up the core centre to a temperature at which helium fusion into carbon occurs through the Salpeter–Hoyle triple-$\alpha$ process. The consequent thermal instability of the degenerate core (the ‘helium flash’) is conjectured to move the star down and to the left in the H–R diagram, on to the ‘horizontal branch’, where the RR Lyrae variables are found.

‘Main Sequence stars’ (23). This paper follows on from (4), presenting computations on the structure of homogeneous stars, with care for the details of the opacity, and incorporating the correct surface condition. Both Population I and Population II systems are studied; Population II stars are assumed to have 5% of the metal abundance and 10% of the CNO abundance of Population I stars. One Population II sequence is given the same helium content $Y$ as Population I (in those days considered the ‘implausible choice’), and the other sequence a much smaller $Y$ value. The Population II masses $\mathcal{M} = M / M_\odot$ vary by only a factor 2–3, whereas the Population I masses considered go up to $\mathcal{M} = 125$. Transition from the pp-chain to the CN-cycle (with simultaneous appearance of the convective core) occurs at $T = (21–22) \times 10^6$ K, while the opacity goes over from photoelectric to electron scattering at $\mathcal{M} = 3$. Over the limited range of Population II masses, the relation $L \propto M^4$ is a good approximation. For the Population I stars, $L \propto M^k$ with $k = 5$ for $\mathcal{M} = 1$, $k = 4$ for $\mathcal{M} = 2$, $k = 3$ for $\mathcal{M}$ between 3 and 30, and at higher masses the growing importance of radiation pressure yields $k = 2$. Low-mass stars, with Kramers opacity and the pp-chain dominant, have radii $R_s$ nearly independent of $M$ until the surface temperature $T_s / \text{unif0ae} 7000$ is reached. Below it the surface boundary condition yields a steady decrease in $R_s$, confirming Osterbrock’s earlier conclusion that accurate results for low-mass stars require the use of the physical surface condition. The authors conclude that provided the correct $T_s$ scale is used, the separation of the zero-age main sequence of the different populations is small, so that main sequence fitting is a reliable technique for determining distance moduli.

‘The ages of Type I and Type II subgiants’ (24). Paper (4) had yielded $6.2 \times 10^9$ years as a typical age for a globular cluster. (A later paper with Haselgrove had yielded $6.5 \times 10^9$ years). In the paper by Burbidge, Burbidge, Fowler and Hoyle (BFH) (22) it had been found that the non-resonant reaction $^{14}\text{N} (p, \gamma) ^{15}\text{O}$ is the determining step in the CN-cycle rather than the resonant reaction $^{12}\text{C} (p, \gamma) ^{13}\text{N}$, so that the cycle operates less efficiently by a factor 10–100. Consequently, in Type II stars with $M / H_\text{17018} < \mathcal{M} < 1.5M_\odot$, the pp-chain dominates, increasing the time of evolution to the main sequence turn-off point to over $10^{10}$ years. This increased age for the oldest galactic components, with its cosmological implications, is now commonly accepted, but it was considered unorthodox in 1959.
From the time of his earliest work on accretion of matter onto stars, Hoyle was concerned with the formation processes of stars, the Solar System and galaxies. In four papers published in 1945 (7–10) he analysed his early ideas concerning star formation and galaxy formation. He was clearly very concerned and aware of the problem of angular momentum.

In a discussion of the origin of the solar nebula (25), he considered the problem with magnetic stresses playing an important role. By implication he had clearly withdrawn his earlier dismissal of Alfvén’s ideas ‘by a straightforward application of the Maxwell stress-tensor’; on the contrary, he now accepted what had been shown formally by Lüst and Schlüter, that a rotationally distorted magnetic field exerts a torque on a proto-Sun, and the consequent outflow of angular momentum into the surrounding proto-planetary gas can be described by the integral over a closed surface of the appropriate component of the moment of the Maxwell tensor.

A much-quoted paper is entitled ‘On the fragmentation of gas clouds into galaxies and stars’ (17). The basic idea of this paper is most simply expressed in terms of the Jeans or virial mass, the minimum, roughly spherical mass that can begin to contract gravitationally against its internal pressure and so separate out from a domain of prescribed density and temperature. For approximately isotropic contraction to continue, the effective $\gamma$ of the gas must be no greater than $\frac{1}{3}$. Hoyle was concerned primarily with a gas of optically thin atomic hydrogen for which collisional ionization and radiation losses ensure that the temperature either stays close to $1.5 \times 10^4$ K or approaches $10^6$ K. Most of the discussion is for a protogalactic cloud with the nearly constant lower temperature. As $\rho$ increases at constant $T$, the virial mass decreases as $(T^3/\rho)^{1/2}$, so it becomes possible for systematically smaller masses to separate out. Hoyle pictured a hierarchical fragmentation process, which terminates at densities for which the optical depth exceeds unity and the isothermality condition is replaced by adiabaticity with $\gamma=\frac{5}{3}$. Hoyle’s argument as to why this not only ‘can’ but ‘must’ happen is that fragmentation allows the gravitational energy released to be thermally dissipated. This basic picture appears to survive dynamical studies of the instability of the collapsing cloud by Hunter and Lynden-Bell, and Rees has given a general argument to show why the opacity criterion should indeed yield masses of stellar order. This is a good example of the success of Hoyle’s intuitive approach. Hoyle was also concerned with the origin of the rotation of galaxies. In 1949 (46) he suggested that protogalaxies acquired their angular momentum as they formed by the tidal torque of neighbouring protogalaxies. This proposal is still the basis of all theoretical work in this field.

After the war, in the period 1945–50, considerable attention began to be paid to one of the major unsolved problems of physics, the origin of the chemical elements. A number of the leading physicists of that period, including Enrico Fermi (ForMemRS 1950) and A. Turkevich, Edward Teller, Maria Mayer, George Gamow and Ralph Alpher (the Alpher–Bethe–Gamow paper) in the USA, and Rudolf (later Sir Rudolf) Peierls FRS in England, tackled this problem by using slightly different approaches. To build up the elements from the basic ingredients—protons, neutrons and electrons—they concluded that they needed a large and intense source of neutrons.
Following the earlier ideas of Lemaitre, who first conceived of the beginning state of the Universe as the ‘Primeval Atom’, they supposed that in the very early Universe a vast flux of neutrons was present, so that in the first few minutes of the expansion the build-up of heavier and heavier elements by the addition of neutrons would occur. They soon found that this scheme would not work, even after the right initial ratio of baryons to photons had been chosen, because there are no stable elements with mass 5, or mass 8, in the periodic table. Only $^2$D, $^3$He, $^4$He and some $^7$Li could be produced. Thus all of the leading physicists, with the exception of George Gamow, R. Alpher, R. Herman and J.W. Follin, gave up the idea. However Gamow, Alpher, Herman and Follin showed that the lightest isotopes could be made in this way (see Alpher & Herman 1950) and were so intrigued by the physics of the expanding hot fireball that they continued their studies.

In the same period, in 1946, Fred Hoyle (12) published the seminal paper on the origin of the elements from hydrogen. He had realized that it was impossible to build the heavy elements by neutron capture in an early Universe. He could also see that there was already enough observational evidence to show that there were differences in abundance of the elements in different stars, so that a universal process appeared to be very unlikely. However, in looking at the observed abundance curve he noticed the small peak with a maximum at $^{26}$Fe (the maximum of the packing-fraction curve) and realized that this must be due to an equilibrium process, although it was clear from the earlier calculations of Chandrasekhar and Henrich that not all of the elements could be built in this way. However, Hoyle calculated that the Fe-peak elements must have been produced at very high temperatures, $ca. \ 3 \times 10^9$ K, in the interiors of stars. He thus concluded that practically all of the elements must have been synthesized in stellar interiors, but under a variety of physical conditions.

Salpeter (1952) showed that $^8$Be, which is unstable but can be formed from two $^4$He nuclei, can, under the conditions found deep in the interiors of giant stars at temperatures $ca. \ 2 \times 10^8$ K, combine with a third $^4$He nucleus to form $^{12}$C, which is stable. Given the importance of carbon in the Universe and the abundance ratio C:O, Hoyle became convinced that the reactions involving $^8$Be + $^4$He $\rightarrow$ $^{12}$C must be much more important than the subsequent reaction $^{12}$C + $^4$He $\rightarrow$ O$^{16}$ + He$^4$ $\rightarrow$ $^{20}$Ne, so that the abundance of $^{12}$C will dominate. For this to occur he predicted that there must be a resonance in the reaction $^8$Be + $^4$He $\rightarrow$ $^{12}$C at an energy level in $^{12}$C some 7 MeV above its ground state, making what became known as the triple $\alpha$ process $^4$He + $^4$He + $^4$He $\rightarrow$ $^{12}$C a resonant reaction and thus much more rapid than Salpeter’s result indicated. This prediction was confirmed experimentally by Ward Whaling in the Kellogg Radiation Lab at Caltech, led by W.A. Fowler and the Lauritens. The result, predicted purely from astrophysics, electrified the nuclear physics community. They began to believe in nuclear astrophysics and in the genius of Fred Hoyle. By the early 1950s it was becoming clear that element synthesis in stars was probably the correct explanation for nearly the whole of the elements in the periodic table.

Fred published another extensive paper in 1954 on stellar nucleosynthesis (18). In the same period, A.G.W. Cameron at Chalk River in Canada (Cameron 1955, 1957) and W.A. Fowler together with E.M. Burbidge (FRS 1964) and me (Fowler et al. 1955) began to try to explain the existence of the neutron-rich isotopes of the heavier elements by arguing that they were produced by capture of neutrons from reactions such as $^{13}$C + $^4$He $\rightarrow$ $^{17}$O + n, or $^{21}$Ne + $^4$He $\rightarrow$ $^{25}$Mg + n in the interior of giant stars. Fowler and the Burbidges were at that time working at the Cavendish Laboratory in Cambridge. We began in that period to work with Fred, and in 1955 we all returned to Caltech, where we worked intensively on all aspects of stellar nucleosynthesis.
An important aspect of all of this work was the growing evidence that not only were heavier elements being synthesized in the deep interiors of the stars and ejected, to be condensed into new generations of stars, but there were many different types of star that showed direct evidence for nucleosynthesis in the spectra of their photospheres; that is, they build elements in their deep interiors, which then, through mixing processes, reach the surface.

In 1956 we thought we had found another observational pointer. Several years after the event, it was reported in Physical Review Letters that the highly transuranic element \(^{254}\text{Cf}\) had been synthesized in the hydrogen bomb test at the Bikini atoll in 1952. The half-life for radioactive decay of \(^{254}\text{Cf}\) is \(55 \pm 1\) days. We noticed that this decay curve matched almost exactly the decay curve of the supernova in IC4182 measured by Baade in 1937. Thus here was apparently direct evidence that very heavy unstable elements, which are neutron rich, were produced in supernova events (21). At the time, this gave us further support for the idea that the rapid neutron capture process in stellar nucleosynthesis (the r-process) takes place in supernova explosions. Much later it was shown that our identification of \(^{254}\text{Cf}\) was incorrect. We now know that the light curve decay is due to \(^{54}\text{Cr}\) and not \(^{254}\text{Cf}\). But the idea that the light curve is due to the radioactive decay of an unstable element with a suitable half-life was right!

By late in 1956 we had concluded that all of the isotopes with the exception of D, He and Li, Be and B are made in stars. We then wrote a long paper describing all of this work, which was published in Reviews of Modern Physics in 1957 (22). This is the paper that has been so widely quoted and referred to as B2FH after its four authors. In the same period, A.G.W. Cameron also wrote a similar account of stellar nucleosynthesis, which unfortunately has not been given as much publicity.

There are two further points to be made concerning this theory. Much later, in 1987, a supernova (SN1987a) was detected in a nearby galaxy, the Large Magellanic Cloud, and high-energy neutrinos were detected from that outburst (Arnett et al. 1989; McCray 1993). Because a large flux of neutrinos had been expected in such a supernova nucleosynthesis event, this provided the first direct evidence that nucleosynthesis does indeed take place in supernova outbursts.

The second point concerns the origin of the second most abundant element, helium. In B2FH it was not supposed that helium was synthesized in stars during the normal processes of stellar evolution. This is because the observed He : H ratio is too large for most of it to have been produced in normal stars in \(10^{10}\) years. However, it has been known for many years that if we suppose that the abundance ratio He : H seen in stars is the same in the whole of the baryonic matter in the Universe, and the production of helium is due to hydrogen burning in stars, the energy that must have been released is much greater than the energy contained in starlight (19) (Burbidge 1958). It turns out that it is almost exactly the currently observed energy density of the cosmic microwave background radiation. This was already noted by Fred and was mentioned in the 1967 paper of Wagoner, Fowler and Hoyle (41). This suggests not only that all of the helium was produced by hydrogen burning in hot stars (at an earlier stage of galactic evolution) and was degraded into microwave radiation by dust: this means that it has nothing to do with the early Universe. This idea is, of course, anathema to the Big Bang cosmologists, who have continuously ignored the result. However, Hoyle and I finally published the argument in detail in 1998 (57), and pointed out that on this basis it seems likely that all of the isotopes were made in stars, the deuterium being synthesized in flares on the surfaces of stars, and Li, Be and B by spallation reactions. Thus it may well be that all of the isotopes are due to stellar nucleosynthesis.
Starting with his work on accretion with R.A. Lyttleton, Hoyle realized that detailed information about the interstellar medium was required if we were properly to understand the accretion process in star formation. These studies came about after criticism by Atkinson (1940), the basic argument being that accretion onto a star from a cloud of atomic hydrogen at a temperature of \( \text{ca.} 10,000 \) K, a value that had been derived by Eddington, would be too small for substantial accretion to occur. This led Hoyle and Lyttleton (1) to a detailed discussion of the physics of heating and cooling mechanisms in such a cloud, and the realization that the key is cooling by molecular hydrogen, \( \text{H}_2 \), a result that was about 30 years ahead of its time.

They showed that the lifetime of molecular hydrogen in interstellar space would be very long so that any molecules that formed would remain until they entered an \( \text{HII} \) region. They also discussed the formation of interstellar molecules in general, this at a time a few months before the first observations of interstellar CH had been made.

In discussing the formation process they realized that normal three-body reactions cannot take place and that some special two-body formation process is required. They did not get this process right, but realized correctly that in a gas with no heavy elements some molecular hydrogen must form before star formation can take place.

In the 1950s Hoyle worked on the fragmentation of gas clouds into galaxies and stars (see above) and did not return to the physics of the interstellar medium until 1960, when he began to work with a new research student (N.C. Wickramasinghe) to investigate the composition of interstellar grains. At that stage it was thought that the most likely composition of the grains was dirty ice, as had been proposed by F. Whipple and worked on extensively by H.C. van de Hulst (ForMemRS 1991) and others. Hoyle and Wickramasinghe proposed that the composition of the grains is dominated by carbon, and that they were formed and ejected from cool carbon stars (28). Their arguments were that the main constituent of the dust is graphite, which arises from cool carbon-rich stellar atmospheres and is expelled into the interstellar medium. The arguments were based on the fact that the high average extinction requires that the grains be composed of the abundant elements carbon and oxygen, that the theoretical extinction curve for graphite fits the observations, that graphite cannot be formed in the diffuse interstellar medium, that graphite has a high enough albedo to explain reflection nebulae, that graphite can form in the pulsating carbon stars, and that radiation pressure can eject the grains.

By the late 1970s (48, 49) they concluded that the ultraviolet observations of the extinction curve near 2175 Å suggested that complex hydrocarbons must be main ingredients.

In the 1970s new spectroscopic data from space-based ultraviolet and ground-based infrared observations led to a large increase in the data available for the identification of interstellar grains. In a series of papers in 1976–77 Hoyle and Wickramasinghe shifted their attention to organic compounds and polymers as the source of ultraviolet and infrared features. For example they compared the infrared emission expected from polysaccharide grains with the observed spectra from a wide variety of sources (47). For some sources such as OH 26.5 + 06 they showed a comparison between the observed spectrum from 2 to 30 \( \mu \)m and the expected emission from an optically thin source of polysaccharides at 400 K (50).

The goodness of fit over a wide spectral range was taken as evidence of a correct identification, although they left open the possibility that other organic compounds were important.
At first they suggested that the polysaccharides might be produced from gaseous formaldehyde in interstellar space but a few months later they suggested (49) that polysaccharides could form in an outflow from a very young type O star with a huge mass loss rate and an optically thick cooler envelope, based on an outflow model for strong infrared sources without H II regions. (44).

At about the same time they realized that graphite by itself could not produce the shape of the important ultraviolet feature in interstellar extinction at 2175 Å. However, allowing for absorptivity from hydrocarbons in the form of interstellar C and N, the peak wavelengths agreed very well with what was observed, and the shape was close, although not perfect. Hoyle and Wickramasinghe pointed out that the observed strength of this feature would require only ca. 10% of interstellar C and N to be in the form of the hydrocarbons.

Although these specific hydrocarbons may not be the dominant form in the interstellar medium it is now generally accepted that some type of hydrocarbon, possibly polycyclic aromatic hydrocarbons, are an important component of interstellar matter.

As is well known, Hoyle and Wickramasinghe went on to suggest that even more complex compounds are present in interstellar matter and in comets, including life itself in the form of bacteria. Fred Hoyle firmly believed that life could not have originated on Earth: he felt that the probability of life forming from non-living material was simply too small to happen on Earth. The work on panspermia attracted widespread hostility at the outset. Biologists seemed offended by the incursion of astronomy into biological sciences, and maintained that the origin of life was a self-contained affair on Earth. However, Hoyle and Wickramasinghe’s challenge of this gained credibility as soon as it was discovered that the oldest evidence for life on Earth coincided with a phase of intense cometary bombardment, about 4 billion years ago. They believed that this shows clearly that life springs up in the harshest possible impact-riddled environment. Hoyle and Wickramasinghe’s first ideas (also hotly disputed in the 1970s) was that comets were at the very least the bringers of organics to the Earth. This position is now appreciated by many in the fields of astronomy and biology.

Although it is generally thought that Hoyle adopted his views on the effect of interstellar matter on the Earth late in his career, at least outside his science fiction, this is not so. Hoyle and Wickramasinghe were also the first to propose a connection between cometary impacts and ecological catastrophes. They suggested that a veil of the cometary dust enveloping the Earth during a comet encounter could have led to the diminution of sunlight, a decrease in photosynthesis, withering of leaves and plants and the death of the dinosaurs 65 million years ago (51). They also connected comets with the onset and termination of the ice ages.

**COSMOLOGY**

Soon after his death, many of the popular obituaries of Hoyle concentrated on his ‘controversial’ role in cosmology. A fairer account is as follows.

He always felt that one of the most important questions in cosmology is whether or not there is an important interrelation between the structure of the Universe and the laws of physics. Early in the twentieth century there was a widespread belief, derived from the arguments of Mach, that the phenomenon of mass (inertial mass) arises from the interaction of local systems with the Universe at large. However, as Einstein developed his theory, it was formulated so that the masses of the particles belonged to themselves—they were constant
with no obvious relation to the Universe. However, Einstein argued that if the Universe is static, mass would be constant. And up to the first part of the twentieth century, astronomers did indeed believe that the Universe (which was to them all contained in the Milky Way) was static.

All of this changed with the discovery in 1929 by Hubble that the Universe is expanding, and that the solutions of Einstein equations that can best represent the Universe were the expanding solutions of Friedmann and Lemaître. Einstein had to abandon his position, and in doing this he stated that using the cosmological constant that was required to give a static solution to his equations was his biggest blunder. Dirac (1938) returned to the Machian point of view by arguing that if the Universe is non-static, the laws of physics must also change with time. This approach was pursued by Jordan and by Brans & Dicke (1961), but here the difficulty is that the changes required in the laws of physics mean that they no longer satisfy the classical tests of relativity, the bending of light by the Sun and the rotation of the perihelion of Mercury.

This was one of the main reasons why Hoyle (15), and separately Bondi & Gold (1948), proposed the steady-state cosmology in 1948. They realized that one can return to Einstein’s original view by developing a model in which there is a constant environment without demanding a static Universe. The Universe can be non-static provided it is stationary, and this requires that matter be created continually. In the Friedmann models, matter is created at all once in the origin of the Universe, and if matter can be created suddenly, why can it not be created continuously? This was the background to the development of the steady-state cosmology that Hoyle developed through a field theoretical approach invoking a creation (repulsion) field, thus modifying Einstein’s equations in the strong field approximation, while Bondi and Gold invoked what they called the perfect cosmological principle.

As mentioned above, they were also very much aware of the fact that at the time it seemed that there was a direct conflict between the age of the Universe estimated from its rate of expansion measured by Hubble and Humason (ca. $1.8 \times 10^9$ years) and the ages of the Solar System and some stars (ca. $3 \times 10^9$ years).

It was immediately seen that the steady-state cosmology could be tested by observations of various kinds, although the rate of creation of matter required was so small that it would not be detectable. It was also clear from many comments and commentaries in that period that in the competition between the steady-state and the Friedmann models (somewhat derisively named the Big Bang by Hoyle in 1949) that most of the astronomical community and its leaders much preferred the Big Bang.

In part, this might have been because the concepts underlying the theory were thought at that time to be of doubtful validity, but there were other strong emotions. For some the idea of creation was acceptable if it took place at an early time and only once—a big bang—but for it to go on steadily at a rate that made it undetectable locally was unacceptable. For some it is clear that Western religion is important when origins are being considered; parallels between the Big Bang and the creation described in the Old Testament are inescapable, and some were driven by religious motives.

Thus, the approach taken by nearly all of the professional community of astronomers was to look for evidence to show that the steady-state model was wrong. Detailed references are given in the book published by Hoyle, Burbidge and Narlikar in 2000 (T6 in Additional references). A series of observational arguments were made all purporting to show that the model was wrong, without anybody looking for evidence that might support the model. The steady-
state model made clear predictions that made it vulnerable to such tests, whereas the Big Bang could usually be made to agree with the observations because there were (and are) many evolutionary parameters that are not specified by the theory.

The first of these lines of evidence was the announcement in 1950 of the discovery of what became known as the Stebbins–Whitford effect. The preliminary measurement of the colours of distant galaxies by Stebbins and Whitford suggested that, after correcting for the normal redshift effects, they were redder than they should be. This would have meant that an evolutionary effect proportional to redshift (distance) was present, and this would be evidence against the steady state. While the observational evidence was believed and quoted by many, later observations by Oke & Sandage (1968) showed it to be incorrect: there is no excess reddening.

The next observational argument brought against the steady state was that the observations at large enough redshifts showed that the Universe is decelerating, whereas in a steady-state Universe it must be accelerating. The result was marginal, but for many years the suggestion that it was decelerating was used as an argument against the steady state. The most recent observations of supernovae in distant galaxies made since 1998 have demonstrated that the Universe is indeed accelerating (Perlmutter et al. 1999; Riess et al. 1998, 2000, 2001), as was predicted by the steady-state theory (15) (Bondi & Gold 1948). However, it is interesting to note that the leading observers (Perlmutter et al. 1999; Riess et al. 1998, 2000, 2001) never acknowledged that they had found a result that supported the classical steady-state cosmology and its variants. Instead they fitted their results into a modified Big Bang model, by ascribing the acceleration to a non-zero cosmological constant and ‘dark energy’ that is really nothing more than continuous creation in a Big Bang Universe.

The observational evidence that led to the most acrimony between Hoyle and his student and collaborator J.V. Narlikar on the one hand, and the Cambridge radio astronomers on the other, centred on the counts of distant radio sources. Although at the time that this dispute began it was not known what these sources were, or how far away they were, it was known that they are extragalactic. Martin (later Sir Martin) Ryle FRS and his group in Cambridge realized that simply by counting them they could be used as a powerful cosmological tool. If they all have the same luminosity $L$, and are distributed though Euclidean space with uniform density $\rho$, the intensity of a source at distance $r$ is $S = L/4\pi r^2$ and the number per unit solid angle within a distance $r$ is $N = \frac{1}{2} \rho r^3$. The number having an intensity greater than $S$ is then given by

$$N = \frac{1}{2}(L/4\pi)^{3/2}S^{-3/2}\rho,$$

and if we plot $\log N$ against $\log S$ we should find for sources in Euclidean space a straight line with a slope of $-\frac{3}{2}$. The effect of a spread in intrinsic luminosity $L$ will not seriously affect this result provided that the range of luminosities is not too great.

Thus, the method used was to construct a $\log N$–$\log S$ curve from the catalogues of radio sources. When the $\log N$–$\log S$ method is applied to sources distributed over a large range of distances, it can be shown that the effect of the curvature of space is to flatten the $\log N$–$\log S$ relation so that the slope falls (numerically) below $-1.5$ at the faint end, the amount of flattening being determined by the cosmological model. It was therefore claimed that, without knowing very much about sources, the counts would be a powerful cosmological tool.

Now, the steady-state cosmology predicts that, taken on a larger scale, the sources should not change in their statistical properties. This means that the $\log N$–$\log S$ curve should flatten
to a value that is different from the evolutionary models. Ryle concluded that because the slope of the \( \log N - \log S \) curve from the first sample of 3C sources was far steeper than \(-1.5\) (he originally claimed that the slope was \(-3.0\)) the steady-state cosmology was ruled out. He believed that the steep slope meant that there must have been many more sources in the past, suggesting strong evolution.

The alternative possibility was that the counts were in error. In fact, by 1957 the Australian group led by Mills & Slee (1957) concluded that the discrepancies between their catalogue of sources and that of Ryle were largely due to instrumental effects associated with the Cambridge data. Of course Ryle totally rejected the Australian data, which were not given any of the publicity afforded the Cambridge data. The Australians concluded that there was no clear evidence for any effects of cosmological importance in the results. But the damage to the steady-state model was serious, because of the public way in which Ryle had attacked it and his refusal to acknowledge any errors. In addition, because he was believed by so many eminent scientists, nearly all of them unfamiliar with the technical aspects of the research, it was generally believed that the steady-state theory had been disproved.

However, the dispute continued, with Hoyle and Narlikar maintaining their doubts about the reliability of the data, and Ryle always insisting that he had disproved the steady state. In February 1961, Ryle presented his finding at a meeting of the Royal Astronomical Society, based on the radio survey that is now known as the 4C survey. This contained another sample of radio sources detected at Cambridge; because the survey went fainter, it contained more radio sources. The claim was that the super-Euclidean slope of the \( \log N - \log S \) curve was now free from errors and uncertainties to a sufficient extent so that the steady-state theory was clearly disproved. Hoyle and Narlikar countered this claim by showing that an inhomogeneity in the distribution of radio galaxies on the scale of ca. 50 Mpc would produce fluctuations in the slope of the \( \log N - \log S \) curve, and that a steep slope was merely indicative of a local deficit of sources. They followed this with detailed modelling and computer simulations (26, 27). Thus, they argued that the data could not be relied upon to disprove the steady-state model.

It is worth recalling two aspects of this work that were not understood at the time. First, Hoyle and Narlikar argued that an inhomogeneity on the scale of 50 Mpc would arise from the inhomogeneities on the scale of superclusters. Although they cited recent work by Abell on clusters of galaxies to support this argument, the general reaction of the astronomical community was that the Universe does not show inhomogeneities on this scale. However, 20 years later the reality of superclusters and voids suggested by Hoyle and Narlikar in their 1961 paper was well established. The second aspect of this work was that it was probably the first large-scale computer simulation in cosmology, for which the IBM 7090 computer in London was used. Fred Hoyle was one of the first in the field of astronomy to appreciate the potential of the computer revolution.

Thirty years later, it is clear that the steady-state theory had not been disproved by those data. However, at that time the community believed that it had been disproved, and they maintained this position until a newer argument came along. This was the emergence of the cosmic microwave background (CMB) radiation as a cosmological probe.

This background radiation field had first been detected by McKellar (1941). He showed from the level of excitation of interstellar molecules that the black-body temperature must lie between 1.8 and 3.4 K. However, this result itself and its significance have always been ignored by cosmologists, almost certainly because their leaders did not know of it when they
began their work on the early Universe in the period after 1946. Thus the detection of the CMB is usually attributed to Penzias & Wilson (1965). However, for several years before this, several powerful groups of cosmologists, particularly the Russian school of Y.B. Zeldovich (ForMemRS 1979) and his students and colleagues in Moscow, and R.H. Dicke and his students in Princeton (who had rediscovered the ideas of Gamow, Alpher and Herman and were attempting to detect the radiation), had become convinced that the hot Big Bang model was correct. They were actively promoting the view that the microwave background radiation arising from such a big bang would be found. They set the tone for the view that the Penzias–Wilson discovery not only showed that their view was correct, but that it brought the death knell to the steady-state theory.

How was this certainty displayed? For example, the Russians always called the microwave background the ‘relict’ radiation, implying that they already knew where it came from. In addition, when Penzias and Wilson detected the radiation at one frequency there was, of course, no evidence that the radiation had a black-body energy distribution as it must have if, according to Gamow and his colleagues, it originated in an early Universe. It took some years for this to be established, but a very heavy bias was involved in all future measurements. All of the observers believed that the curve should have a black-body form, so that when some of the early experiments gave a flux lying above the black-body curve they were immediately suspect. Of course, ultimately the COBE data showed that those measurements were wrong and the curve does have an almost exact black-body form out to centimetre wavelengths.

The view in the 1960s was that to get such a black-body curve from many discrete sources, as is required in a steady-state theory, was highly contrived, and could in effect be ruled out. This discovery was therefore considered to be the strongest piece of evidence against the steady-state theory, stronger than the Stebbins–Whitford effect, the observed value of the deceleration parameter, or the counts of radio sources. In 1968 Hoyle gave the Bakerian Lecture (42) summarizing the situation from his point of view.

He had wavered in his view concerning the correctness or not of the Big Bang model earlier in the 1960s. In that period, following the earlier work by Hayashi (1950) and by Alpher and Herman (1950), with Roger Tayler (FRS 1995) he made a new calculation of the He : H ratio synthesized in a big bang (31). After this more calculations were made by Peebles (1966) and in the most detail by Wagoner, Fowler and Hoyle (41). But Hoyle was never comfortable with the idea that the lightest isotopes were made in primordial nucleosynthesis, and as was mentioned in the work with Wagoner and Fowler, the possibility was considered that the light isotopes might have been made in supermassive stars with masses of order 10^8 times that of the Sun.

However, for most cosmologists the Big Bang model had prevailed. But in a very basic way Hoyle remained unconvinced. First, he was aware that there were many problems and also many assumptions that had to be made if the physics of the early Universe as it was portrayed were to be accepted. I shall discuss these below. Second, and more importantly, newer observational results made some of the basic assumptions invalid. The new results came from the radio-astronomical studies of extragalactic sources. Identifications of these sources had shown that they often arise in distant galaxies, and that the amounts of energy in non-thermal forms (meaning fluxes of relativistic particles and magnetic fields) being released from the nuclei of these galaxies must be very large, ca. 10^{46} ergs or more.

In 1963, Fred Hoyle and William Fowler (29, 30) proposed what seemed at the time an outrageous idea, namely that there might exist supermassive stars with masses upwards of a
million solar masses and that the gravitational collapse of such objects might explain the vast energies associated with the radio sources. (The sequence of events that led them to this idea was as follows. Several years before, I (Burbidge 1959) had shown that very large energies, up to $10^{61}$ ergs, are present in the form of relativistic particles and magnetic fields in many extended extragalactic radio sources. Because we believe that they must be generated in very small volumes in the nuclei of these galaxies, their origin must be attributed to nuclear energy, which can easily be ruled out, to gravitational energy, or to creation energy. In 1961 I had proposed a model that required chain reactions of supernovae to be responsible (Burbidge 1961). Hoyle, Fowler and I were discussing these ideas in that period, and Hoyle and Fowler were not convinced by my scheme. Thus instead they turned to the model involving the gravitational collapse of a single massive superstar.) When they discussed the stellar evolution of such a superstar leading to a super-supernova, they also concluded that in such huge masses, the force of gravity becomes supreme, and the usual stellar pressure is unable to hold the object in equilibrium for long. This will then lead to gravitational collapse. Hoyle and Fowler argued that such compact supermassive objects would have at their disposal tremendous gravitational energy that, when converted to dynamical energy, would ultimately lead to sources with a huge emission of energy.

In addition, starting in 1960, radio and optical astronomers discovered a new class of objects which are very powerful, with very large redshifts, almost star-like in appearance, and quite unlike normal galaxies or stars. These were named quasi-stellar objects (QSOs), or quasars. This discovery led to an appreciation of the theoretical work on gravitational collapse of Hoyle and Fowler, so much so that an international symposium on relativistic astrophysics was convened in Dallas, Texas, in December 1963, in which the Hoyle–Fowler idea was discussed in conjunction with observations of the newly discovered quasars (Robinson et al. 1965). The meeting was unusual in the sense that for the first time it brought together general relativists and high-energy astrophysicists, and the subject of ‘relativistic astrophysics’ was launched. The black-hole enthusiasts of today are sometimes unaware that the Hoyle–Fowler concept of a collapsed massive object was none other than the massive black hole currently in fashion, which with an accretion disk is currently believed by many to give rise to the observed outbursts in many kinds of non-thermal sources.

The QSOs were soon found to have properties very different from those of normal galaxies. First, there was no correlation between their redshifts and apparent magnitudes, as occurs for normal galaxies (the Hubble relation), and from which it was originally shown that the Universe is expanding. Second, it was soon discovered that the flux from the QSOs varies in time, something that was unheard of in galaxies. It means that the radiating regions of the QSOs are very small—probably no larger than the Solar System. Hoyle immediately saw that this placed severe limits on the properties of the radiating process and with Sargent and me (37) showed that either highly relativistic motion of the radiating surfaces was required or these objects were much closer to us than would be deduced if it were assumed that their redshifts are due to the expansion of the Universe.

The fact that the relation between the redshift and the apparent magnitude is essentially a scatter diagram meant that there is no prima facie evidence that these redshifts, as distinct from those found in normal galaxies, are due to expansion.

In 1966 Hoyle and I wrote a detailed paper (36) discussing whether or not the evidence pointed to the QSOs lying comparatively nearby, or whether they lie at cosmological distances. Soon after this it became apparent from statistical arguments, and the work of H.C. Arp...
of the Mount Wilson and Palomar Observatories, that a number of the bright QSOs with high redshifts are closely associated with bright galaxies with very small redshifts.

There were many observations suggesting that QSOs, and thus a fraction of the radio sources, do not lie at cosmological distances and therefore cannot be used for cosmological studies. By 1971 Hoyle understood this. He discussed all of these results in a series of lectures given at the State University of New York in Buffalo (43). He saw at once that this set new rules for cosmology.

In the 30 years since then, the observational evidence supporting this view has grown, but unfortunately practically all of the other leading cosmologists have ignored this area of extragalactic astronomy. There has apparently been such a widespread conviction that the Big Bang model (now called the standard) point of view is correct that it has been supposed that such results can be disregarded. However, it is these phenomena, and others discovered in high-energy astrophysics, that determined the direction of Hoyle’s research in extragalactic astronomy after about 1970. The new observations started with the identification of the extragalactic radio sources with very active (explosive) centres of galaxies—now generally called active galactic nuclei. They and the QSOs have shown without question that there are major sources of energy and ejected mass in the Universe that appear sporadically in a wide variety in galaxies long after these galaxies formed.

In the 1950s and 1960s, V.A. Ambartsumian (ForMemRS 1969) had already made the radical proposal that the centres of galaxies are places where the material of new galaxies is created and ejected (Ambartsumian 1965). While Ambartsumian’s ideas based completely on the observations have been largely ignored by the cosmological establishment, these are the cosmogonical ideas out of which Hoyle and his colleagues formulated in the 1990s the quasi-steady-state cosmology (QSSC) in which it is argued that the centres of active galaxies are the creation sources, and it is in them, in the vicinity of near black holes, that the C (Creation)-field operates. Thus matter is being created out of a set of singular points. This leads to expansion and contraction with a period of about $4 \times 10^{10}$ years superimposed on an overall expansion with a characteristic time of $c a. 10^{12}$ years.

This theory, based on the Creation field theory of Hoyle and Narlikar, was developed in several detailed papers published since 1990 (52–54, 56) and in a book (T6 in Additional references). Although many details remain to be worked out, it is likely that all of the observed properties of the Universe can be understood within the framework of this theory.

**Problems with the Standard (Big Bang) Cosmology**

As far as light element production is concerned, Hoyle’s discomfort with the standard Big Bang model was associated with the fact that he was only too well aware that despite the refinements that led the community closer and closer to their belief that agreement between theory and observations ‘proved’ that the theory was correct, everything depended on the initial choice of the ratio of photons to baryons, for which there is no theoretical basis at all. Alpher & Herman (1950) put in a ratio of photon density to baryon density close to $3 \times 10^{-10}$ as Gamow did originally, close to the value used today, but the only justification was to get the right answer for the He : H ratio. The background to this is that Hayashi (1950) had shown that at temperatures of $c a. 10^{10}$ K neutrons and protons are brought into equilibrium by their reactions with electrons and neutrinos and their antiparticles. Pair production and annihilation
keeps the neutrinos or antineutrinos in equilibrium with electrons and positrons, and these are kept in equilibrium with the photons. Thus the key ingredient for nucleosynthesis, the ratio of neutrons to protons, is uniquely determined by the temperature, and thus the initial condition is fixed.

Other major problems that Hoyle always had difficulty with were the arbitrary nature of the physics of the early Universe, and the way that the Big Bang theorists try to explain galaxy formation. The arbitrary nature of the early Universe can be seen by reversing the time axis associated with the expansion. As the Universe shrinks the radiation energy begins to dominate the matter and ultimately the matter is broken down into quarks. We now move out of the realm of known physics. A further contraction by a factor of about $10^{10}$ is invoked, leading to what is called a ‘phase transition’ in which everything is converted into a new kind of so-called scalar particle. These scalar particles are supposed to interact to produce what is described as a ‘false vacuum’ maintaining positive energy at all costs. This false vacuum consumes space-time in a process of deflation—this is the inflation epoch of Guth and Linde when time is reversed. The consuming of space-time leads to what? To a quantum transition to somewhere else!

Of course, the basic problem is that all of these discussions lie completely outside the realm of testable physics, and everything becomes a matter of taste. Even if this kind of scenario is accepted, there is the major problem of understanding how structures (galaxies) condense in an expanding Universe. Hoyle had written a classical paper on gravitational collapse in 1953 (17), but he and others understood that gravitational instability involving normal baryonic matter alone would not lead to the formation of galaxies as we see them in a Big Bang Universe.

He was also disturbed by the way that cosmology in general was practised after 1965 when the Big Bang became the standard approach, and the large number of physicists with no background in the history of the subject came in believing that this was the only approach. And of course it got worse.

A new ingredient that became popular in the 1960s was the idea that there must be large amounts of dark matter in the Universe. There has always been a good case for the existence of baryonic matter that we cannot detect except by its gravitational effect, if for no other reason than that stars of very low mass will be very faint and hard to detect, and stars of all masses will, after they have evolved, end up as dark stars. In addition, diffuse matter can be detected only indirectly. The second argument that has been widely used is that most (all, in the estimation of the standard cosmologists) of the physical systems of galaxies—binaries, small groups and large clusters—are stable systems, so that the virial argument can be applied. When this is done, for the system to be stationary almost always requires the assumption that a large amount of dark matter is present. It was first shown in the 1970s that spiral galaxies have flat rotation curves, and unless Newton’s law is modified this means that these galaxies must have massive dark halos.

This evidence all leads to the question, ‘How large a fraction of the mass-energy in the Universe is in the form of dark matter?’ By the 1980s standard cosmologists had become wedded to the view that we live in a flat Universe—an Einstein–de Sitter Universe—with the critical mass density, i.e. $\Omega = 1$.

However, the belief that the light isotopes were made in a big bang sets a limit to how much of the matter can be in baryonic form, because if $\Omega$ approaches 1 much of the deuterium would be converted to helium. And already, estimates of the dark matter content from the other techniques had led to the conclusion that the total baryonic density could only be a small percentage of the total density. This meant that if one is determined to believe in a flat Universe,
the bulk of the matter must be of non-baryonic form. Theorists first considered heavy neutrinos and then, as opinion and fashion changed, they moved to hot dark matter, and more recently, to cold dark matter.

Hoyle watched as more and more of the theorists devoted themselves to extensive attempts to understand galaxy formation using non-baryonic cold dark matter (for which there is not a scintilla of observational evidence) as an essential ingredient in the formation process, together with initial density fluctuations (for which also there is no independent evidence but that are required). And then, if necessary, a further fudge factor called—with unconscious humour, but plausible physics, ‘biasing’—was invoked to make the theory fit the observations.

A final twist made in recent years was the return of belief in a non-zero cosmological constant. It was pointed out above that the Hoyle–Bondi–Gold classical steady-state cosmology and the more recent QSSC with dust both predict that the Universe is accelerating. Einstein, in his earliest attempt to build a static model for the Universe, had to introduce the cosmological constant because he wanted to apply his theory to the Universe, which at this time was thought to be static.

The discovery, by using Type Ia supernovae as standard candles, that the Universe is accelerating (Perlmutter et al. 1999; Riess et al. 1998, 2000, 2001) is one of the most important cosmological discoveries made in recent years. However, instead of reporting this result as confirming one of the predictions of the steady-state theory and conceding that it was a piece of observational evidence in favour of a class of cosmological models favoured by Hoyle and his collaborators, what did the observers say about their results? Partly from ignorance of the history of the subject, but above all believing in the Big Bang, and that , the position has been taken that a large fraction of , perhaps 70% of it, is due to a positive cosmological constant that can be interpreted as a negative pressure. It is a vacuum energy whose density is constant. It has been named ‘quintessence’, but it is a fudge factor in this context. In a proper theory (see Hoyle & Narlikar (39)) it is a creation field.

I have gone into these arguments in some detail to convey some of the reasons why in his later years Fred Hoyle was so disturbed by the developments in modern cosmology. He felt that its practitioners had created a Church filled with false prophets, because no serious attempt is now being made to try to really understand the observations as discoveries are made. Instead, on the basis of weak evidence but strong beliefs, it is continuously asserted that we know in essence which is the correct cosmological model. If something comes along that is hard to explain, it is either ignored altogether or attributed to bad measurements. Alternatively, it is force-fitted into the current scheme, i.e. quintessence, in a situation in which the acceleration of the Universe was predicted in the original steady-state cosmology. But never is the possibility that the model itself is wrong seriously considered. Hoyle felt that this situation is more and more strongly reminiscent of the epicycles of the Greek geocentric theory. In spite of all this he remained highly optimistic, because he knew that sooner or later the observational data must ultimately prevail.

THE WORK OF HOYLE AND NARLIKAR ON ACTION AT A DISTANCE

(I am grateful to Professor J.V. Narlikar for providing much of the following material.)

Reference was made earlier to Mach’s ideas and their influence on cosmology. A talk by Hermann Bondi at the Varenna Summer School in June 1961 triggered off a new line of inves-
tigations that prompted Hoyle and Narlikar to follow up the highly interesting and original ideas that John Wheeler (ForMemRS 1995) and Richard Feynman (ForMemRS 1965) first proposed (Wheeler & Feynman 1945). Rather than beginning with Bondi’s talk, the story is best told chronologically.

Wheeler & Feynman (1945) revived the action-at-a-distance formulation of classical electrodynamics. Earlier work of Karl Schwarzschild, H. Tetrode and A.D. Fokker had shown that interaction between any two electric charges could be formally described as propagating directly with speed of light, that is, along light cones, so as to be Lorentz invariant. However, this led to reactions propagating backwards in time along the same null tracks. Instead of the usual retarded interaction of a typical charge \( a \), which we may denote by

\[
F_{\text{ret}}(a)
\]

we get the effect in a time-symmetric form:

\[
\frac{1}{2} \left[ F_{\text{ret}}(a) + F_{\text{adv}}(a) \right].
\]

The second part of the expression inside the square brackets indicates the advanced interaction from \( a \). The retarded interaction is confined to the future light cone, whereas the advanced interaction is confined to the past light cone. The latter of course conflicts with the causality principle, and this had been the main stumbling block to any effort of pushing the ‘action at a distance’ concept any further. Certainly, the well-established field concept in electrodynamics did not pose such obvious difficulties.

Wheeler and Feynman found an ingenious way out of this difficulty by demonstrating that in a static Euclidean Universe that had electric charges so distributed that any radiation emitted by a typical charge \( a \) will get absorbed in the Universe, the advanced and retarded signals from all charges interfere in such a way as to cancel all advanced signals and double all retarded ones. It is therefore clear that the effect from charge \( a \) reduces to the usual retarded one, in full conformity with causality. It also follows that the result of the above ‘response’ of the Universe is such that it generates an extra term near the charge \( a \) equal to

\[
\frac{1}{2} \left[ F_{\text{ret}}(a) - F_{\text{adv}}(a) \right].
\]

In the 1930s P.A.M. Dirac had argued somewhat heuristically that the radiation reaction on an electric charge can be calculated by evaluating the limit of the above term at the charge \( a \). This prescription worked, although in Maxwell’s field theory there was no real explanation for it. In the Wheeler–Feynman formulation the result is seen as the response of the Universe.

Although this seemed to complete the problem of classical action at a distance, Wheeler and Feynman noticed that there was still one snag. Because both the Universe and the action-at-a-distance theory were time-symmetric, the above argument could also be extended to show that, in another solution, the net effect is not the retarded one but the advanced one. To get rid of the latter unwanted solution, these authors invoked thermodynamic irreversibility; that is, arguing that considerations of asymmetrical initial conditions lead one to the retarded rather than the advanced solution.

So the matter rested until, in 1961–62, J. Hogarth, from Canada, pointed out that the real cause of asymmetry lay not in thermodynamics but in cosmology (Hogarth 1962). Had Wheeler and Feynman considered an expanding Universe, they would have noticed that the advanced and retarded solutions cannot be treated on equal footing in the computation of the response of the Universe. Hogarth proceeded to show that the correct response (retarded
solutions) is given in a Universe that absorbs all retarded signals but not the advanced ones, whereas the wrong response is given by a Universe that absorbs all advanced signals but not the retarded ones. He found that the steady-state Universe gives the correct response, whereas most Big Bang models give the wrong response.

It was Hogarth’s work that Hermann Bondi reported at the Varenna summer school. Unlike the observational uncertainties, this work gave an elegant and clear-cut way of choosing the correct model—provided, of course, that one could argue that the action-at-a-distance approach could work as well as the field theory approach, not only classically but also in the quantum framework.

Hoyle and Narlikar took it on themselves to follow up on this matter. They first ironed out some technical problems with Hogarth’s result, and then went towards the more difficult task of showing the quantum-compatibility of action at a distance. During the decade 1961–71 they completed this work to the extent of showing that all results of standard field theoretical quantum electrodynamics can be derived by the action at a distance method. After writing a series of papers they wrote a book containing their results (45). In 1995, in an article written to commemorate 50 years of the Wheeler–Feynman paper in the same journal (Reviews of Modern Physics), Hoyle and Narlikar (55) took the work one step further by showing that the so-called infinities associated with the renormalization programme can be completely eliminated by the action at a distance, because in this approach there is no infinite self-action associated with field theory, but a finite response of the Universe.

Jayant Narlikar recalls that the success of this approach and its ability to discard all Big Bang models in favour of the steady-state model was a powerful argument in support of Hoyle’s conviction that, despite the apparent successes of the Big Bang model, it must be wrong.

A logical follow-up of action-at-a-distance electrodynamics led Hoyle and Narlikar to apply the same ideas with suitable modification to inertia and gravitation. This led them during 1964–66 to the conformal theory of gravitation, which reduces to general relativity for most practical purposes but is perfectly compatible with Mach’s principle (32–35, 38, 40). Some three decades later this theory was further extended to include the creation of matter, leading to the QSSC.
strenuously objected, led him to write to the Vice-Chancellor in September 1964 resigning from the Plumian Professorship.

The Vice-Chancellor asked Fred to leave his resignation in abeyance, because he found that there was much dismay among members of the Council of the Senate about his proposed move. Both Dr (later Dame) Mary Cartwright FRS, Mistress of Girton College, and Professor Alan (later Sir Alan) Cottrell FRS, sometime Master of Jesus College, talked at length to Fred, and finally he agreed to rescind his resignation if an Institute could be set up in Cambridge.

The financial problems were then solved by Todd and Sir John Cockcroft FRS, Master of Churchill College, who worked out with the Vice-Chancellor an arrangement by which funds totalling about £750000 would be provided in equal parts by the Wolfson Foundation (of which Cockcroft was a member of the governing board), the Nuffield Foundation (of which Todd was the chairman) and the (then recently formed) Science Research Council (SRC). The university would contribute the land.

Hoyle then had to go through the complicated and unaccustomed (for him) business of writing proposals for these funds—to the Nuffield Foundation for salaries, to the Wolfson Foundation for the building and to the SRC for the computer. He also had extensive discussions about the site, finally obtaining a place to build the Institute along the Madingley Road at Madingley Rise adjacent to the Cambridge Observatories (for details see Hoyle’s autobiography (A2 in Additional references), pp. 342–345).

This was not the end of the negotiations, however, because the SRC, although accepting the proposal for funds, first argued that the Institute could be anywhere, as long as it was not in Cambridge at all. In view of what had gone before, this was of course ridiculous. After further discussion, by 1966 the SRC rescinded this veto on the understanding that the Institute in Cambridge be placed under its own aegis (rather that of a Faculty) with a management committee on which Todd and Cockcroft would both sit, with Todd as Chairman. The university would pay 20% of the cost of running the Institute, which would consist of rates on the building, fuel and electricity bills, and some secretarial assistance. In July 1966 the Senate at Cambridge passed a Grace founding the Institute of Theoretical Astronomy. Building started on 1 August 1966 and was completed on 1 August 1967.

As a research establishment the Institute was an instant success. Already before the building had been completed, in the summers of 1966 and 1967, several of Hoyle’s collaborators from abroad spent the two or three months in Cambridge using a small amount of space that was made available by Professor R.O. Redman FRS at the Cambridge Observatories, of which he was the Director. They included William A. Fowler, Geoffrey and Margaret Burbidge, D. Clayton and R. Wagoner, mostly working on nuclear astrophysics.

In 1967 Hoyle began to appoint staff and soon built up the Institute to its full complement. In the space of one or two years it became known as one of the most important centres in the world for theoretical astronomy. The requests for short visits became overwhelming. In the first five years, when Hoyle was the Director, a significant fraction of the younger generation of theoretical astrophysicists from the UK and the rest of Europe, and from the USA and further afield, spent time at the Institute. It rapidly became known as one of the great international centres of research in astrophysics.

The original period of the research grants under which the Institute was funded was 1 August 1967 to 31 July 1972. Thus a new funding proposal had to be put in late in 1971–72. This timing was unfortunate because Redman was due to retire in 1972, and in its wisdom the General Board of the university proposed to shut down the Observatories on Redman’s retire-
ment unless the Institute and the Observatories could be amalgamated. The proposal was complicated by the fact that much would depend on who was elected as successor to Redman as Professor of Astrophysics.

Thus the submission of a new funding proposal was delayed until the decisions on both the joint budget and Redman’s successor became clear. Hoyle was opposed to the amalgamation of the two institutions because he felt that the new budget would not be adequate. At the same time he was very anxious to get as a successor to Redman an astronomer with very considerable experience with the large optical telescopes in the USA and elsewhere. However, on the Board of Electors (of which Hoyle was one) there were several individuals who had a very different view, and politics entered very strongly into the final decision. Hoyle’s candidate was not chosen, and it was over this issue (and the planned amalgamation) that he finally resigned his Plumian Professorship and left Cambridge for good in September 1972.

As a postscript to this period, it should be added that the amalgamation went through in 1972–73 and both groups, now renamed The Institute of Astronomy, were forced to run on a much reduced budget, as Hoyle had anticipated. It meant that the relativity group of Brandon Carter (FRS 1981) and Stephen Hawking (FRS 1974) moved to the Department of Applied Mathematics and Theoretical Physics. However, the Institute very soon regained its strength and has thrived over the past 30 years. Its creation and Hoyle’s direction of it for the first five years were clearly one of his greatest contributions to the progress of astronomy.

FRED HOYLE AND THE ANGLO-AUSTRALIAN TELESCOPE PROJECT

After many years of negotiation, in 1967 the British Government and the Australian Government agreed to join forces to build and operate a large optical telescope in Australia. The SRC in Britain and the Department of Education in Australia set up a Joint Policy Committee to oversee this project. This committee ultimately evolved into the Anglo-Australian Telescope (AAT) Board, with three members appointed from each country. From the UK the original members were Sir Richard Woolley FRS, Hermann Bondi and, from the SRC, Mr J. Hosie. Almost immediately after the first meeting, Bondi resigned because he had accepted a position with the European Space Agency, and Fred Hoyle was appointed in his place. After Woolley retired as Director of the Royal Greenwich Observatory, he was replaced by the incoming Director, Professor Margaret Burbidge.

From his appointment to the Joint Policy Committee until he retired from the Board in 1974, Hoyle took a major part in all of the initial decisions and the highly complicated technical and political decisions that had to be made by the Board. In the first stages, the chairman was the senior Australian board member Dr E.G. Bowen (FRS 1975), but after he was replaced, Hoyle became chairman. He presided at the dedication of the Anglo-Australian Telescope by the Prince of Wales in 1974.

A detailed account of all of this is given in The history of the AAT by S.C. Ben Gascoigne, Katrina M. Proust and Malcolm O. Robins (Cambridge University Press, 1990) and in a short monograph written by Hoyle (The Anglo-Australian Telescope, Cardiff University Press, 1975).

It is clear that both as a member and as the chairman of the AAT Board, Hoyle had a crucial role, not only in supporting the correct technical approach but also in the very difficult politics. The political issues were concerned with the battle that was fought and won concerning the view that the Board must be an independent entity with its members making final decisions
concerning the way that the facility would be operated, and the appointment of a first Director, Dr E.J. Wampler, who was an experienced and highly distinguished astronomer from the USA. There was much opposition to this, particularly from the Director of the Mount Stromlo Observatory and his supporters in the Australian administration, but the long-term success of the AAT is in part due to the correct decisions that were made at the beginning.

**Fred Hoyle as a Writer and Science Popularizer**

Throughout his professional career Fred Hoyle was a prolific author of research monographs, textbooks on astronomy, popular astronomy, on science in general, on sociological and political topics, and on science fiction. In what follows I summarize these works; a full list is given at the end of the memoir.


In 1949 Hoyle gave a series of BBC broadcast lectures entitled ‘The nature of the Universe’. They were extremely popular and through them he became widely known as a leading communicator of science. They were published in book form in 1950, with further editions in 1952 and 1960. His second popular book was *Frontiers of astronomy*.


After his original work on interstellar dust starting in the 1960s, Hoyle (with Wickramasinghe) published a long series of books and pamphlets, all of which stemmed from their realization that interstellar dust contains a component involving organic molecules that is bound to interact with the diffuse matter in the Solar System. They developed the idea that through comets, and dust trapped in the Solar System, extraterrestrial life forms could reach the upper atmosphere and the Earth. Books in this category include *Life cloud* (Dent, 1978), *Diseases from space* (Dent, 1979), *Cosmic life force* (Dent, 1988) and *Life on Mars?* (Clinical Press, Redland, 1997).

Hoyle also became interested in the idea that Stonehenge could be interpreted as an ancient circle built with astronomical application (*On Stonehenge*, 1977). In a completely different field he and Wickramasinghe were much criticized, probably correctly so, for their attempt to explain ‘Archaeopteryx, the primordial bird’ (Swansea, Christopher Davis, 1986). Like most great scientists Fred was sometimes wrong, because he did not really understand the data or misinterpreted it. However, he did not deserve the savage criticism that was heaped on him on some occasions for his forays into biology.

Fred was also a prolific author of science fiction. His first novel, *The black cloud* (Heinemann, 1957) was probably his best. This was succeeded by *Ossian’s ride* (Heinemann, 1959), *October the first is too late* (Heinemann, 1966), *Element 79* (New American Library, 1967), and *Fifth planet* (with Geoffrey Hoyle; Heinemann, 1964). Also with his son Geoffrey he wrote a series of science fiction novels for children.

They also wrote a science fiction play for children entitled *Rockets in Ursa Major*, which played to packed audiences at the Mermaid Theatre in London during the Easter vacation in 1962. Fred became a close friend of the well-known stage and screen actor Sir Bernard (later
Lord) Miles, who together with his wife owned and was the actor–manager of the Mermaid Theatre for many years.

Fred Hoyle and John Elliot also wrote a science fiction serial for the BBC entitled *A for Andromeda* and its sequel *Andromeda breakthrough*. *A for Andromeda* aired from 3 October to 14 November 1961 and was a great TV success. In choosing the cast for *A for Andromeda* Hoyle and Elliot chose Julie Christie, who played the part of Christine, the computer programmer who dies and is absorbed by the computer. According to John Elliot it was Fred who saw in her the cool, androgynous humanoid as he conceived the part, and immediately spotted her at an end-of-year performance of students at the Royal Academy of Dramatic Art. This serial made her very popular. She was already unavailable for the sequel, and within four years she had won an Academy Award in Hollywood. Both serials were published in book form in 1962 and 1964.

Finally, mention should be made of Hoyle’s work with the American composer Leo Smit. He wrote the lyrics for several pieces, including a piece entitled *Copernicus: four-part chorus of mixed voices, narrator, chorus, and instrumental ensemble, Copernicus, Desto*. He and Smit also wrote an opera, Hoyle writing the libretto and Smit the music. It was called *The alchemy of love*.

**FAMILY AND LEISURE ACTIVITIES**

Fred and Barbara Hoyle had an exceedingly close and happy marriage. They had two children, Geoffrey and Elizabeth.

From his early days in Yorkshire Fred was always happiest when he was walking on the moors and on the hills and dales, first in Yorkshire and the Pennines and later in Scotland. He knew many of the well-known climbers from the North Country whom he had met often on his walks and in the pubs.

After he left Cambridge in 1972 he and Barbara moved to a house in the Lake District where they lived for many years. In 1990 they moved back south to Bournemouth.

**APPOINTMENTS, HONOURS AND AWARDS**

*Appointments*

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<td>University Lecturer in Mathematics, Cambridge University</td>
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<td>Plumian Professor of Astronomy and Experimental Philosophy, Cambridge University</td>
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<td>1969–72</td>
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*Honours*

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<td>1972–2001</td>
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<tr>
<td>1975–2001</td>
<td>Honorary Research Professor, University College Cardiff</td>
</tr>
<tr>
<td>1957</td>
<td>Elected Fellow of The Royal Society</td>
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1969 Associate, National Academy of Sciences, USA
1964 Member, American Academy of Arts and Sciences
1971–73 President, Royal Astronomical Society
1972 Knighted
1977 Fellow, Royal Irish Academy
1980 Member, American Philosophical Society

Awards

1967 Kalinga Prize, UNESCO
1968 Gold Medal, Royal Astronomical Society
1969 Jansky Prize, National Radio Astronomy Observatory
1970 Bruce Medal, Astronomical Society of the Pacific
1974 Royal Medal, The Royal Society
1986 Association pour le Développement de l’Observatoire de Nice ADION Medal
1992 Karl Schwarzschild Medal, Astronomische Gesellschaft
1994 Balzan Prize (jointly with M. Schwarzschild) International Balzan Foundation
1997 Crafoord Prize, Swedish Academy of Sciences

ACKNOWLEDGEMENTS

I am indebted to many friends and colleagues for help in preparing this memoir. They include Lady Barbara Hoyle, Mr Geoffrey Hoyle and Dr Elizabeth Butler, Sir Herman Bondi FRS, Professor Margaret Burbidge FRS, Sir Alan Cottrell FRS, Professor C. Domb FRS, Professor John Faulkner, Professor T. Gold FRS, Professor L. Mestel FRS, Professor J.V. Narlikar, Professor P. Solomon and Professor N.C. Wickramasinghe. I am considerably indebted to Margaret Burbidge, Kate Ericson and Shirley Roy for much help in La Jolla in preparing this memoir.

The frontispiece photograph was taken ca. 1984 by Geoffrey Hoyle.

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